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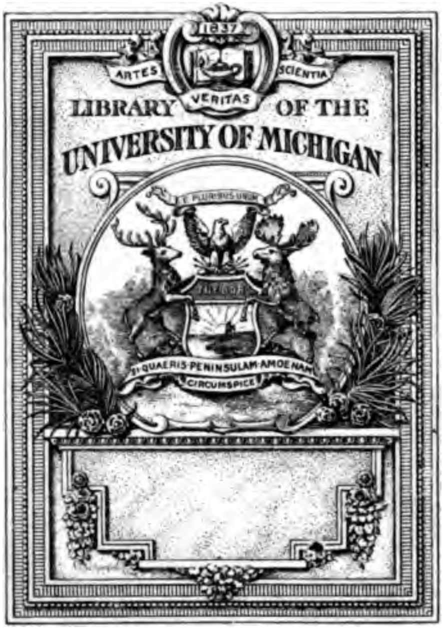

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CONGRESS OF ARTS AND SCIENCE

UNIVERSAL EXPOSITION ST. LOUIS 1904

IN EIGHT VOLUMES

VOLUME IV

CONGRESS OF ARTS AND SCIENCE

UNIVERSAL EXPOSITION, ST. LOUIS, 1904

EDITED BY

HOWARD J. ROGERS, A.M., LL.D.

DIRECTOR OF CONGRESSES

VOLUME IV

PHYSICS
CHEMISTRY
ASTRONOMY
SCIENCES OF THE EARTH

BOSTON AND NEW YORK
HOUGHTON, MIFFLIN AND COMPANY
The Riverside Press, Cambridge
1906

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Published June 1906

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DIVISION C—PHYSICAL SCIENCE

DIVISION C — PHYSICAL SCIENCE

(*Hall 4, September 20, 10 a. m.*)

SPEAKER: PROFESSOR ROBERT S. WOODWARD, Columbia University.

THE UNITY OF PHYSICAL SCIENCE

BY ROBERT SIMPSON WOODWARD

[**Robert Simpson Woodward,** Ph.D., Sc.D., LL.D., President of the Carnegie Institution of Washington. **b.** Rochester, Mich., 1849. C.E. University of Michigan, 1872; Ph.D. University of Michigan, 1892; Honorary LL.D. University of Wisconsin, 1904; Sc.D., University of Pennsylvania, and Columbia University, 1905. Assistant engineer, U. S. Lake Survey, 1872–82; assistant astronomer, U. S. Transit of Venus Commission, 1882–84; astronomer, geographer, and chief geographer, U. S. Geological Survey, 1884–90; assistant, U. S. Coast and Geodetic Survey, 1890–93; Professor of Mechanics and Mathematical Physics, Columbia University, 1893–1905; Dean of School of Pure Science, *ibid.*, 1895–1905; President of Carnegie Institution of Washington, 1905. Member of National Academy of Sciences; Past President and Treasurer (since 1894) of American Association for the Advancement of Science; Past President of American Mathematical Society and of New York Academy of Sciences; member of Astronomical and Astrophysical Society of America, Geological Society of America, Physical Society of America, and Washington Academy of Sciences. **Author of** *Smithsonian Geographical Tables*; *Higher Mathematics* (with Mansfield Merriman); also of many Government reports and numerous papers and addresses on subjects in astronomy, geodesy, mathematics, mathematical physics, and education.]

THERE is a tradition, still tacitly sanctioned even by men of science, that there have been epochs when the more eminent minds were able to compass the entire range of knowledge. Amongst the vanishing heroic figures of the past it seems possible, indeed, to discern, here and there, a Galileo, a Huygens, a Descartes, a Leibnitz, a Newton, a Laplace, or a Humboldt, each capable, at least, of summing up with great completeness the state of contemporary knowledge. Traditions, however, are generally more or less mythical, and the myth in this case seems to be in flat contradiction with the fact that there never was such an epoch, that the great masters of our distinguished predecessors were, after all, much like the masters of to-day, simply the leading specialists of their times. But however this may be, if we grant the possibility of the requisite attainments, even in a few individuals at any epoch, we shall speedily conclude that there never was an epoch so much in need of them as the immediate present, when the divisional speakers of this Congress are called upon to explain the unities which pervade the ever-widening and largely diverse fields of their several domains.

The domain of physical science, concerning which I have the honor to address you to-day, presents peculiar and peculiarly formidable difficulties in the way of a summary review. While we may not be disposed to limit the wide range of inclusion specified by our programme, we must at once disclaim any attempt to speak authoritatively with respect to most of its details. There is, in fact, such a vast array of knowledge now comprehended under any one of the six Departments of our Division, that the boldest author must hesitate to enter on a limited discussion with respect to any of them. But if it is thus difficult to consider any department of physical science, it appears incomparably more difficult to contemplate all of them in the bewildering complexity of their interrelations and in the bewildering diversity of their subject-matter. What, for example, could seem more appalling to the average man of science than the duty of explaining the connections of archeology and astrophysics, or those of ecology and electrons?

Happily, however, the managers of the Congress have provided an adequate division of labor, whereby the technical details of the various Departments are allotted to experts, giving thus to a divisional speaker a degree of freedom with respect to depth in some way commensurate with the breadth of his task. Presuming, therefore, that I may deal only with the broader outlines and salient features of the subject, I invite your attention to a summary view of the present status and the apparent trend of physical science.

Whatever may be affirmed with respect to science in general, there appears to be no doubt that all of the physical sciences are characterized by three remarkable unities, — a unity of origin, a unity of growth, and a unity of purpose. Physical science originates in observation and experiment; it rises from the fact-gathering stage of unrelated qualities to the higher plane of related quantities, and passes thence on to the realm of correlation, computation, and prediction under theory; and its purpose is to interpret in consistent and verifiable terms the universe, of which we form a part. The recognition of these unities is of prime importance; for it helps us to understand and to anticipate a great diversity of perfection amongst the different branches of science, and hence leads us to appreciate the desirability of hearty coöperation on the part of scientific workers in order that progress may be ever positive towards the common goal.

Glancing rapidly *seriatim* at the different departments of physical science as specified by our programme, we come first to a consideration of formal physics, and we may most quickly orient ourselves aright in this department by trying to state in what respects the physics of to-day differs from the physics of a hundred years ago.

In spite of the extraordinary perfection of the work of Lagrange, Laplace, Fourier, Young, Fresnel, Poisson, Green, Gauss, and others of the early part of the nineteenth century, it will be at once admitted that great progress has been made. In addition to noteworthy advances and improvements along the lines laid down by these masters, there have been developed the relatively new fields of elasticity, electromagnetics, thermodynamics, and astrophysics; and there has been discovered the widest of all generalizations in physical science, — the law of conservation of energy. Whereas it was easy a century ago to conceive, as in gravitational astronomy, of action at a distance across empty space, the universe in the mean time has come to appear more and more plethoric not only with "gross matter," but with that most wonderful entity we call the ether. The astronomers have shown us, in fact, that the number of molar systems in the universe is enormously greater than was supposed possible a century ago; while the physicists have revealed to us molecular systems rivaling our solar system and its Jovian and Saturnian subsystems, and they have loaded down the ether with a burden of properties and relationships which its usual tenuity seems scarcely fitted to bear. Whereas, also, a century ago the tendency of thought, under the stimulus of the remarkable developments of the elastic solid theory of light and the fluid theories of electricity, was chiefly towards an ether whose continuity would have pleased Anaxagoras, the tendency to-day is chiefly towards an ether whose atomicity would have pleased Democritus.

On the whole, it must be said that the advances of the past century, and especially those of the past half-century, have been mainly along the lines of molecular physics. The epoch of Laplace was distinctly an epoch of molar physics; the epoch of to-day is distinctly an epoch of molecular physics. Light, heat, electricity, and magnetism have been definitely correlated as molecular and ethereal phenomena; while the recently discovered X-rays and the wonders of radioactivity, along with the "electrons," the "corpuscles" and the "electrions" of current investigations, all point towards a molecular constitution of the ether. Thermodynamics, likewise, large as it has grown in recent decades, is essentially a development of the molecular theory of gases. It would be too bold, perhaps, to assert that the trend of accumulating knowledge is towards an atomic unity of matter, but the day seems not far distant when there will be room for a new *Principia* and for a treatise which will accomplish for molecular systems what the *Mécanique Céleste* accomplished for the solar system.

One of the most important advances of recent decades is found in the fixation of ideas with respect to the units of physical science, and in the great improvements which have been wrought in metrology by the "International Bureau of Weights and Measures." Our standards of length, mass, and time are now fixed with a degree of precision which leaves little to be desired for the present; and the capital resources of measurement and calculation are now available to an extent never hitherto approached.

It should be noted, however, that confidence in the stability of our standards is by no means comparable with the perfection of their current applications. Indeed, we may raise with respect to them the question so long mooted with regard to the motions of the members of the solar system: namely, are they stable? Notwithstanding the admirable precision of the intercomparisons of the prototype meters and prototype kilograms and the equally admirable precision of Professor Michelson's determination of the length of the meter in terms of wave-lengths of cadmium light, we cannot affirm that these observed relations will hold indefinitely. Our inherited notions of mass have been rather rudely shaken, also, by the penetrating criticisms of Mach, and it appears possible even that the law of conservation of mass may need modification in the light of pending researches. But worst of all, our time-unit, the sidereal day, is so far from possessing the element of constancy that we may affirm with practical certainty that it is secularly variable. Having realized, through Professor Michelson's superb determination just referred to, the cosmic standard of length suggested by Maxwell thirty years ago, we are now much more in need of an equally trustworthy cosmic standard of time.

If the progress of physics during the past century has been chiefly in the direction of atomic theory, the progress of chemistry has been still more so. Chemistry is, in fact, the science of atoms and molecules *par excellence*, a distinction it has maintained for well-nigh a full century under the dominance of the fruitful atomic and molecular hypotheses of Dalton and of Avogadro and Ampère, and under the similarly fruitful laws of gases established by Dalton and Gay-Lussac. Perhaps the most striking feature of this progress, in a general way, is the gradual disappearance it has entailed of the imaginary lines which have been long thought to separate the fields of chemistry and physics. Through the remarkable discoveries of Faraday the two fields have been found to overlap in actual electrical contact. Through the wonderful revelations of spectrum analysis, originating with Bunsen and Kirchhoff, they have been proved to be very largely common ground. And through the broader generalizations inaugurated by Willard Gibbs, Helmholtz, and others, they are now both somewhat in danger of being annexed as a sub-province of rational mechanics.

To one whose work has fallen more especially in the fields of precise astronomy, geodesy, or metrology, it might seem a just reproach to chemistry that it is a science whose measurements and calculations demand, as a rule, no greater arithmetical resources than those of four-place tables of logarithms and anti-logarithms. The so-called "Constants of Nature" supplied by chemistry are, in fact, known with a low degree of certainty; a degree expressed, say, by three to five significant figures. A small amount of reflection, however, will convince one that the phenomena with which the chemist has to deal are usually far more complex than those which have yielded the splendid precision of astronomy, geodesy, and metrology. Moreover, it should be observed that the certainties even of these highly perfected sciences are very unequal in their different branches. It appears more correct, therefore, as well as more just, considering the central position it occupies and the wide range of its ramifications, along with the vast aggregate of qualitative and quantitative knowledge it has massed, to assert that the precision of chemistry affords the best numerical index of the present state of physical science. That is, when reduced to the most compact form of statement, the certainties of physical science are best indicated, in a general way, by a table of the combining weights of the eighty-odd chemical elements.

When one contemplates the numbers of such a table, and when one adds to its suggestions those which flow from the various periodic groupings of the same numbers, he can hardly avoid being inspired by the day-dreams of those who have looked long for the atomic unity of matter. But however the grand problem which thus obtrudes itself may be resolved finally, it appears certain that this table must stand as one of the great landmarks along the path of progress in physical science.

It was justly remarked by Laplace in his *Système du Monde* that "L'Astronomie, par la dignité de son objet et par la perfection de ses théories, est le plus beau monument de l'esprit humain, le titre le plus noble de son intelligence"; and we must all admit that subsequent progress has gone far to maintain this high position for the most ancient and interesting of the older sciences. One finds little difficulty in accounting for the early rise of astronomical science and for the universal interest in celestial phenomena. Their immanence and omnipresence appeal even to the dullest intellects. But it is not so easy to account for the remarkable fact that although astronomy deals chiefly with the relations of bodies separated by immense distances, progress in its development has thus far been at least equal to, if not in advance of, the progress of physics and chemistry, which have to deal with matter close at hand. Without attempting a full explanation of this fact, it may suffice to observe that the principal phenomena of astronomy thus far developed appear to be relatively simple in comparison with those of the other physical sciences; and that the immense distances which separate the celestial bodies, instead of being an obstacle to, are a fortunate circumstance directly in favor of, the triumphant advances which have distinguished astronomical science from the epoch of Galileo down to the present day.

Not less noteworthy than his high estimate of the position of astronomy in his time are Laplace's anticipations of the course of future progress. Our admiration is kindled by the clearness of his vision with respect to ways and means, and by the penetration of his predictions of future discoveries. Advances in sidereal astronomy, he rightly thought, would depend chiefly on improvements in telescopes; while advances in dynamical astronomy were to come along with increased precision in the observed places of the members of the solar system and along with the growing perfection of analysis. It is almost needless to say that Laplace's brilliant anticipations have been quite surpassed by the actual developments. Observational astronomy has become one of the most delicately perfect of all the sciences; dynamical astronomy easily outstrips all competitors in the perfection of its theories and in the certainty of its predictions; while the newly developed branch of astrophysics supplies the last link in the chain of evidence of the essential unity of the material universe.

The order of the dimensions and the order of the mass contents of the visible universe, at any rate, have been pretty clearly made out. In addition to the vast aggregate of direct observational evidence collected and recorded during the past century, numerous theoretical researches have gone far, also, to interpret the laws which reign in the apparent chaos of the stars. The solar system, with its magnificent subsystems, has been proved to exhibit the type of stellar systems in general.

In a profound investigation recently published, Lord Kelvin has sought to correlate under the law of gravitation the principal observed data of the visible universe. Assuming this universe to lie within a sphere of radius equal to the distance of a star whose parallax is one thousandth of a second of arc, he concludes that there must be something like a thousand million masses of the magnitude of our sun within that sphere. Light traveling at the rate of 300,000 kilometers per second would require about six thousand years to traverse the diameter of this universe, and while the average distance asunder of the visible stars is considerably less, it is still of the same order. It is only essential, therefore, to imagine our luminary surrounded by a thousand million such suns, most of which are, in all probability, attended by groups of planets, to get some idea of the quantity of matter within visual range of our relatively insignificant terrestrial abode. And the imposing range of the astronomer's time-scale is perhaps impressively brought home to us when we reflect that a million years is the smallest convenient unit for recording the life-history of a star, while the current events in that history are transmitted across the interstellar medium by vibrations which occur at the rate of about six hundred million million times per second. Measured by its accumulation of achievements, then, the astronomy of to-day fulfills the requirements of a highly developed science. It is characterized by a vast aggregate of accurately determined facts related by theories founded on a small number of hypotheses. In the past it has called forth the two greatest of all systematic treatises, the *Principia* of Newton and the *Mécanique Céleste* of Laplace. It has probably done more also than any other science, up to the present time, to illuminate the dark periods during which man has floundered in his struggle for advancement; and the indications are that its prestige will long continue.

But there are spots on every sun; and lest some may infer, even humorously, as Carlyle did seventy-odd years ago, that our system of the world is "as good as perfect," attention should be called to some noteworthy defects in astronomical data and to some singular obscurities in astronomical theory. Here, however, great caution and brevity are essential to avoid poaching on the preserves of our colleagues of the Sections. It may suffice, therefore, merely to mention, under the head of defective data, the low precision of the solar parallax, the aberration constant, the masses of the members of the solar system, and the uncertainty of our time-unit, already referred to. Two instances, likewise, which belong to the general field of physics as well, may suffice as illustrations of obscurities in astronomical theory. Stated in the order of their apparent complexity, these obscurities refer to the law of gravitation and to the phenomenon of stellar aberration. Probably both are related, and one may hope that any explanation of either will throw light on the other.

So long as no attempt is made to reconcile the law of gravitation with other branches of physics, progress, up to a certain point, is easy; and probably great advantage has resulted from the fact that dynamical astronomers have not been seriously disturbed by a desire to harmonize this law with the more elementary laws of mechanics. Perhaps they have unconsciously rested on the platform that gravitation is one of the "primordial causes" which are impenetrable to us. There are some indications that even Laplace and Fourier did so rest. However this may be, it has grown steadily more and more imperative during the past century to explain gravitation, or to discover the mechanism which provides that the force between two widely separated masses is proportional to their product directly and to the square of the distance between them inversely. All evidence seems to indicate that the ether must provide this mechanism; but, strangely enough, so far, the ether has baffled all attempts to reveal the secret. The problem has been attacked also on the purely observational side of the numerical value of the gravitation constant. But the splendid experimental researches for this purpose throw no light on the mechanism in question, and, unfortunately, they bring out values for the constant of a low order of precision.

With regard to stellar aberration, it must be at once admitted that we have neither an adequate theory nor a precisely determined fact. The astronomer has generally contented himself with the elementary view that aberration is a purely kinematical phenomenon; that the earth not only slips through the ether without sensible retardation, but that the ether slips through the earth without sensible effects. This difficulty was recognized, in a way, by Young and Fresnel, and, although the subject of elaborate investigation in recent decades, it has proved equally baffling with Newtonian gravitation. As in the case of the latter also, the numerous attempts made to determine the constant of aberration by observational methods have been rewarded by results of only meagre precision. Possibly the time has arrived when one may raise the question, Within what limits is it proper to speak of a gravitation constant or of an aberration constant?

If we agree with Laplace that astronomy is entitled to the highest rank among the physical sciences, we can accord nothing short of second place to the sciences of the earth. Most of them are, indeed, intimately related to astronomy; and some of them are scarcely less ancient in their origins, less dignified in their objects, or less perfect in their theories. Primarily, also, it should be observed, geophysics is not simply a part of, but is the very foundation of, astronomy; for the earth furnishes the orientation, the base-line, and the timepiece by means of which the astronomer explores the heavens. Geology, likewise, in the broader sense of the term, as we are now coming to see, is a fundamental science not only by reason of its interpretations of terrestrial phenomena, but also by reason of its parallel interpretations of celestial phenomena; for there is little doubt that in the evolution of the earth we may read a history which is in large degree typical of the history of celestial bodies. In any revised estimate, therefore, of the relative rank of the physical sciences, while it would be impossible to lower the science of the heavens, it would appear essential to raise the sciences of the earth to a much higher plane of importance than was thought appropriate by our predecessors of a hundred years ago.

As with physics, chemistry, and astronomy, the wonderful progress of the nineteenth century in geophysical science has been along lines converging towards the more recondite properties of matter. All parts of the earth, through observation, experiment, induction, and deduction, have yielded increasing evidence of limited unities amid endless diversities. Adopting the convenient terminology of geologists for the different shells of the earth, let us glance rapidly in turn at the sciences of the atmosphere, the hydrosphere or oceans, the lithosphere or crust, and the centrosphere or nucleus.

The atmosphere is the special province of meteorologists, and although they are not yet able to issue long-range predictions, like those guaranteed by our theories of tides and terrestrial magnetism, it must be admitted that they have made great progress towards a rational description of the apparently erratic phenomena of the weather. One of the peculiar anomalies of this science illustrates in a striking way the general need of additional knowledge of the properties of matter; in this case, especially, the properties of gases. It is the fact that in meteorology greater progress has been made, up to date, in the interpretation of the kinetic than in the interpretation of the static phenomena of the atmosphere. Considering that static properties are usually much simpler than kinetic properties, it seems strange that we should know much more about cyclones, for example, than we do about the mass and the mass distribution of the atmosphere. In respect to this apparently simple question meteorology seems to have made no advance beyond the work of Laplace. There are indications, however, that this, along with many other questions, must await the advent of a new *Principia.*

The geodesists, who are the closest allies of the astronomers, may be said to preside over the hydrosphere, since most of their theories as well as most of their observations are referred to the sea level. They have determined the shape and the size of the earth to a surprising degree of certainty; but they are now confronted by problems which depend chiefly on the mass and mass distribution of the earth. The exquisite refinement of their observational methods has brought to light a minute wandering in the earth of its axis of rotation, which makes the latitude of any place a variable quantity; but the interpretation of this phenomenon is again a physical and not a mensurational problem. They have worked improvements also in all kinds of apparatus for refined measurements, as of base-lines, angles, and differences of level; but here, likewise, they appear to approach limits set by the properties of matter.

The lithosphere was once thought to be the restricted province of geologists, but they now lay claim to the entire earth, from the centre of the centrosphere to the limits of the atmosphere, and they threaten to invade the region of the astronomers on their way toward the outlying domain of cosmogony. Geology illustrates better than any other science, probably, the wide ramifications and the close interrelations of physical phenomena. There is scarcely a process, a product, or a principle in the whole range of physical science, from physics and chemistry up to astronomy and astrophysics, which is not fully illustrated in its uniqueness or in its diversity by actual operations still in progress on the earth, or by actual records preserved in her crust. The earth is thus at once the grandest of laboratories and the grandest of museums available to man.

Any summary statement, from a non-professional student, of the advances in geology during the past century, would be hopelessly inadequate. Such a task could be fitly undertaken only by an expert, or by a corps of them. But out of the impressive array of achievements of this science, two seem to be especially worthy of general attention. They are the essential determination of the properties and the rôle of the lithosphere, and the essential determination of the time-scale suitable for measuring the historical succession of terrestrial events. The lithosphere is the theatre of the principal activities, mechanical and biological, of our planet; and a million years is the smallest convenient unit for recording the march of those activities. When one considers the intellectual as well as the physical obstacles which had to be surmounted, and when one recalls the bitter controversies between the Neptunists and the Vulcanists and between the Catastrophists and the Uniformitarians, these achievements are seen to be amongst the most important in the annals of science.

The centrosphere is the *terra incognita* whose boundaries only are accessible to physical science. It is that part of the earth concerning which astronomers, geologists, and physicists have written much, but concerning which, alas! we are still in doubt. Where direct observation is unattainable, speculation is generally easy, but the exclusion of inappropriate hypotheses is, in such cases, generally difficult. Nevertheless, it may be affirmed that the range of possibilities for the state of the centrosphere has been sharply restricted during the past half-century. Whatever may have been the origin of our planet, whether it has evolved from nebular condensation or from meteoric accretion; and whatever may be the distribution of temperature within the earth's mass as a whole; it appears certain that pressure is the dominant factor within the nucleus. Pressure from above, supplied in hydrostatic measure by the plastic lithosphere, supplemented by internal pressure below, must determine, it would seem, within narrow limits, the actual distribution of density throughout the centrosphere, regardless of its material composition, of its effective rigidity, or of its potential liquidity. Here, however, we are extending the known properties of matter quite beyond the bounds of experience, or of present possible experiment; and we are again reminded of the unity of our needs by the diversity of our difficulties.

In his recently published autobiography, Herbert Spencer asserts that at the time of issue of his work on biology (1864) "not one person in ten or more knew the meaning of the word . . . and among those who knew it, few cared to know anything about the subject." That the attitude of the educated public towards biological science could have been thus indifferent, if not inimical, forty years ago, seems strange enough now even to those of us who have witnessed in part the scientific progress subsequent to that epoch. But this was a memorable epoch, marked by the advent of the great intellectual awakening ushered in by the generalizations of Darwin, Wallace, Spencer, and their coadjutors. And the quarter of a century which immediately followed this epoch appears, as we look back upon it, like an heroic age of scientific achievement. It was an age during which some men of science, and more men not of science, lost their heads temporarily, if not permanently; but it was also an age during which most men of science, and thinking people in general, moved forward at a rate quite without precedent in the history of human advancement. A new, and a greatly enlarged, view of the universe was introduced in the doctrine of evolution, advanced and opposed, alike vigorously, chiefly by reason of its biological applications and implications. Galileo, Newton, and Laplace had given us a system of the inorganic world; Darwin, Spencer, and their followers have foreshadowed a system which includes the organic world as well.

The astonishing progress of biology in recent times furnishes the most convincing evidence of the unity and the efficiency of the methods of physical science in the interpretation of natural phenomena. For the biologist has followed the same methods, with changes appropriate to his subject-matter only, as those found fruitful in astronomy, chemistry, and all the rest. And whatever may be the increased complexity of the organic over the inorganic world, or however high the factor of life may seem to raise the problems of biology above the plane of the other physical sciences, there has appeared no sufficient reason, as yet, to doubt either the validity or the adequacy of those methods.

Moreover, the interrelations of biology with chemistry and physics especially are yearly growing more and more extended and intimate through the rapidly expanding researches of bacteriology, physiology, and physiological chemistry, plant and animal pathology, and so on, up through cytology to the embryology of the higher forms of life. Through the problems of these researches also we are again brought face to face, sooner or later, with the problems of molecular science.

And finally, what may be said of anthropology, which is at once the most interesting and the most novel of the physical sciences, — interesting by reason of its subject-matter, novel by reason of its applications? Some of us, perhaps, might be inclined to demur from a classification which makes man, along with matter, a fit object of investigation in physical science. Granted even that he is usually a not altogether efficient thermodynamic engine, it may yet appear that he is worthy of a separate category. Fortunately, however, it is not a rule of physical science to demand immediate answers to such ulterior questions. It is enough for the present to know that man furnishes no exception, save in point of complexity, to the manifestations of physical phenomena so widely exhibited in the animal kingdom.

But whatever may be our inherited prejudices, or our philosophic judgments, we are confronted by the fact that the study of man in all his attributes is now an established domain of science. And herein we rise to a table-land of transcendent fascination; for, to adapt a phrase of an eminent master in physical science, the instruments of investigation are the objects of research. Herein also we find the culminating unity, not only of the physical sciences, but of all of the sciences; and it is chiefly for the promotion of these higher interests of anthropology that we are assembled in this cosmopolitan congress to-day.

It has been our good fortune to witness in recent decades an unparalleled series of achievements in the fields of physical science. All of them, from anthropology and astronomy up to zoölogy, have yielded rich harvests of results; and one is prone to raise the question whether a like degree of progress may be expected to prevail during the century on which we have now entered. No man can tell what a day may bring forth; much less may one forecast the progress of a decade or a century. But, judging from the long experience of the past, there are few reasons to doubt and many reasons to expect that the future has still greater achievements available. It would appear that we have found the right methods of investigation. Philosophically considered, the remarkable advances of the past afford little cause for marvel. On the contrary, they are just such results as we should anticipate from persistent pursuit of scientific investigation. Conscious of the adequacy of his methods, therefore, the devotee to physical science has every inducement to continue his labors with unflagging zeal and confident optimism.

DEPARTMENT IX — PHYSICS

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(*Hall* 6, *September* 20, 2 *p. m.*)

CHAIRMAN: PROFESSOR HENRY CREW, Northwestern University.
SPEAKERS: PROFESSOR EDWARD L. NICHOLS, Cornell University.
PROFESSOR CARL BARUS, Brown University.

THE Chairman of the Department of Physics was Professor Henry Crew, of Northwestern University, who opened the proceedings of the Department by saying: "Whatever views we may entertain concerning the classification of the sciences which Professor Münsterberg has proposed for the guidance of this congress, we will, I believe, all concur in the opinion that it is full of suggestion and very instructive. For my own part, I think it gives a really profound glimpse into the relationships of the various departments of human learning. You will recall that the first main division is between the pure and applied sciences. We have come together this afternoon to consider a subject which lies in the former group. But physics is not the only pure science: it is merely one belonging to that subdivision which deals with phenomena. Again, there are two classes of phenomena, the mental and the physical: and physics has to do only with the latter class. Indeed, it does not cover the entire field of physical phenomena, but constitutes merely one of the six Departments in this Division. Physics is, however, the most general and most fundamental of this group of six. It is properly found, therefore, at the head of the list. Our theme this afternoon, then, is that fundamental science which deals with the general properties of matter and energy and which includes the general principles of all physical phenomena. We are fortunate in having with us men who, by wide experience gained in their own researches, and by a thorough study of the philosophy of the subject, are eminently fitted to treat this topic."

THE FUNDAMENTAL CONCEPTS OF PHYSICAL SCIENCE

BY EDWARD LEAMINGTON NICHOLS

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ALL algebra, as was pointed out by von Helmholtz[1] nearly fifty years ago, is based upon the three following very simple propositions:

Things equal to the same thing are equal to each other.

If equals be added to equals the wholes are equal.

If unequals be added to equals the wholes are unequal.

Geometry, he adds, is founded upon a few equally obvious and simple axioms.

The science of physics, similarly, has for its foundation three fundamental conceptions: those of *mass*, *distance*, and *time*, in terms of which all physical quantities may be expressed.

Physics, in so far as it is an exact science, deals with the relations of these so-called physical quantities; and this is true not merely of those portions of the science which are usually included under the head of physics, but also of that broader realm which consists of the entire group of the physical sciences, viz., astronomy, the physics of the heavens; chemistry, the physics of the atom; geology, the physics of the earth's crust; biology, the physics of matter imbued with life; physics proper (mechanics, heat, electricity, sound, and light).

The manner in which the three fundamental quantities L, M, and T (length, mass, and time) enter, in the case of a physical quantity, is given by its *dimensional formula*.

Thus the dimensional formula for an acceleration is LT^{-2} which expresses the fact that an acceleration is a velocity (a length divided by a time) divided by a time. Energy has for its dimensional formula L^2MT^{-2}; it is a force, $LT^{-2}M$ (an acceleration multiplied by a mass), multiplied by a distance.

Not all physical quantities, in the present state of our knowledge, can be assigned a definite dimensional formula, and this indicates that not all of physics has as yet been reduced to a clearly established mechanical basis. The dimensional formula thus affords a valuable criterion of the extent and boundaries of our strictly definite knowledge of physics. Within these boundaries we are on safe and easy ground, and are dealing, independent of all speculation, with the relations between precisely defined quantities. These relations are mathematical, and the entire superstructure is erected upon the three fundamental quantities, L, M, and T, and certain definitions; just as geometry arises from its axioms and definitions.

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[1] Von Helmholtz, *Populäre Wissenschaftliche Vorträge*, p. 136.

Of many of those physical quantities, for which we are not as yet able to give the dimensional formula, our knowledge is precise and definite, but it is incomplete. In the case, for example, of one important group of quantities, those used in electric and magnetic measurements, we have to introduce, in addition to L, M, and T, a constant factor to make the dimensional formula complete. This, the *suppressed factor* of Rücker,[1] is μ, the magnetic permeability, when the quantity is expressed in the electromagnetic system, and becomes k, the specific inductive capacity, when the quantity is expressed in terms of the electrostatic system.

Here the existence of the suppressed factor is indicative of our ignorance of the mechanics involved. If we knew in what way a medium like iron increased the magnetic field, or a medium like glass the electric field, we should probably be able to express μ and k in terms of the three selected fundamental dimensions and complete the dimensional formulæ of a large number of quantities.

Where direct mechanical knowledge ceases, the great realm of physical speculation begins. It is the object of such speculation to place all phenomena upon a mechanical basis; excluding as unscientific all occult, obscure, and mystical considerations.

Whenever the mechanism by means of which phenomena are produced is incapable of direct observation either because of its remoteness in space, as in the case of physical processes occurring in the stars, or in time, as in the case of the phenomena with which the geologist has to do, or because of the minuteness of the moving parts, as in molecular physics, physical chemistry, etc., the speculative element is unavoidable. Here we are compelled to make use of analogy. We infer the unknown from the known. Though our logic be without flaw, and we violate no mathematical principle, yet are our conclusions not absolute. They rest of necessity upon *assumptions*, and these are subject to modification indefinitely as our knowledge becomes more complete.

A striking instance of the uncertainties of extrapolation and of the precarious nature of scientific assumptions is afforded by the various estimates of the temperature of the sun. Pouillet placed this temperature between 1461°C. and 1761°C.; Secchi at 5,000,000°; Ericsson at 2,500,000°. The newer determinations[1] of the temperature of the surface are, to be sure, in better agreement. Le Chatelier finds it to be 7600°; Paschen, 5400°; Warburg, 6000°. Wilson and Gray publish as their corrected result 8000°. The estimate of the internal temperature is of a more speculative character. Schuster's computation gives 6,000,000° to 15,000,000°; that of Kelvin, 200,000,000°; that of Ekholm, 5,000,000°.

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[1] Rücker, *Philos. Mag.*, 27, p. 104. 1889.

Another interesting illustration of the dangers of extrapolation occurs in the history of electricity. Faraday, starting from data concerning the variation between the length of electric sparks through air with the difference of potential, made an interesting computation of the potential difference between earth and sky necessary to discharge a cloud at a height of one mile. He estimated the difference of potential to be about 1,000,000 volts. Later investigations of the sparking distance have, however, shown this function to possess a character quite different from that which might have been inferred from the earlier work, and it is likely that Faraday's value is scarcely nearer the truth than was the original estimate of the temperature of the sun, mentioned above.

Still another notable instance of the errors to which physical research is subject when the attempt is made to extend results beyond the limits established by actual observation occurs in the case of the measurements of the infra-red spectrum of the sun by Langley. His beautiful and ingenious device, the bolometer, made it possible to explore the spectrum to wave-lengths beyond those for which the law of dispersion of the rock-salt prism had at that time been experimentally determined. Within the limits of observation the dispersion showed a curve of simple form, tending apparently to become a straight line as the wave-length increased. There was nothing in the appearance of the curve to indicate that it differed in character from the numerous empirical curves of similar type employed in experimental physics, or to lead even the most experienced investigator to suspect values for the wave-length derived from an extension of the curve. The wave-lengths published by Langley were accordingly accepted as substantially correct by all other students of radiation; but subsequent measurements of the dispersion of rock salt at the hands of Rubens and his co-workers showed the existence of a second sudden and unlooked-for turn of the curve just beyond the point at which the earlier determinations ceased; and in consequence Langley's wave-lengths and all work based upon them are now known to be not even approximately accurate. The history of physics is full of such examples of the dangers of extrapolation, or, to speak more broadly, of the tentative character of most of our assumptions in experimental physics.

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[1] See Arrhenius, *Kosmische Physik*, p. 131.

We have, then, two distinct sets of physical concepts. The first of these deals with that positive portion of physics, the mechanical basis of which, being established upon direct observation, is fixed and definite, and in which the relations are as absolute and certain as those of mathematics itself. Here speculation is excluded. Matter is simply one of the three factors, which enters, by virtue of its mass, into our formulæ for energy, momentum, etc. Force is simply a quantity of which we need to know only its magnitude, direction, point of application, and the time during which it is applied. The Newtonian conception of force — the producer of motion — is adequate. All troublesome questions as to how force acts, of the mechanism by means of which its effects are produced, are held in abeyance.

Speculative physics, to which the second set of concepts belongs, deals with those portions of the science for which the mechanical basis has to be imagined. Heat, light, electricity, and the science of the nature and ultimate properties of matter belong to this domain.

In the history of the theory of heat we find one of the earliest manifestations of a tendency so common in speculative physics that it may be considered characteristic: the assumption of a medium. The medium in this case was the so-called *imponderable* caloric; and it was one of a large class, of which the two electric fluids, the magnetic fluid, etc., were important members.

The theory of heat remained entirely speculative up to the time of the establishment of the mechanical equivalent of heat by Joule. The discovery that heat could be measured in terms of work injected into thermal theory the conception of energy, and led to the development of thermodynamics.

Generalizations of the sort expressed by Tyndall's phrase, *heat a mode of motion*, follow easily from the experimental evidence of the part which energy plays in thermal phenomena, but the specification of the precise mode of motion in question must always depend upon our views concerning the nature of matter, and can emerge from the speculative stage only, if ever, when our knowledge of the mechanics of the constitution of matter becomes fixed. The problem of the mechanism by which energy is stored or set free rests upon a similar speculative basis.

These are proper subjects for theoretical consideration, but the dictum of Rowland [1] that we get out of mathematical formulæ only what we put into them should never be lost from sight. So long as we put in only assumptions we shall take out hypotheses, and useful as these may prove, they are to be regarded as belonging to the realm of scientific speculation. They must be recognized as subject to modification indefinitely as we, in consequence of increasing knowledge, are led to modify our assumptions.

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[1] Rowland, *President's Address to the American Physical Society*, 1900.

The conditions with which the physicist has to deal in his study of optics are especially favorable to the development of the scientific imagination, and it is in this field that some of the most remarkable instances of successful speculative work are to be found. The emission theory died hard, and the early advocates of the undulatory theory of light were forced to work up, with a completeness probably without parallel in the history of science, the evidence, necessarily indirect, that in optics we have to do with a wave-motion. The standpoint of optical theory may be deemed conclusive, possibly final, so far as the general proposition is concerned that it is the science of a wave-motion. In a few cases, indeed, such as the photography of the actual nodes of a standing wave-system, by Wiener, we reach the firm ground of direct observation.

Optics has nevertheless certain distinctly speculative features. Wave-motion demands a medium. The enormous velocity of light excludes known forms of matter; the transmission of radiation *in vacuo* and through outer space from the most remote regions of the universe, and at the same time through solids such as glass, demands that this medium shall have properties very different from that of any substance with which chemistry has made us acquainted.

The assumption of a medium is, indeed, an intellectual necessity, and the attempt to specify definitely the properties which it must possess in order to fulfill the extraordinary functions assigned to it has afforded a field for the highest display of scientific acumen. While the problem of the mechanism of the luminiferous ether has not as yet met with a satisfactory solution, the ingenuity and imaginative power developed in the attack upon its difficulties command our admiration.

Happily the development of what may be termed the older optics did not depend upon any complete formulation of the mechanics of the ether. Just as the whole of the older mechanics was built up from Kepler's laws, Newton's laws of motion, the law of gravitational attraction, the law of inverse squares, etc., without any necessity of describing the mechanics of gravitation or of any force, or of matter itself, so the system of geometrical relations involved in the consideration of reflection and refraction, diffraction, interference, and polarization was brought to virtual completion without introducing the troublesome questions of the nature of the ether and the constitution of matter.

Underlying this field of geometrical optics, or what I have just termed the older optics, are, however, a host of fundamental questions of the utmost interest and importance, the treatment of which depends upon molecular mechanics and the mechanics of the ether. Our theories as to the nature and causes of radiation, of absorption, and of dispersion, for example, belong to the newer optics, and are based upon our conceptions of the constitution of matter; and since our ideas concerning the nature of matter, like our knowledge of the ether, is purely speculative, the science of optics has a doubly speculative basis. One type of selective absorption, for example, is ascribed to resonance of the particles of the absorbing substance, and our modern dispersion theories depend upon the assumption of natural periods of vibration of the particles of the refracting medium of the same order of frequency as that of the light-waves. When the frequency of the waves falling upon a substance coincides with the natural period of vibration of the particles of the latter, we have selective absorption, and accompanying it, anomalous dispersion. For these and numerous other phenomena no adequate theory is possible which does not have its foundation upon some assumed conception as to the constitution of matter.

The development of the modern idea of the ether forms one of the most interesting chapters in the history of physics. We find at first a tendency to assume a number of distinct media corresponding to the various effects (visual, chemical, thermal, phosphorescent, etc.) of light-waves, and later the growth of the conception of a single medium, the luminiferous ether.

In the development of electricity and magnetism, meantime, the assumption of media was found to be an essential—something without which no definite philosophy of the phenomena was possible. At first there was the same tendency to a multiplicity of media — there were the positive and negative electric fluids, the magnetic fluid, etc. Then there grew up in the fertile mind of Faraday that wonderful fabric of the scientific imagination, the electric field; the conception upon which all later attempts to form an idea of a thinkable mechanism of electric and magnetic action have been established.

It is the object of science, as has been pointed out by Ostwald, to reduce the number of hypotheses; the highest development would be that in which a single hypothesis served to elucidate the relations of the entire universe. Maxwell's discovery that the whole theory of optics is capable of expression in terms identical with those found most convenient and suitable in electricity, in a word, that optics may be treated simply as a branch of electromagnetics, was the first great step towards such a simplification of our fundamental conceptions. This was followed by Hertz's experimental demonstration of the existence of artificially produced electromagnetic waves in every respect identical with light-waves, an achievement which served to establish upon a sure foundation the conception of a single medium. The idea of one universal medium as the mechanical basis for all physical phenomena was not altogether new to the theoretical physicist, but the unification of optics and electricity did much to strengthen this conception.

The question of the ultimate structure of matter, as has already been pointed out, is also speculative in the sense that the mechanism upon which its properties are based is out of the range of direct observation. For the older chemistry and the older molecular physics the assumption of an absolutely simple atom and of molecules composed of comparatively simple groupings of such atoms sufficed. Physical chemistry and that new phase of molecular physics which has been termed the physics of the ion demand the breaking up of the atom into still smaller parts and the clothing of these with an electric charge. The extreme step in this direction is the suggestion of Larmor that the electron is a "disembodied charge" of negative electricity. Since, however, in the last analysis, the only conception having a definite and intelligible mechanical basis which physicists have been able to form of an electric charge is that which regards it as a phenomenon of the ether, this form of speculation is but a return under another name to views which had earlier proved attractive to some of the most brilliant minds in the world of science, such as Helmholtz and Kelvin. The idea of the atom, as a vortex motion of a perfect fluid (the ether), and similar speculative conceptions, whatever be the precise form of mechanism imagined, are of the same class as the moving electric charge of the later theorists.

Lodge,[1] in a recent article in which he attempts to voice in a popular way the views of this school of thought, says:

"*Electricity under strain constitutes 'charge'; electricity in locomotion constitutes light. What electricity itself is we do not know, but it may, perhaps, be a form or aspect of matter. . . . Now we can go one step further and say, matter is composed of electricity and of nothing else. . . .*"

If for the word *electricity* in this quotation from Lodge we substitute *ether*, we have a statement which conforms quite as well to the accepted theories of light and electricity as his original statement does to the newer ideas it is intended to express.

This reconstructed statement would read as follows:

Ether under strain constitutes "charge"; *ether* in locomotion constitutes current and magnetism; *ether* in vibration constitutes light. What *ether* itself is we do not know, but it may, perhaps, be a form or aspect of matter. Now we can go one step further and say: "Matter is composed of *ether* and of nothing else."

The use of the word electricity, as employed by Lodge and others, is now much in vogue, but it appears to me unfortunate. It would be distinctly conducive to clearness of thought and an avoidance of confusion to restrict the term to the only meaning which is free from criticism; that in which it is used to designate the science which deals with electrical phenomena.

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[1] Lodge, *Harper's Magazine*, August, 1904, p. 383.

The only way in which the noun *electricity* enters, in any definite and legitimate manner, into our electrical treatises is in the designation of Q in the equations—

$$Q = \int I dt, \; C = Q|E, \; W = QE, \text{ etc.}$$

Here we are in the habit — whether by inheritance from the age of the electric fluid, by reason of the hydrodynamic analogy, or as a matter of convention or of convenience merely — of calling Q the *quantity of electricity*.

Now Q is "charge" and its unit, the coulomb, is unit-charge. The alternative expression, *quantity of electricity*, is a purely conventional designation and without independent physical significance. It owes its prevalence among electricians to the fact that by virtue of long familiarity we prefer to think in terms of matter, which is tangible, rather than of ether. Charge is to be regarded as fundamental, and its substitute, quantity of electricity, as merely an artificial term of convenience; because of the former we have a definite mechanical conception, whereas we can intelligently define a quantity of electricity only in terms of *charge*.

In the science of heat the case differs, in that the term heat is used, if not as precisely synonymous with energy, at least for a quantity having the same dimensions as energy and having as its unit the erg. It might easily have happened, as has happened in electrical theory, that the ancient notion of a *heat substance* should survive, in which case we should have had for the quantity of heat not something measured in terms of energy, but, as in the case of electricity, one of the terms which enter into our expression for energy. We should then have had to struggle continually, in thermodynamics, as we now do in electrical theory, against the tendency to revert to an antiquated and abandoned view.

It would, I cannot but think, have been fortunate had the word electricity been used for what we now call electrical energy; using *charge*, or some other convenient designation, for the quantity Q. That aspect of the science in accordance with which we regard it as a branch of energetics in which movements of the ether are primarily involved would have been duly emphasized. We should have been quit forever of the bad notion of electricity as a medium, just as we are already freed from the incubus of heat as a medium. We should have had *electricity — a mode of motion* (or stress), *ether*, as we have *heat — a mode of motion of matter*. When our friends asked us: "What is electricity?" we should have had a ready answer for them instead of a puzzled smile.

One real advance which has been attained by means of the theory of ionization, and it is of extreme significance and of far-reaching importance, consists in the discovery that electrification, or the possession of charge, instead of being a casual or accidental property, temporarily imparted by friction or other process, is a fundamental property of matter. According to this newer conception of matter, the fruit of the ionic theory, the ultimate parts of matter are electrically charged particles. In the language of Rutherford:[1]

"It must then be supposed that the process of ionization in gases consists in a removal of a negative corpuscle or electron from the molecule of gas. At atmospheric pressure this corpuscle immediately becomes the centre of an aggregation of molecules which moves with it and *is* the negative ion. After removal of the negative ion the molecule retains a positive charge and probably also becomes the centre of a cluster of new molecules.

"The *electron* or *corpuscle* is the body of smallest mass yet known to science. It carries a negative charge of 3.4×10^{-10} electrostatic units. Its presence has only been detected when in rapid motion, when it has for speeds up to about 10^{10} cms. a second, an apparent mass m given by $e/m - 1.86\times10^{7}$ electromagnetic units. This apparent mass increases with the speed as the velocity of light is approached."

At low pressures the electron appears to lose its load of clustering molecules, so that finally the negative ion becomes identical with the electron or corpuscle, and has a mass, according to the estimates of J. J. Thomson, about one thousandth of that of the hydrogen atom. The positive ion is, however, supposed to remain of atomic size even at low pressures.

The ionic theory and the related hypothesis of electrolytic dissociation afford a key to numerous phenomena concerning which no adequate or plausible theories had hitherto been formed. By means of them explanations have been found, for example, of such widely divergent matters as the positive electric charge known to exist in the upper atmosphere, and the perplexing phenomena of fluorescence.

The evidence obtained by J. J. Thomson and other students of ionization, that electrons from different substances are identical, has greatly strengthened the conviction which for a long time has been in process of formation in the minds of physicists, that all matter is in its ultimate nature identical. This conception, necessarily speculative, has been held in abeyance by the facts, regarded as established, and lying at the foundation of the accepted system of chemistry, of the conservation of matter and the intransmutability of the elements. The phenomena observed in recent investigations of radioactive substances have, however, begun to shake our faith in this principle.

If matter is to be regarded as a product of certain operations performed upon the ether, there is no theoretical difficulty about transmutation of elements, variation of mass, or even the complete disappearance or creation of matter. The absence of such phenomena in our experience has been the real difficulty, and if the views of students of radioactivity concerning the transformations undergone by uranium, thorium, and radium are substantiated, the doctrines of the conservation of mass and matter which lie at the foundation of the science of chemistry will have to be modified. There has been talk of late of violations of the principle of the conservation of energy in connection with the phenomena of radioactivity, but the conservation of matter is far more likely to lose its place among our fundamental conceptions.

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[1] Rutherford, *Radioactivity*, p. 53. 1904.

The development of physics on the speculative side has led, then, to the idea, gradually become more definite and fixed, of a universal medium, the existence of which is a matter of inference. To this medium properties have been assigned which are such as to enable us to form an intelligible, consistent conception of the mechanism by means of which phenomena, the mechanics of which is not capable of direct observation, may be logically considered to be produced. The great step in this speculation has been the discovery that a single medium may be made to serve for the numerous phenomena of optics, and that, without ascribing to it any characteristics incompatible with a luminiferous ether, it is equally available for the description and explanation of electric and magnetic fields, and finally may be made the basis for intelligible theories of the structure of matter.

To many minds this seemingly universal adaptability of the ether to the needs of physics almost removes it from the field of speculation; but it should not be forgotten that a system, entirely imaginary, may be devised, which fits all the known phenomena and appears to offer the only satisfactory explanation of the facts, and which subsequently is abandoned in favor of other views. The history of physics is full of instances where a theory is for a time regarded as final on account of its seeming completeness, only to give way to something entirely different.

In this consideration of the fundamental concepts I have attempted to distinguish between those which have the positive character of mathematical laws and which are entirely independent of all theories of the ultimate nature of matter, and those which deal with the latter questions and which are essentially speculative. I have purposely refrained from taking that further step which plunges us from the heights of physics into the depths of philosophy.

With the statement that science in the ultimate analysis is nothing more than *an attempt to classify and correlate our sensations* the physicist has no quarrel. It is, indeed, a wholesome discipline for him to formulate for himself his own relations to his science in terms such as those which, to paraphrase and translate very freely the opening passages of his recent *Treatise on Physics*, Chwolson[1] has employed.

"For every one there exist two worlds, an inner and an outer, and our senses are the medium of communication between the two. The outer world has the property of acting upon our senses, to bring about certain changes, or, as we say, to exert certain stimuli.

"The inner world, for any individual, consists of all those phenomena which are absolutely inaccessible (so far as direct observation goes) to other individuals. The stimulus from the outer world produces in our inner world a subjective perception which is dependent upon our *consciousness*. The subjective perception is made objective, viz., is assigned *time* and *place* in the outer world and given a name. The investigation of the processes by which this objectivication is performed is a function of philosophy."

Some such confession of faith is good for the man of science, —*lest he forget;* but once it is made he is free to turn his face to the light once more, thankful that the *investigation of objectivication* is, indeed, *a function of philosophy*, and that the only speculations in which he, as a physicist, is entitled to engage are those which are amenable at every step to mathematics and to the equally definite axioms and laws of mechanics.

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[1] Chwolson, *Physik*, vol. I, Introduction.

THE PROGRESS OF PHYSICS IN THE NINETEENTH CENTURY

BY CARL BARUS

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You have honored me by requesting at my hands an account of the advances made in physics during the nineteenth century. I have endeavored, in so far as I have been able, to meet the grave responsibilities implied in your invitation; yet had I but thought of the overwhelmingly vast territory to be surveyed, I well might have hesitated to embark on so hazardous an undertaking. To mention merely the *names* of men whose efforts are linked with splendid accomplishments in the history of modern physics would far exceed the time allotted to this address. To bear solely on certain subjects, those, for instance, with which I am more familiar, would be to develop an unsymmetrical picture. As this is to be avoided, it will be necessary to present a straightforward compilation of all work above a certain somewhat vague and arbitrary lower limit of importance. Physics is, as a rule, making vigorous though partial progress along independent parallel lines of investigation, a discrimination between which is not possible until some cataclysm in the history of thought ushers in a new era. It will be essential to abstain from entering into either explanation or criticism, and to assume that all present are familiar with the details of the subjects to be treated. I can neither popularize nor can I endeavor to entertain, except in so far as a rapid review of the glorious conquests of the century may be stimulating.

In spite of all this simplicity of aim, there is bound to be distortion. In any brief account, the men working at the beginning of the century, when investigations were few and the principles evolved necessarily fundamental, will be given greater consideration than equally able and abler investigations near the close, when workers (let us be thankful) were many, and the subjects lengthening into detail. Again, the higher order of genius will usually be additionally exalted at the expense of the less gifted thinker. I can but regret that these are the inevitable limitations of the cursory treatment prescribed.

As time rolls on, the greatest names more and more fully absorb the activity of a whole epoch.

Metrology

Finally, it will hardly be possible to consider the great advances made in physics except on the theoretical side. Of renowned experimental researches, in particular of the investigations of the constants of nature to a degree of ever-increasing accuracy, it is not practicable to give any adequate account. Indeed, the refinement and precision now demanded have placed many subjects beyond the reach of individual experimental research, and have culminated in the establishment of the great national or international laboratories of investigation at Sèvres (1872), at Berlin (1887, 1890), at London (1900), at Washington (1901). The introduction of uniform international units in cases of the arts and sciences of more recent development is gradually, but inexorably, urging the same advantages on all. Finally, the access to adequate instruments of research has everywhere become an easier possibility for those duly qualified, and the institutions and academies which are systematically undertaking the distribution of the means of research are continually increasing in strength and in number.

Classification

In the present paper it will be advisable to follow the usual procedure in physics, taking in order the advances made in dynamics, acoustics, heat, light, and electricity. The plan pursued will, therefore, specifically consider the progress in elastics, crystallography, capillarity, solution, diffusion, dynamics, viscosity, hydrodynamics, acoustics; in thermometry, calorimetry, thermodynamics, kinetic theory, thermal radiation; in geometric optics, dispersion, photometry, fluorescence, photochemistry, interference, diffraction, polarization, optical media; in electrostatics, Volta contacts, Seebeck contacts, electrolysis, electric current, magnetism, electromagnetism, electrodynamics, induction, electric oscillation, electric field, radioactivity.

Surely this is too extensive a field for any one man! Few who are not physicists realize that each of these divisions has a splendid and voluminous history of development, its own heroes, its sublime classics, often culled from the activity of several hundred years. I repeat that few understand the unmitigatedly fundamental character, the scope, the vast and profound intellectual possessions, of pure physics; few think of it as the one science into which all other sciences must ultimately converge — or a separate representation would have been given to most of the great divisions which I have named.

Hence even if the literary references may be given in print with some fullness, it is impossible to refer verbally to more than the chief actors, and quite impossible to delineate sharply the real significance and the relations of what has been done. Moreover, the dates will in most instances have to be omitted from the reading. It has been my aim, however, to collect the greater papers in the history of physics, and the suggestion is implied that science would gain if by some august tribunal researches of commanding importance were formally canonized for the benefit of posterity.

Elastics

To begin with elasticity, whose development has been of such marked influence throughout the whole of physics, we note that the theory is virtually a creation of the nineteenth century. Antedating Thomas Young, who in 1807 gave to the subject the useful conception of a modulus, and who seems to have definitely recognized the shear, there were merely the experimental contribution of Galileo (1638), Hooke (1660), Mariotte (1680), the elastic curve of J. Bernoulli (1705), the elementary treatment of vibrating bars of Euler and Bernoulli (1742), and an attempted analysis of flexure and torsion by Coulomb (1776).

The establishment of a theory of elasticity on broad lines begins almost at a bound with Navier (1821), reasoning from a molecular hypothesis to the equation of elastic displacement and of elastic potential energy (1822–1827); yet this startling advance was destined to be soon discredited, in the light of the brilliant generalizations of Cauchy (1827). To him we owe the six component stresses and the six component strains, the stress quadric and the strain quadric, the reduction of the components to three principal stresses and three principal strains, the ellipsoids, and other of the indispensable conceptions of the present day. Cauchy reached his equations both by the molecular hypothesis and by an analysis of the oblique stress across an interface, — methods which predicate fifteen constants of elasticity in the most general case, reducing to but one in the case of isotropy. Contemporaneous with Cauchy's results are certain independent researches by Lamé and Clapeyron (1828) and by Poisson (1829).

Another independent and fundamental method in elastics was introduced by Green (1837), who took as his point of departure the potential energy of a conservative system in connection with the Lagrangian principle of virtual displacements. This method, which has been fruitful in the hands of Kelvin (1856), of Kirchhoff (1876), of Neumann (1885), leads to equations with twenty-one constants for the æolotropic medium reducing to two in the simplest case.

The wave-motion in an isotropic medium was first deduced by Poisson in 1828, showing the occurrence of longitudinal and transverse waves of different velocities; the genéral problem of wave-motion in æolotropic media, though treated by Green (1842), was attacked with requisite power by Blanchet (1840–1842) and by Christoffel (1877).

Poisson also treated the case of radial vibrations of a sphere (1828), a problem which, without this restriction, awaited the solutions of Jaerisch (1879) and of Lamb (1882). The theory of the free vibrations of solids, however, is a generalization due to Clebsch (1857–58, *Vorlesungen,* 1862).

Elasticity received a final phenomenal advance through the long-continued labors of de St. Venant (1839–55), which in the course of his editions of the work of Moigno, of Navier (1863), and of Clebsch (1864), effectually overhauled the whole subject. He was the first to assert adequately the fundamental importance of the shear. The profound researches of de St. Venant on the torsion of prisms and on the flexure of prisms appeared in their complete form in 1855 and 1856. In both cases the right sections of the stressed solids are shown to be curved, and the curvature is succinctly specified; in the former Coulomb's inadequate torsion formula is superseded, and in the latter flexural stress is reduced to a transverse force and a couple. But these mere statements convey no impression of the magnitude of the work.

Among other notable creations with a special bearing on the theory of elasticity there is only time to mention the invention and application of curvilinear coördinates by Lamé (1852); the reciprocal theorem of Betti (1872), applied by Cerruti (1882) to solids with a plane boundary — problems to which Lamé and Clapeyron (1828) and Boussinesq (1879–85) contributed by other methods; the case of the strained sphere studied by Lamé (1854) and others; Kirchhoff's flexed plate (1850); Rayleigh's treatment of the oscillations of systems of finite freedom (1873); the thermo-elastic equations of Duhamel (1838), of F. Neumann (1841), of Kelvin (1878); Kelvin's analogy of the torsion of prisms with the supposed rotation of an incompressible fluid within (1878); his splendid investigations (1863) of the dynamics of elastic spheroids and the geophysical applications to which they were put.

Finally, the battle royal of the molecular school following Navier, Poisson, Cauchy, and championed by de St. Venant, with the disciples of Green, headed by Kelvin and Kirchhoff, — the struggle of the fifteen constants with the twenty-one constants, in other words, — seems to have temporarily subsided with a victory for the latter through the researches of Voigt (1887–89).

Crystallography

Theoretical crystallography, approached by Steno (1669), but formally founded by Haüy (1781, *Traité*, 1801), has limited its development during the century to systematic classifications of form. Thus the thirty-two type sets of Hessel (1830) and of Bravais (1850) have expanded into the more extensive point series involving 230 types due to Jordan (1868), Sohncke (1876), Federow (1890), and Schoenfliess (1891). Physical theories of crystalline form have scarcely been unfolded.

Capillarity

Capillarity antedated the century in little more than the provisional, though brilliant, treatment due to Clairaut (1743). The theory arose in almost its present state of perfection in the great memoir of Laplace (1805), one of the most beautiful examples of the Newton-Boscovichian (1758) molecular dynamics. Capillary pressure was here shown to vary with the principal radii of curvature of the exposed surface, in an equation involving two constants, one dependent on the liquid only, the other doubly specific for the bodies in contact. Integrations for special conditions include the cases of tubes, plates, drops, contact angle, and similar instances. Gauss (1829), dissatisfied with Laplace's method, virtually reproduced the whole theory from a new basis, avoiding molecular forces in favor of Lagrangian displacements, while Poisson (1831) obtained Laplace's equations by actually accentuating the molecular hypothesis; but his demonstration has since been discredited. Young in 1805 explained capillary phenomena by postulating a constant surface tension, a method which has since been popularized by Maxwell (*Heat*, 1872).

With these magnificent theories propounded for guidance at the very threshold of the century, one is prepared to anticipate the wealth of experimental and detailed theoretical research which has been devoted to capillarity. Among these the fascinating monograph of Plateau (1873), in which the consequences of theory are tested by the behavior both of liquid lamellæ and by suspended masses, Savart's (1833), and particularly Rayleigh's, researches with jets (1879–83), Kelvin's ripples (1871), may be cited as typical. Of peculiar importance, quite apart from its meteorological bearing, is Kelvin's deduction (1870) of the interdependence of surface tension and vapor pressure when varying with the curvature of a droplet.

Diffusion

Diffusion was formally introduced into physics by Graham (1850). Fick (1855), appreciating the analogy of diffusion and heat conduction, placed the phenomenon on a satisfactory theoretical basis, and Fick's law has since been rigorously tested, in particular by H. F. Weber (1879).

The development of diffusion from a physical point of view followed Pfeffer's discovery (1877) of osmotic pressure, soon after to be interpreted by van 't Hoff (1887) in terms of Boyle's and Avogadro's laws. A molecular theory of diffusion was thereupon given by Nernst (1887).

Dynamics

In pure dynamics the nineteenth century inherited from the eighteenth that unrivaled feat of reasoning called by Lagrange the *Mécanique analytique* (1788), and the great master was present as far as 1813 to point out its resources and to watch over the legitimacy of its applications. Throughout the whole century each new advance has but vindicated the preëminent power and safety of its methods. It triumphed with Maxwell (1864), when he deduced the concealed kinetics of the electromagnetic field, and with Gibbs (1876–78), when he adapted it to the equilibrium of chemical systems. It will triumph again in the electromagnetic dynamics of the future.

Naturally there were reactions against the tyranny of the method of "liaisons." The most outspoken of these, propounded under the protection of Laplace himself, was the celebrated *Mécanique physique* of Poisson (1828), an accentuation of Boscovich's (1758) dynamics, which permeates the work of Navier, Cauchy, de St. Venant, Boussinesq, even Fresnel, Ampère, and a host of others. Cauchy in particular spent much time to reconcile the molecular method with the Lagrangian abstractions. But Poisson's method, though sustained by such splendid genius, has, nevertheless, on more than one occasion — in capillarity, in elastics — shown itself to be untrustworthy. It was rudely shaken when, with the rise of modern electricity, the influence of the medium was more and more pushed to the front.

Another complete reconstruction of dynamics is due to Thomson and Tait (1867), in their endeavor to gain clearness and uniformity of design, by referring the whole subject logically back to Newton. This great work is the first to make systematic use of the doctrine of the conservation of energy.

Finally, Hertz (1894), imbued with the general trend of contemporaneous thought, made a powerful effort to exclude force and potential energy from dynamics altogether — postulating a universe of concealed motions such as Helmholtz (1884) had treated in his theory of cyclic systems, and Kelvin had conceived in his adynamic gyrostatic ether (1890). In fact, the introduction of concealed systems and of ordered molecular motions by Helmholtz and Boltzmann has proved most potent in justifying the Lagrangian dynamics in its application to the actual motions of nature.

The specific contributions of the first rank which dynamics owes to the last century, engrossed as it was with the applications of the subject, or with its mathematical difficulties, are not numerous. In chronological order we recall naturally the statics (1804) and the rotational dynamics (1834) of Poinsot, all in their geometrical character so surprisingly distinct from the contemporary dynamics of Lagrange and Laplace. We further recall Gauss's principle of least constraint (1829), but little used, though often in its applications superior to the method of displacement; Hamilton's principle of varying action (1834) and his characteristic function (1834, 1835), the former obtainable by an easy transition from D'Alembert's principle and by contrast with Gauss's principle, of such exceptional utility in the development of modern physics; finally the development of the Leibnitzian doctrine of work and *vis viva* into the law of the conservation of energy, which more than any other principle has consciously pervaded the progress of the nineteenth century. Clausius's theorem of the *Virial* (1870) and Jacobi's (1866) contributions should be added among others.

The potential, though contained explicitly in the writings of Lagrange (1777), may well be claimed by the last century. The differential equation underlying the doctrine had already been given by Laplace in 1782, but it was subsequently to be completed by Poisson (1827). Gauss (1813, 1839) contributed his invaluable theorems relative to the surface integrals and force flux, and Stokes (1854) his equally important relation of the line and the surface integral. Legendre (published 1785) and Laplace (1782) were the first to apply spherical harmonics in expansions. The detailed development of volume surface and line potential has enlisted many of the ablest writers, among whom Chasles (1837, 1839, 1842), Helmholtz (1853), C. Neumann (1877, 1880), Lejeune-Dirichlet (1876), Murphy (1833), and others are prominent.

The gradual growth of the doctrine of the potential would have been accelerated, had not science to its own loss overlooked the famous essay of Green (1828), in which many of the important theorems were anticipated, and of which Green's theorem and Green's function are to-day familiar reminders.

Recent dynamists incline to the uses of the methods of modern geometry and to the vector calculus with continually increasing favor. Noteworthy progress was first made in this direction by Moebius (1837–43, *Statik*, 1838), but the power of these methods to be fully appreciated required the invention of the *Ausdehnungslehre*, by Grassmann (1844), and of *quaternions*, by Hamilton (1853).

Finally the profound investigations of Sir Robert Ball (1871, *et seq.*, *Treatise*) on the theory of screws with its immediate dynamical applications, though as yet but little cultivated except by the author, must be reckoned among the promising heritages of the twentieth century.

On the experimental side it is possible to refer only to researches of a strikingly original character, like Foucault's pendulum (1851) and Fizeau's gyrostat; or like Boys's (1887, *et seq.*) remarkable quartz-fibre torsion-balance, by which the Newtonian constant of gravitation and the mean density of the earth originally determined by Maskelyne (1775–78) and by Cavendish (1798) were evaluated with a precision probably superior to that of the other recent measurements, the pendulum work of Airy (1856) and Wilsing (1885–87), or the balance methods of Jolly (1881), König, and Richarz (1884). Extensive transcontinental gravitational surveys like that of Mendenhall (1895) have but begun.

Hydrodynamics

The theory of the equilibrium of liquids was well understood prior to the century, even in the case of rotating fluids, thanks to the labors of Maclaurin (1742), Clairaut (1743), and Lagrange (1788). The generalizations of Jacobi (1834) contributed the triaxial ellipsoid of revolution, and the case has been extended to two rotating attracting masses by Poincaré (1885) and Darwin (1887). The astonishing revelations contained in the recent work of Poincaré are particularly noteworthy.

Unlike elastics, theoretical hydrodynamics passed into the nineteenth century in a relatively well-developed state. Both types of the Eulerian equations of motion (1755, 1759) had left the hands of Lagrange (1788) in their present form. In relatively recent times H. Weber (1868) transformed them in a way combining certain advantages of both, and another transformation was undertaken by Clebsch (1859). Hankel (1861) modified the equation of continuity, and Svanberg and Edlund (1847) the surface conditions.

Helmholtz in his epoch-making paper of 1858 divided the subject into those classes of motion (flow in tubes, streams, jets, waves) for which a velocity potential exists and the vortex motions for which it does not exist. This classification was carried even into higher orders of motion by Craig and by Rowland (1881). For cases with a velocity potential, much progress has been made during the century in the treatment of waves, of discontinuous fluid motion, and in the dynamics of solids suspended in frictionless liquids. Kelland (1844), Scott Russel (1844), and Green (1837) dealt with the motion of progressive waves in relatively shallow vessels, Gerster (1804) and Rankine (1863) with progressive waves in deep water, while Stokes (1846, 1847, 1880), after digesting the contemporaneous advances in hydrodynamics, brought his powerful mind to bear on most of the outstanding difficulties. Kelvin introduced the case of ripples (1871), afterwards treated by Rayleigh (1883). The solitary wave of Russel occupied Boussinesq (1872, 1882), Rayleigh (1876), and others; group-waves were treated by Reynolds (1877) and Rayleigh (1879). Finally the theory of stationary waves received extended attention in the writings of de St. Venant (1871), Kirchhoff (1879), and Greenhill (1887). Early experimental guidance was given by the classic researches of C. H. and W. Weber (1825).

The occurrence of discontinuous variation of velocity within the liquid was first fully appreciated by Helmholtz (1868), later by Kirchhoff (1869), Rayleigh (1876), Voigt (1885), and others. It lends itself well to conformal representations.

The motions of solids within a liquid have fascinated many investigators, and it is chiefly in connection with this subject that the method of sources and sinks was developed by English mathematicians, following Kelvin's method (1856) for the flow of heat. The problem of the sphere was solved more or less completely by Poisson (1832), Stokes (1843), Dirichlet (1852); the problem of the ellipsoid by Green (1833), Clebsch (1858), generalized by Kirchhoff (1869). Rankine treated the translatory motion of cylinders and ellipsoids in a way bearing on the resistance of ships. Stokes (1843) and Kirchhoff entertain the question of more than one body. The motion of rings has occupied Kirchhoff (1869), Boltzmann (1871), Kelvin (1871), Bjerknes (1879), and others. The results of C. A. Bjerknes (1868) on the fields of hydrodynamic force surrounding spheres, pulsating or oscillating, in translatory or rotational motion, accentuate the remarkable similarity of these fields with the corresponding cases in electricity and magnetism, and have been edited in a unique monograph (1900) by his son. In a special category belong certain powerful researches with a practical bearing, such as the modern treatment of ballistics by Greenhill and of the ship propeller of Ressel (1826), summarized by Gerlach (1885, 1886).

The numerous contributions of Kelvin (1888, 1889) in particular have thrown new light on the difficult but exceedingly important question of the stability of fluid motion.

The century, moreover, has extended the working theory of the tides due to Newton (1687) and Laplace (1774), through the labors of Airy, Kelvin, and Darwin.

Finally the forbidding subject of vortex motion was gradually approached more and more fully by Lagrange, Cauchy (1815, 1827), Svanberg (1839), Stokes (1845); but the epoch-making integrations of the differential equations, together with singularly clear-cut interpretations of the whole subject, are due to Helmholtz (1858). Kelvin (1867, 1883) soon recognized the importance of Helmholtz's work and extended it, and further advance came in particular from J. J. Thomson (1883) and Beltrami (1875). The conditions of stability in vortex motion were considered by Kelvin (1880), Lamb (1878), J. J. Thomson, and others, and the cases of one or more columnar vortices, of cylindrical vortex sheets, of one or more vortex rings, simple or linked, have all yielded to treatment.

The indestructibility of vortex motion in a frictionless fluid, its open structure, the occurrence of reciprocal forces, were compared by Kelvin (1867) with the essential properties of the atom. Others like Fitzgerald in his cobwebbed ether, and Hicks (1885) in his vortex sponge, have found in the properties of vortices a clue to the possible structure of the ether. Yet it has not been possible to deduce the principles of dynamics from the vortex hypothesis, neither is the property which typifies the mass of an atom clearly discernible. Kelvin invokes the corpuscular hypothesis of Lesage (1818).

Viscosity

The development of viscous flow is largely on the experimental side, particularly for solids, where Weber (1835), Kohlrausch (1863, *et seq.*), and others have worked out the main laws. Stokes (1845) deduced the full equations for liquids. Poiseiulle's law (1847), the motion of small solids in viscous liquids, of vibrating plates, and other important special cases, has yielded to treatment. The coefficients of viscosity defined by Poisson (1831), Maxwell (1868), Hagenbach (1860), O. E. Meyer (1863), are exhaustively investigated for gases and for liquids. Maxwell (1877) has given the most suggestive and Boltzmann (1876) the most carefully formulated theory for solids, but the investigation of absolute data has but begun. The difficulty of reconciling viscous flow with Lagrange's dynamics seems first to have been adjusted by Navier.

Aeromechanics

Aerostatics is indissolubly linked with thermodynamics. Aerodynamics has not marked out for itself any very definite line of progress. Though the resistance of oblique planes has engaged the attention of Rayleigh, it is chiefly on the experimental side that the subject has been enriched, as, for instance, by the labors of Langley (1891) and Lilienthal. Langley (1897) has, indeed, constructed a steam-propelled aeroplane which flew successfully; but man himself has not yet flown.

Moreover, the meteorological applications of aerodynamics contained in the profound researches of Guldberg and Mohn (1877), Ferrel (1877), Oberbeck (1882, 1886), Helmholtz (1888, 1889), and others, as well as in such investigations as Sprung's (1880) on the inertia path, are as yet rather qualitative in their bearing on the actual motions of the atmosphere. The marked progress of meteorology is observational in character.

Acoustics

Early in the century the velocity of sound given in a famous equation of Newton was corrected to agree with observation by Laplace (1816).

The great problems in acoustics are addressed in part to the elastician, in part to the physiologist. In the former case the work of Rayleigh (1877) has described the present stage of development, interpreting and enriching almost every part discussed. In the latter case Helmholtz (1863) has devoted his immense powers to a like purpose and with like success. König has been prominently concerned with the construction of accurate acoustic apparatus.

It is interesting to note that the differential equation representing the vibration of strings was the first to be integrated; that it passed from D'Alembert (1747) successively to Euler (1779), Bernoulli (1753) and Lagrange (1759). With the introduction of Fourier's series (1807) and of spherical harmonics at the very beginning of the century, D'Alembert's and the other corresponding equations in acoustics readily yielded to rigorous analysis. Rayleigh's first six chapters summarize the results for one and for two degrees of freedom.

Flexural vibration in rods, membranes, and plates become prominent in the unique investigations of Chladni (1787, 1796, *Akustik*, 1802). The behavior of vibrating rods has been developed by Euler (1779), Cauchy (1827), Poisson (1833), Strehlke (1833), Lissajous (1833), Seebeck (1849), and is summarized in the seventh and eighth chapters of Rayleigh's book. The transverse vibration of membranes engaged the attention of Poisson (1829). Round membranes were rigorously treated by Kirchhoff (1850) and by Clebsch (1862); elliptic membranes by Mathieu (1868). The problem of vibrating plates presents formidable difficulties resulting not only from the edge conditions, but from the underlying differential equation of the fourth degree due to Sophie Germain (1810) and to Lagrange (1811). The solutions have taxed the powers of Poisson (1812, 1829), Cauchy (1829), Kirchhoff (1850), Boussinesq (1871–79), and others. For the circular plate Kirchhoff gave the complete theory. Rayleigh systematized the results for the quadratic plate, and the general account makes up his ninth and tenth chapters.

Longitudinal vibrations, which are of particular importance in case of the organ-pipe, were considered in succession by Poisson (1817), Hopkins (1838), Quet (1855); but Helmholtz in his famous paper of 1860 gave the first adequate theory of the open organ-pipe, involving viscosity. Further extension was then added by Kirchhoff (1868), and by Rayleigh (1870, *et seq.*), including particularly powerful analysis of resonance. The subject in its entirety, including the allied treatment of the resonator, completes the second volume of Rayleigh's *Sound.*

On the other hand, the whole subject of tone-quality, of combination and difference tones, of speech, of harmony, in its physical, physiological, and æsthetic relations, has been reconstructed, using all the work of earlier investigators, by Helmholtz (1862), in his masterly *Tonempfindungen.* With rare skill and devotion König contributed a wealth of siren-like experimental appurtenances.

Acousticians have been fertile in devising ingenious methods and apparatus, among which the tuning-fork with resonator of Marloye, the siren of Cagniard de la Tour (1819), the Lissajous curves (1857), the stroboscope of Plateau (1832), the manometric flames of König (1862, 1872), the dust methods of Chladni (1787) and of Kundt (1865–68), Melde's vibrating strings (1860, 1864), the phonograph of Edison and of Bell (1877), are among the more famous.

Heat: Thermometry

The invention of the air thermometer dates back at least to Amontons (1699), but it was not until Rudberg (1837), and more thoroughly Regnault (1841, *et seq.*) and Magnus (1842), had completed their work on the thermal expansion and compressibility of air, that air thermometry became adequately rigorous. On the theoretical side Clapeyron (1834), Helmholtz (1847), Joule (1848), had in various ways proposed the use of the Carnot function (1894) for temperature measurement, but the subject was finally disposed of by Kelvin (1849, *et seq.*) in his series of papers on temperature and temperature measurement.

Practical thermometry gained much from the measurement of the expansion of mercury by Dulong and Petit (1818), repeated by Regnault. It also profited by the determination of the viscous behavior of glass, due to Pernet (1876) and others, but more from the elimination of these errors by the invention of the Jena glass.

It is significant to note that the broad question of thermal expansion has yet no adequate equation, though much has been done experimentally for fluids by the magnificent work of Amagat (1869, 1873, *et seq.*).

Heat Conduction

The subject of heat conduction from a theoretical point of view was virtually created by the great memoir of Fourier (1822), which shed its first light here, but subsequently illumined almost the whole of physics. The treatment passed successively through the hands of many of the foremost thinkers, notably of Poisson (1835, 1837), Lamé (1836, 1839, 1843), Kelvin (1841–44), and others. With the latter (1856) the ingenious method of sources and sinks originated. The character of the conduction is now well known for continuous media, isotropic or not, bounded by the more simple geometrical forms, in particular for the sphere under all reasonable initial and surface conditions. Much attention has been given to the heat conduction of the earth, following Fourier, by Kelvin (1862, 1878), King (1893), and others.

Experimentally, Wiedemann and Franz (1853) determined the relative heat conduction of metals and showed that for simple bodies a parallel gradation exists for the cases of heat and of electrical conductivity. Noteworthy absolute methods for measuring heat conduction were devised in particular by Forbes (1842), F. Neumann (1862), Ångström (1861–64), and a lamellar method applying to fluids by H. F. Weber (1880).

Calorimetry

Practical calorimetry was virtually completed by the researches of Black in 1763. A rich harvest of experimental results, therefore, has since accrued to the subjects of specific, latent, and chemical heats, due in particularly important cases to the indefatigable Regnault (1840, 1845, *et seq.*). Dulong and Petit (1819) discovered the remarkable fact of the approximate constancy of the atomic heats of the elements. The apparently exceptional cases were interpreted for carbon silicon and boron by H. F. Weber (1875), and for sulphur by Regnault (1840). F. Neumann (1831) extended the law to compound bodies, and Joule (1844) showed that in many cases specific heat could be treated as additively related to the component specific heats.

Among recent apparatus the invention of Bunsen's ice calorimeter (1870) deserves particular mention.

Thermodynamics

Thermodynamics, as has been stated, in a singularly fruitful way interpreted and broadened the old Leibnitzian principle of *vis viva* of 1686. Beginning with the incidental experiments of Rumford (1798) and of Davy (1799) just antedating the century, the new conception almost leaped into being when J. R. Mayer (1842, 1845) defined and computed the mechanical equivalent of heat, and when Joule (1843, 1845, *et seq.*) made that series of precise and judiciously varied measurements which mark an epoch. Shortly after Helmholtz (1847), transcending the mere bounds of heat, carried the doctrine of the conservation of energy throughout the whole of physics.

Earlier in the century Carnot (1824), stimulated by the growing importance of the steam engine of Watt (1763, *et seq.*), which Fulton (1806) had already applied to transportation by water and which Stephenson (1829) soon after applied to transportation by land, invented the reversible thermodynamic cycle. This cycle or sequence of states of equilibrium of two bodies in mutual action is, perhaps, without a parallel in the prolific fruitfulness of its contributions to modern physics. Its continued use in fifty years of research has but sharpened its logical edge. Carnot deduced the startling doctrine of a temperature criterion for the efficiency of engines. Clapeyron (1834) then gave the geometrical method of representation universally used in thermodynamic discussions to-day, though often made more flexible by new coördinates as suggested by Gibbs (1873).

To bring the ideas of Carnot into harmony with the first law of thermodynamics it is necessary to define the value of a transformation, and this was the great work of Clausius (1850), followed very closely by Kelvin (1851) and more hypothetically by Rankine (1851). The latter's broad treatment of energetics (1855) antedates many recent discussions. As early as 1858 Kirchhoff investigated the solution of solids and of gases thermodynamically, introducing at the same time an original method of treatment.

The second law was not generally accepted without grave misgiving. Clausius, indeed, succeeded in surmounting most of the objections, even those contained in theoretically delicate problems associated with radiation. Nevertheless, the confusion raised by the invocation of Maxwell's "demon" has never quite been calmed; and while Boltzmann (1877, 1878) refers to the second law as a case of probability, Helmholtz (1882) admits that the law is an expression of our inability to deal with the individual atom. Irreversible processes as yet lie quite beyond the pale of thermodynamics. For these the famous inequality of Clausius is the only refuge. The value of an uncompensated transformation is always positive.

The invention of mechanical systems which more or less fully conform to the second law has not been infrequent. Ideas of this nature have been put forward by Boltzmann (1866, 1872), by Clausius (1870, 1871), and more powerfully by Helmholtz (1884) in his theory of cyclic systems, which in a measure suggested the hidden mechanism at the root of Hertz's dynamics. Gibbs's (1902) elementary principles of statistical mechanics seem, however, to contain the nearest approach to a logical justification of the second law — an approach which is more than a dynamical illustration.

The applications of the first and second laws of thermodynamics are ubiquitous. As interesting instances we may mention the conception of an ideal gas and its properties; the departure of physical gases from ideality as shown in Kelvin and Joule's plug experiment (1854, 1862); the corrected temperature scale resulting on the one hand, and the possibility of the modern liquid air refrigerator of Linde and Hampson (1895) on the other. Difficulties encountered in the liquefaction of incoercible gases by Cailletet and Pictet (1877) have vanished even from the hydrogen coercions of Olezewski (1895) and of Dewar and Travers.

Again, the broad treatment of fusion and evaporation, beginning with James Thomson's (1849) computation of the melting point of ice under pressure, Kirchhoff's (1858) treatment of sublimation, the extensive chapter of thermo-elastics set on foot by Kelvin's (1883) equation, are further examples.

To these must be added Andrews's (1869) discovery of the continuity of the liquid and the gaseous states foreshadowed by Cagniard de la Tour (1822, 1823); the deep insight into the laws of physical gases furnished by the experimental prowess of Amagat (1881, 1893, 1896), and the remarkably close approximation amounting almost to a prediction of the facts observed which is given by the great work of van der Waals (1873).

The further development of thermodynamics, remarkable for the breadth, not to say audacity, of its generalizations, was to take place in connection with chemical systems. The analytical power of the conception of a thermodynamic potential was recognized nearly at the same time by many thinkers:[1] by Gibbs (1876), who discovered both the isothermal and the adiabatic potential; by Massieu (1877), independently in his *Fonctions characteristiques ;* by Helmholtz (1882), in his *Freie Energie;* by Duhem (1886) and by Planck (1887, 1891), in their respective thermodynamic potentials. The transformation of Lagrange's doctrine of virtual displacements of infinitely more complicated systems than those originally contemplated, in other words the introduction of a virtual thermodynamic modification in complete analogy with the virtual displacement of the *mécanique analytique,* marked a new possibility of research of which Gibbs made the profoundest use. Unaware of this marshaling of powerful mathematical forces, van 't Hoff (1886, 1888) consummated his marvelously simple application of the second law; and from interpretations of the experiments of Pfeffer (1877) and of Raoult (1883, 1887) propounded a new theory of solution, indeed, a basis for chemical physics, in a form at once available for experimental investigation.

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[1] Maxwell's *available energy* is accidentally overlooked in the text.

The highly generalized treatment of chemical statics by Gibbs bore early fruit in its application to Deville's phenomenon of dissociation (1857), and in succession Gibbs (1878, 1879), Duhem (1886), Planck (1887), have deduced adequate equations, while the latter in case of dilute solutions gave a theoretical basis for Guldberg and Waage's law of mass action (1879). An earlier independent treatment of dissociation is due to Horstmann (1869, 1873).

In comparison with the brilliant advance of chemical statics which followed Gibbs, the progress of chemical dynamics has been less obvious; but the outlines of the subject have, nevertheless, been succinctly drawn in a profound paper by Helmholtz (1886), followed with much skill by Duhem (1894, 1896) and Natanson (1896).

Kinetic Theory of Gases

The kinetic theory of gases at the outset, and as suggested by Herapath (1821), Joule (1851, 1857), Krönig (1856), virtually reaffirmed the classic treatise of Bernoulli (1738). Clausius in 1857–62 gave to the theory a modern aspect in his derivation of Boyle's law in its thermal relations, of molecular velocity and of the ratio of translational to total energy. He also introduced the mean free path (1858). Closely after followed Maxwell (1860), adducing the law for the distribution of velocity among molecules, later critically and elaborately examined by Boltzmann (1868–81). Nevertheless, the difficulties relating to the partition of energy have not yet been surmounted. The subject is still under vigorous discussion, as the papers of Burbury (1899) and others testify.

To Maxwell (1860, 1868) is due the specifically kinetic interpretation of viscosity, of diffusion, of heat conduction, subjects which also engaged the attention of Boltzmann (1872–87). Rigorous data for molecular velocity and mean free path have thus become available, and van der Waals (1873) added a final allowance for the size of the molecules. Less satisfactory has been the exploration of the character of molecular force for which Maxwell, Boltzmann (1872, *et seq.*), Sutherland (1886, 1893), and others have put forward tentative investigations.

The intrinsic equation of fluids discovered and treated in the great paper of van der Waals (1873), though partaking of the character of a first approximation, has greatly promoted the coördination of most of the known facts. Corresponding states, the thermal coefficients, the vapor pressure relation, the minimum of pressure-volume products, and even molecular diameters, are reasonably inferred by van der Waals from very simple premises. Many of the results have been tested by Amagat (1896).

The data for molecular diameter furnished by the kinetic theory as a whole, viz., the original values of Loschmidt (1865), of van der Waals (1873), and others, are of the same order of values as Kelvin's estimates (1883) from capillarity and contact electricity. Many converging lines of evidence show that an approximation to the truth has surely been reached.

Radiation

Our knowledge of the radiation of heat, diathermacy, thermocrosis, was promoted by the perfection which the thermopyle reached in the hands of Melloni (1835–53). These and other researches set at rest forever all questions relating to the identity of heat and light. The subject was, however, destined to attain a much higher order of precision with the invention of Langley's bolometer (1881). The survey of heat spectra, beginning with the laborious attempts of Herschel (1840), of E. Becquerel (1843, 1870), H. Becquerel (1883), and others, has thus culminated in the magnificent development shown in Langley's charts (1883, 1884, *et seq.*).

Kirchhoff's law (1860), to some extent anticipated by Stewart (1857, 1858), pervades the whole subject. The radiation of the black body, tentatively formulated in relation to temperature by Stefan (1879) and more rigorously by Boltzmann (1884), has furnished the savants of the Reichsanstalt with means for the development of a new pyrometry whose upper limit is not in sight.

Among curious inventions Crooke's radiometer (1874) and Bell's photophone may be cited. The adaptation of the former in case of high exhaustion to the actual measurement of Maxwell's (1873) light pressure by Lebedew (1901) and Nichols and Hull (1903) is of quite recent history.

The first estimate of the important constant of solar radiation at the earth was made by Pouillet (1838); but other pyrheliometric methods have since been devised by Langley (1884) and more recently by Ångström (1886, *et seq.*).

Velocity of light

Data for the velocity of light, verified by independent astronomical observations, were well known prior to the century; for Römer had worked as long ago as 1675, and Bradley in 1727. It remained to actually measure this enormous velocity in the laboratory, apparently an extraordinary feat, but accomplished simultaneously by Fizeau (1849) and by the aid of Wheatstone's revolving mirror (1834) by Foucault (1849, 1850, 1862). Since that time precision has been given to this important constant by Cornu (1871, 1873, 1874), Forbes and Young (1882), Michelson (1878, *et seq.*), and Newcomb (1885). Foucault (1850), and more accurately Michelson (1884), determined the variation of velocity with the medium and wave-length, thus assuring to the undulatory theory its ultimate triumph. Grave concern, however, still exists, inasmuch as Michelson and Morley (1886) by the most refined measurement, and differing from the older observations of Fizeau (1851, 1859), were unable to detect the optical effect of the relative motion of the atmosphere and the luminiferous ether predicted by theory.

Römer's observation may in some degree be considered as an anticipation of the principle first clearly stated by Döppler (1842), which has since become invaluable in spectroscopy. Estimates of the density of the luminiferous ether have been published, in particular by Kelvin (1854).

Geometric optics

Prior to the nineteenth century geometric optics, having been mustered before Huyghens (1690), Newton (1704), Malus (1808), Lagrange (1778, 1803), and others, had naturally attained a high order of development. It was, nevertheless, remodeled by the great paper of Gauss (1841), and was thereafter generalized step by step by Listing, Möbius (1855), and particularly by Abbe (1872), postulating that in character, the cardinal elements are independent of the physical reasons by which one region is imaged in another.

So many able thinkers, like Airy (1827), Maxwell (1856, *et seq.*), Bessel (1840, 1841), Helmholtz (1856, 1867), Ferraris (1877, 1880), and others have contributed to the furtherance of geometric optics, that definite mention is impossible. In other cases, again, profound methods like those of Hamilton (1828, *et seq.*), Kummer (1859), do not seem to have borne correspondingly obvious fruit. The fundamental bearing of diffraction on geometric optics was first pointed out by Airy (1838), but developed by Abbe (1873), and after him by Rayleigh (1879). An adequate theory of the rainbow, due to Airy and others, is one of its picturesque accomplishments (1838).

The so-called astronomical refraction of a medium of continuously varying index, successively treated by Bouguer (1739, 1749), Simpson (1743), Bradley (1750, 1762), owes its recent refined development to Bessel (1823, 1826, 1842), Ivory (1822, 1823, *et seq.*), Radau (1884), and others. Tait (1883) gave much attention to the allied treatment of mirage.

In relation to instruments the conditions of aplantism were examined by Clausius (1864), by Helmholtz (1874), by Abbe (1873, *et seq.*), by Hockin (1884), and others, and the apochromatic lens was introduced by Abbe (1879). The microscope is still well subserved by either the Huyghens or the Ramsden (1873) eye-piece, but the objective has undergone successive stages of improvement, beginning with Lister's discovery in 1830. Amici (1840) introduced the principle of immersion; Stephenson (1878) and Abbe (1879), homogeneous immersion; and the Abbe-Zeiss apochromatic objective (1886), the outcome of the Jena-glass experiments, marks, perhaps, the high-water mark of the art for the microscope. Steinheil (1865, 1866) introduced the guiding principle for photographic objectives. Alvan Clark carried the difficult technique of telescope lens construction to a degree of astonishing excellence.

Spectrum — Dispersion

Curiously, the acumen of Newton (1666, 1704) stopped short of the ultimate conditions of purity of spectrum. It was left to Wollaston (1802), about one hundred years later, to introduce the slit and observe the dark lines of the solar spectrum. Fraunhofer (1814, 1815, 1823) mapped them out carefully and insisted on their solar origin. Brewster (1833, 1834), who afterwards (1860) published a map of 3000 lines, was the first to lay stress on the occurrence of absorption, believing it to be atmospheric. Forbes (1836) gave even greater definiteness to absorption by referring it to solar origin. Foucault (1849) pointed out the coincidence of the sodium lines with the D group of Fraunhofer, and discovered the reversing effect of sodium vapor. A statement of the parallelism of emission and absorption came from Ångström (1855) and with greater definiteness and ingenious experiments from Stewart (1860). Nevertheless, it was reserved to Kirchhoff and Bunsen (1860, 1861) to give the clear-cut distinctions between the continuous spectra and the characteristically fixed bright-line or dark-line spectra upon which spectrum analysis depends. Kirchhoff's law was announced in 1861, and the same year brought his map of the solar spectrum and a discussion of the chemical composition of the sun. Huggins (1864, *et seq.*), Ångström (1868), Thalén (1875), followed with improved observations on the distribution and wave-length of the solar lines; but the work of these and other observers was suddenly overshadowed by the marvelous possibilities of the Rowland concave grating (1882, *et seq.*). Rowland's maps and tables of the solar spectrum as they appeared in 1887, 1889, *et seq.*, his summary of the elements contained in the sun (1891), each marked a definite stage of advance of the subject. Mitscherlich (1862, 1863) probably was the first to recognize the banded or channeled spectra of compound bodies. Balmer (1885) constructed a valuable equation for recognizing the distribution of single types of lines. Kayser and Runge (1887, *et seq.*) successfully analyzed the structure of the spectra of alkaline and other elements.

The modernized theory of the grating had been given by Rayleigh in 1874 and was extended to the concave grating by Rowland (1892, 1893) and others. A general theory of the resolving power of prismatic systems is also due to Rayleigh (1879, 1880), and another to Thollon (1881).

The work of Rowland for the visible spectrum was ably paralleled by Langley's investigations (1883 *et seq.*) of the infra-red, dating from the invention of the bolometer (1881). Superseding the work of earlier investigators like Fizeau and Foucault (1878) and others, Langley extended the spectrum with detailed accuracy to over eight times its visible length. The solar and the lunar spectrum, the radiations of incandescent and of hot bodies, were all specified absolutely and with precision. With artificial spectra Rubens (1892, 1899) has since gone further, reaching the longest heat-waves known.

A similarly remarkable extension was added for the ultra-violet by Schumann (1890, 1892), contending successfully with the gradually increasing opacity of all known media.

Experimentally the suggestion of the spectroheliograph by Lockyer (1868) and by Janssen (1868) and its brilliant achievement by Hale (1892) promise notable additions to our knowledge of solar activity.

Finally, the refractions of absorbing media have been of great importance in their bearing on theory. The peculiarities of metallic reflection were announced from his earlier experiments (1811) by Arago in 1817 and more fully investigated by Brewster (1815, 1830, 1831). F. Neumann (1832) and MacCullagh (1837) gave sharper statements to these phenomena. Equations were advanced by Cauchy (1836, *et seq.*) for isotropic bodies, and later with greater detail by Rayleigh (1872), Ketteler (1875, *et seq.*), Drude (1887, *et seq.*), and others. Jamin (1847, 1848) devised the first experiments of requisite precision and found them in close agreement with Cauchy's theory. Kundt (1888) more recently investigated the refraction of metallic prisms.

Anomalous dispersion was discovered by Christiansen in 1870, and studied by Kundt (1871, *et seq.*). Sellmeyer's (1872) powerful and flexible theory of dispersion was extended to include absorption effects by Helmholtz (1874), with greater detail by Ketteler (1879, *et seq.*), and from a different point of view by Kelvin (1885).

The electromagnetic theory lends itself particularly well to the same phenomena, and Kolázek (1887, 1888), Goldhammer (1892), Helmholtz (1892), Drude (1893), and others instanced its adaptation with success.

Photometry, Fluorescence, Photochemistry

The cosine law of Lambert (1760) has since been interpreted in a way satisfying modern requirements by Fourier (1817, 1824) and by Lommel (1880). Among new resources for the experimentalist the spectrophotometer, the Lummer-Brodhun photometer (1889), and Rood's flicker photometer (1893, 1899), should be mentioned.

Fluorescence, though ingeniously treated by Herschel (1845, 1853) and Brewster (1846, *et seq.*), was virtually created in its philosophical aspects by Stokes in his great papers (1852, *et seq.*) on the subject. In recent years Lommel (1877) made noteworthy contributions. Phosphorescence has engaged the attention of E. Becquerel (1859), among others.

The laws of photochemistry are in large measure due to Bunsen and Roscoe (1857, 1862). The practical development of photography from its beginnings with Daguerre (1829, 1838) and Niépce and Fox-Talbot (1839), to its final improvement by Maddox (1871) with the introduction of the dry plate, is familiar to all. Vogel's (1873) discovery of appropriate sensitizers for different colors has added new resources to the already invaluable application of photography to spectroscopy.

Interference

The colors of thin plates treated successively by Boyle (1663), Hooke (1665), and more particularly by Newton (1672, *Optiks*, 1704), became in the hands of Young (1802) the means of framing an adequate theory of light. Young also discovered the colors of mixed plates and was cognizant of loss of half a wave-length on reflection from the denser medium. Fresnel (1815) gave an independent explanation of Newton's colors in terms of interference, devising for further evidence his double mirrors (1816), his biprism (1819), and eventually the triple mirror (1820). Billet's plates and split lens (1858) belong to the same classical order, as do also Lloyd's (1837) and Haidinger's (1849) interferences. Brewster's (1817) observation of interference in case of thick plates culminated in the hands of Jamin (1856, 1857) in the useful interferometer. The scope of this apparatus was immensely advanced by the famous device of Michelson (1881, 1882), which has now become a fundamental instrument of research. Michelson's determination of the length of the meter in terms of the wave-length of light with astounding accuracy is a mere example of its accomplishments.

Wiener (1890) in his discovery of the stationary light-wave introduced an entirely new interference phenomenon. The method was successfully applied to color photography by Lippmann (1891, 1892), showing that the electric and not the magnetic vector is photographically active.

The theory of interferences from a broader point of view, and including the occurrence of multiple reflections, was successively perfected by Poisson (1823), Fresnel (1823), Airy (1831). It has recently been further advanced by Feussner (1880, *et seq.*), Sohncke and Wangerin (1881, 1883), Rayleigh (1889), and others. The interferences along a caustic were treated by Airy (1836), but the endeavor to reconstruct geometric optics on a diffraction basis has as yet only succeeded in certain important instances, as already mentioned.

Diffraction

Though diffraction dates back to Grimaldi (1665) and was well known to Newton (1704), the first correct though crude interpretation of the phenomenon is due to Young (1802, 1804). Independently Fresnel (1815) in his original work devised similar explanations, but later (1818, 1819, 1826) gave a more rational theory in terms of Huyghens's principle, which he was the first adequately to interpret. Fresnel showed that all points of a wave-front are concerned in producing diffraction, though the ultimate critical analysis was left to Stokes (1849).

In 1822 Fraunhofer published his remarkable paper, in which, among other inventions, he introduced the grating into science. Zone plates were studied by Cornu (1875) and by Soret (1875). Rowland's concave grating appeared in 1881; Michelson's echelon spectrometer in 1899.

The theory of gratings and other diffraction phenomena was exhaustively treated by Schwerd (1837). Babinet established the principle bearing his name in 1837. Subsequent developments were in part concerned with the improvement of Fresnel's method of computation, in part with a more rigorous treatment of the theory of diffraction. Stokes (1850, 1852) gave the first account of the polarization accompanying diffraction, and thereafter Rayleigh (1871) and many others, including Kirchhoff (1882, 1883), profoundly modified the classic treatment. Airy (1834, 1838) and others elaborately examined the diffraction due to a point source in view of its important bearing on the efficiency of optical instruments.

A unique development of diffraction is the phenomenon of scattering propounded by Rayleigh (1871) in his dynamics of the blue sky. This great theory which Rayleigh has repeatedly improved (1881, *et seq.*) has since superseded all other relevant explanations.

Polarization

An infinite variety of polarization phenomena grew out of Bartholinus's (1670) discovery. Sound beginnings of a theory were laid by Huyghens (*Traité*, 1690), whose wavelet principle and elementary wave-front have persisted as an invaluable acquisition, to be generalized by Fresnel in 1821.

Fresh foundations in this department of optics were laid by Malus (1810) in his discovery of the cosine law and the further discovery of the polarization of reflected light. Later (1815) Brewster adduced the conditions of maximum polarization for this case.

In 1811 Arago announced the occurrence of interferences in connection with parallel plane-polarized light, phenomena which under the observations of Arago and Fresnel (1816, 1819), Biot (1816), Brewster (1813, 1814, 1818), and others grew immensely in variety, and in the importance of their bearing on the undulatory theory. It is on the basis of these phenomena that Fresnel in 1819 insisted on the transversality of light-waves, offering proof which was subsequently made rigorous by Verdet (1850). Though a tentative explanation was here again given by Young (1814), the first adequate theory of the behavior of thin plates of æolotropic media with polarized light came from Fresnel (1821).

Airy (1833) elucidated a special case of the gorgeously complicated interferences obtained with convergent pencils; Neumann in 1834 gave the general theory. The forbidding equations resulting were geometrically interpreted by Bertin (1861, 1884), and Lommel (1883) and Neumann (1841) added a theory for stressed media, afterwards improved by Pockels (1889).

The peculiarly undulatory character of natural light owes its explanation largely to Stokes (1852), and his views were verified by many physicists, notably by Fizeau (1862) showing interferences for path differences of 50,000 wave-lengths, and by Michelson for much larger path differences.

The occurrence of double refraction in all non-regular crystals was recognized by Haüy (1788) and studied by Brewster (1818). In 1821, largely by a feat of intuition, Fresnel introduced his generalized elementary wave-surface, and the correctness of his explanation has since been substantiated by a host of observers. Stokes (1862, *et seq.*) was unremittingly active in pointing out the theoretical bearing of the results obtained. Hamilton (1832) supplied a remarkable criterion of the truth of Fresnel's theory deductively, in the prediction of both types of conic refraction. The phenomena were detected experimentally by Lloyd (1833).

The domain of natural rotary polarization, discovered by Arago (1811) and enlarged by Biot (1815), has recently been placed in close relation to non-symmetrical chemical structure by LeBel (1874) and van 't Hoff (1875), and a tentative molecular theory was advanced by Sohncke (1876).

Boussinesq (1868) adapted Cauchy's theory (1842) to these phenomena. Independent elastic theories were propounded by MacCullagh (1837), Briot, Sarrau (1868); but there is naturally no difficulty in accounting for rotary polarization by the electromagnetic theory of light, as was shown by Drude (1892).

Among investigational apparatus of great importance the Soleil (1846, 1847) saccharimeter may be mentioned.

Theories

In conclusion, a brief summary may be given of the chief mechanisms proposed to account for the undulations of light. Fresnel suggested the first adequate optical theory in 1821, which, though singularly correct in its bearing on reflection and refraction in the widest sense, was merely tentative in construction. Cauchy (1829) proposed a specifically elastic theory for the motion of relatively long waves of light in continuous media, based on a reasonable hypothesis of molecular force, and deduced therefrom Fresnel's reflection and refraction equations. Green (1838), ignoring molecular forces and proceeding in accordance with his own method in elastics, published a different theory, which did not, however, lead to Fresnel's equations. Kelvin (1888) found the conditions implied in Cauchy's theory compatible with stability if the ether were considered as bound by a rigid medium. The ether implied throughout is to have the same elasticity everywhere, but to vary in density from medium to medium, and vibration to be normal to the plane of polarization.

Neumann (1835), whose work has been reconstructed by Kirchhoff (1876), and MacCullagh (1837), with the counter-hypothesis of an ether of fixed density but varying in elasticity from medium to medium, also deduced Fresnel's equations, obtaining at the same time better surface conditions in the case of æolotropic media. The vibrations are in the plane of polarization.

All the elastic theories essentially predict a longitudinal light-wave. It was not until Kelvin in 1889–90 proposed his remarkable gyrostatic theory of light, in which force and displacement become torque and twist, that these objections to the elastic theory were wholly removed. MacCullagh, without recognizing their bearing, seems actually to have anticipated Kelvin's equation.

With the purpose of accounting for dispersion, Cauchy in 1835 gave greater breadth to his theory by postulating a sphere of action of ether particles commensurate with wave-length, and in this direction he was followed by F. Neumann (1841), Briot (1864), Rayleigh (1871), and others, treating an ether variously loaded with material particles. Among theories beginning with the phenomena observed, that of Boussinesq (1867, *et seq.*) has received the most extensive development.

The difficult surface conditions met with when light passes from one medium to another, including such subjects as ellipticity, total reflection, etc., have been critically discussed, among others, by Neumann (1835) and Rayleigh (1888); but the discrimination between the Fresnel and the Neumann vector was not accomplished without misgiving before the advent of the work of Hertz.

It appears, therefore, that the elastic theories of light, if Kelvin's gyrostatic adynamic ether be admitted, have not been wholly routed. Nevertheless, the great electromagnetic theory of light propounded by Maxwell (1864, *Treatise*, 1873) has been singularly apt not only in explaining all the phenomena reached by the older theories and in predicting entirely novel results, but in harmoniously uniting, as parts of a unique doctrine, both the electric or photographic light vector of Fresnel and Cauchy and the magnetic vector of Neumann and MacCullagh. Its predictions have, moreover, been astonishingly verified by the work of Hertz (1890), and it is to-day acquiring added power in the convection theories of Lorentz (1895) and others.

Electrostatics

Coulomb's (1785) law antedates the century; indeed, it was known to Cavendish (1771, 1781). Problems of electric distribution were not seriously approached, however, until Poisson (1811) solved the case for spheres in contact. Afterwards Clausius (1852), Helmholtz (1868), and Kirchhoff (1877) examined the conditions for discs, the last giving the first rigorous theory of the experimentally important plate-condenser. In 1845–48 the investigation of electric distribution received new incentive as an application of Kelvin's beautiful method of images. Maxwell (*Treatise*, 1873) systematized the treatment of capacity and induction coefficients.

Riess (1837), in a classic series of experiments on the heat produced by electrostatic discharge, virtually deduced the potential energy of a conductor and in a measure anticipated Joule's law (1841). In 1860 appeared Kelvin's great paper on the electromotive force needed to produce a spark. As early as 1855, however, he had shown that the spark discharge is liable to be of the character of a damped vibration and the theory of electric oscillation was subsequently extended by Kirchhoff (1867). The first adequate experimental verification was due to Feddersen (1858, 1861).

The specific inductive capacity of a medium with its fundamental bearing on the character of electric force was discovered by Faraday in 1837. Of the theories propounded to account for this property the most far-reaching is Maxwell's (1865), which culminates in the unique result showing that the refraction index of a medium is the square root of its specific inductive capacity. With regard to Maxwell's theory of the Faraday stress in the ether as compared with the subsequent development of electrostriction in other media by many authors, notably by Boltzmann (1880) and by Kirchhoff (1885), it is observable that the tendency of the former to assign concrete physical properties to the tube of force is growing, particularly in connection with radioactivity. Duhem (1892, 1895) insists, however, on the greater trustworthiness of the thermodynamic potential.

The seemingly trivial subject of pyroelectricity interpreted by Æpinus (1756) and studied by Brewster (1825), has none the less elicited much discussion and curiosity, a vast number of data by Hankel (1839–93) and others, and a succinct explanation by Kelvin (1860, 1878). Similarly piezoelectricity, discovered by the brothers Curie (1880), has been made the subject of a searching investigation by Voigt (1890). Finally Kerr (1875, *et seq.*) observed the occurrence of double refraction in an electrically polarized medium. Recent researches, among which those of Lemoine (1896) are most accurate, have determined the phase difference corresponding to the Kerr effect under normal conditions, while Voigt (1899) has adduced an adequate theory.

Certain electrostatic inventions have had a marked bearing on the development of electricity. We may mention in particular Kelvin's quadrant electrometer (1867) and Lippmann's capillary electrometer (1873). Moreover, among apparatus originating in Nicholson's duplicator (1788) and Volta's electrophorus, the Töpler-Holtz machine (1865–67), with the recent improvement due to Wimshurst, has replaced all others. Atmospheric electricity, after the memorable experiment of Franklin (1751), made little progress until Kelvin (1860) organized a systematic attack. More recently a revival of interest began with Exner (1886), but more particularly with Linss (1887), who insisted on the fundamental importance of a detailed knowledge of atmospheric conduction. It is in this direction that the recent vigorous treatment of the atmosphere as an ionized medium has progressed, owing chiefly to the indefatigable devotion of Elster and Geitel (1899, *et seq.*) and of C. T. R. Wilson (1897, *et seq.*). Qualitatively the main phenomena of atmospheric electricity are now plausibly accounted for; quantitatively there is as yet very little specific information.

Volta Contacts

Volta's epoch-making experiment of 1797 may well be added to the century which made such prolific use of it; indeed, the Voltaic pile (1800–02) and Volta's law of series (1802) come just within it. Among the innumerable relevant experiments Kelvin's dropping electrodes (1859) and his funnel experiment (1867) are among the more interesting, while the *Spannungsreihe* of R. Kohlrausch (1851, 1853) is the first adequate investigation. Nevertheless, the phenomenon has remained without a universally acceptable explanation until the present day, when it is reluctantly yielding to electronic theory, although ingenious suggestions like Helmholtz's *Doppelschicht* (1879), the interpretations of physical chemistry and the discovery of the concentration cell (Helmholtz; Nernst, 1888, 1889; Planck, 1890) have thrown light upon it.

Among the earliest theories of the galvanic cell is Kelvin's (1851, 1860), which, like Helmholtz's, is incomplete. The most satisfactory theory is Nernst's (1889). Gibbs (1878) and Helmholtz (1882) have made searching critical contributions, chiefly in relation to the thermal phenomena.

Volta's invention was made practically efficient in certain famous galvanic cells, among which Daniell's (1836), Grove's (1839), Clarke's (1878), deserve mention, and the purposes of measurement have been subserved by the potentiometers of Poggendorff (1841), Bosscha (1855), Clarke (1873).

Seebeck Contacts

Thermoelectricity, destined to advance many departments of physics, was discovered by Seebeck in 1821. The Peltier effect followed in 1834, subsequently to be interpreted by Icilius (1853). A thermodynamic theory of the phenomena came from Clausius (1853) and with greater elaboration, together with the discovery of the Thomson effect, from Kelvin (1854, 1856), to whom the thermoelectric diagram is due. This was subsequently developed by Tait (1872, *et seq.*) and his pupils. Avenarius (1863), however, first observed the thermoelectric parabola.

The modern platinum-iridium or platinum-rhodium thermoelectric pyrometer dates from about 1885 and has recently been perfected at the Reichsanstalt. Melloni (1835, *et seq.*) made the most efficient use of the thermopyle in detecting minute temperature differences.

Electrolysis

Though recognized by Nichols and Carlisle (1800) early in the century, the laws of electrolysis awaited the discovery of Faraday

(1834). Again, it was not till 1853 that further marked advances were made by Hittorf's (1853–59) strikingly original researches on the motions of the ions. Later Clausius (1857) suggested an adequate theory of electrolysis, which was subsequently to be specialized in the dissociation hypothesis of Arrhenius (1881, 1884). To the elaborate investigations of F. Kohlrausch (1879, *et seq.*), however, science owes the fundamental law of the independent velocities of migration of the ions.

Polarization discovered by Ritter in 1803 became in the hands of Planté (1859–1879) an invaluable means for the storage of energy, an application which was further improved by Faure (1880).

Steady Flow

The fundamental law of the steady flow of electricity, in spite of its simplicity, proved to be peculiarly elusive. True, Cavendish (1771–81) had definite notions of electrostatic resistance as dependent on length section and potential, but his intuitions were lost to the world. Davy (1820), from his experiments on the resistances of conductors, seems to have arrived at the law of sections, though he obscured it in a misleading statement. Barlow (1825) and Becquerel (1825–26), the latter operating with the ingenious differential galvanometer of his own invention, were not more definite. Surface effects were frequently suspected. Ohm himself, in his first paper (1825), confused resistance with the polarization of his battery, and it was not till the next year (1826) that he discovered the true law, eventually promulgated in his epoch-making *Die galvanische Kette* (1827).

It is well known that Ohm's mathematical deductions were unfortunate, and would have left a gap between electrostatics and voltaic electricity. But after Ohm's law had been further experimentally established by Fechner (1830), the correct theory was given by Kirchhoff (1849) in a way to bridge over the gap specified. Kirchhoff approached the question gradually, considering first the distribution of current in a plane conductor (1845–46), from which he passed to the laws of distribution in branched conductors (1847–48) — laws which now find such universal application. In his great paper, moreover, Kirchhoff gives the general equation for the activity of the circuit and from this Clausius (1852) soon after deduced the Joule effect theoretically. The law, though virtually implied in Riess's results (1837), was experimentally discovered by Joule (1841).

As bearing critically or otherwise on Ohm's law we may mention the researches of Helmholtz (1852), of Maxwell (1876), the solution of difficult problems in regard to terminals or of the resistance of special forms of conductors, by Rayleigh (1871, 1879), Hicks (1883), and others, the discussion of the refraction of lines of flow by Kirchhoff (1845), and many researches on the limits of accuracy of the law.

Finally, in regard to the evolution of the modern galvanometer from its invention by Schweigger (1820), we may enumerate in succession Nobili's astatic system (1834), Poggendorff's (1826) and Gauss's (1833) mirror device, the aperiodic systems, Weber's (1862) and Kelvin's critical study of the best condition for galvanometry, so cleverly applied in the instruments of the latter. Kelvin's siphon recorder (1867), reproduced in the Depretz-D'Arsonval system (1882), has adapted the galvanometer to modern conditions in cities. For absolute measurement Pouillet's tangent galvanometer (1837), treated for absolute measurement by Weber (1840), and Weber's dynamometer (1846) have lost little of their original importance.

Magnetism

Magnetism, definitely founded by Gilbert (1600) and put on a quantitative basis by Coulomb (1785), was first made the subject of recondite theoretical treatment by Poisson (1824–27). The interpretation thus given to the mechanism of two conditionally separable magnetic fluids facilitated discussion and was very generally used in argument, as for instance by Gauss (1833) and others, although Ampère had suggested the permanent molecular current as early as 1820. Weber (1852) introduced the revolvable molecular magnet, a theory which Ewing (1890) afterwards generalized in a way to include magnetic hysteresis. The phenomenon itself was independently discovered by Warburg (1881) and by Ewing (1882), and has since become of special practical importance.

Faraday in 1852 introduced his invaluable conception of lines of magnetic force, a geometric embodiment of Gauss's (1813, 1839) theorem of force flux, and Maxwell (1855, 1862, *et seq.*) thereafter gave the rigorous scientific meaning to this conception which pervades the whole of contemporaneous electromagnetics.

The phenomenon of magnetic induction, treated hypothetically by Poisson (1824–27) and even by Barlow (1820), has since been attacked by many great thinkers, like F. Neumann (1848), Kirchhoff (1854); but the predominating and most highly elaborated theory is due to Kelvin (1849, *et seq.*). This theory is broad enough to be applicable to æolotropic media and to it the greater part of the notation in current use throughout the world is due. A new method of attack of great promise has, however, been introduced by Duhem (1888, 1895, *et seq.*) in his application of the thermodynamic potential to magnetic phenomena.

Magneticians have succeeded in expressing the magnetic distribution induced in certain simple geometrical figures like the sphere, the spherical shell, the ellipsoid, the infinite cylinder, the ring. Green in 1828 gave an original but untrustworthy treatment for the finite cylinder. Lamellar and solenoidal distributions are defined by Kelvin (1850), to whom the similarity theorems (1856) are also due. Kirchhoff's results for the ring were practically utilized in the absolute measurements of Stoletow (1872) and of Rowland (1878).

Diamagnetism, though known since Brugmans (1778), first challenged the permanent interest of science in the researches of Becquerel (1827) and of Faraday (1845). It is naturally included harmoniously in Kelvin's great theory (1847, *et seq.*). Independent explanations of diamagnetism, however, have by no means abandoned the field; one may instance Weber's (1852) ingenious generalization of Ampère's molecular currents (1820) and the broad critical deductions of Duhem (1889) from the thermodynamic potential. For the treatment of æolotropic magnetic media, Kelvin's (1850, 1851) theory seems to be peculiarly applicable. Weber's theory would seem to lend itself well to electronic treatment.

The extremely complicated subject of magnetostriction, originally observed by Matteuci (1847) and by Joule (1849) in different cases, and elaborately studied by Wiedemann (1858, *et seq.*), has been repeatedly attacked by theoretical physicists, among whom Helmholtz (1881), Kirchhoff (1885), Boltzmann (1879), and Duhem (1891) may be mentioned. None of the carefully elaborated theories accoun s in detail for the facts observed.

The relations of magnetism to light have increased in importance since the fundamental discoveries of Faraday (1845) and of Verdet (1854), and they have been specially enriched by the magneto-optic discoveries of Kerr (1876, *et seq.*), of Kundt (1884, *et seq.*), and more recently by the Zeemann effect (1897, *et seq.*). Among the theorie put forth for the latter, the electronic explanation of Lorentz (1898 1899) and that of Voigt (1899) are supplementary or at least not con tradictory. The treatment of the Kerr effect has been systematized by Drude (1892, 1893). The instantaneity of the rotational effect was first shown by Bichat and Blondlot (1882), and this result has since been found useful in chronography. Sheldon demonstrated the possibility of reversing the Faraday effect. Finally terrestrial magnetism was revolutionized and made accessible to absolute measurement by Gauss (1833), and his method served Weber (1840, *et seq.*) and his successors as a model for the definition of absolute units throughout physics. Another equally important contribution from the same great thinker (1840) is the elaborate treatment of the distribution of terrestrial magnetism, the computations of which have been twice modernized, in the last instance by Neumeyer[1] (1880). Magnetometric methods have advanced but little since the time of Gauss (1833), and Weber's (1853) earth inductor remains a standard instrument of research. Observationally, the development of cycles of variation in the earth's constants is looked forward to with eagerness, and will probably bear on an adequate theory of terrestrial magnetism, yet to be framed. Arrhenius (1903) accentuates the importance of the solar cathode torrent in its bearing on the earth's magnetic phenomena.

Electromagnetism

Electromagnetism, considered either in theory or in its applications, is, perhaps, the most conspicuous creation of the nineteenth century. Beginning with Oersted's great discovery of 1820, the quantitative measurements of Biot and Savart (1820) and Laplace's (1821) law followed in quick succession. Ampère (1820) without delay propounded his famous theory of magnetism. For many years the science was conveniently subserved by Ampère's swimmer (1820), though his functions have since advantageously yielded to Fleming's hand rule for moving current elements. The induction produced by ellipsoidal coils or the derivative cases is fully understood. In practice the rule for the magnetic circuit devised by the Hopkinsons (1886) is in general use. It may be regarded as a terse summary of the theories of Euler (1780), Faraday, Maxwell, and particularly Kelvin (1872), who already made explicit use of it. Nevertheless, the clear-cut practical interpretation of the present day had to be gradually worked out by Rowland (1873, 1884), Bosanquet (1883–85), Kapp (1885), and Pisati (1890).

The construction of elementary motors was taken up by Faraday (1821), Ampère (1822), Barlow (1822), and others, and they were treated rather as laboratory curiosities; for it was not until 1857 that Siemens devised his shuttle-wound armature, and the development of the motor thereafter went *pari passu* with the dynamo, to be presently considered. It culminated in a new principle in 1888, when Ferraris, and somewhat later Tesla (1888) and Borel (1888), introduced polyphase transmission and the more practical realization of Arago's rotating magnetic field (1824).

Theoretical electromagnetics, after a period of quiescence, was again enriched by the discovery of the Hall effect (1879, *et seq.*), which at once elicited wide and vigorous discussion, and for which Rowland (1880), Lorentz (1883), Boltzmann (1886), and others put forward theories of continually increasing finish. Nernst and v. Ettingshausen (1886, 1887) afterwards added the thermomagnetic effect.

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[1] Dr. L. A. Bauer kindly called my attention to the more recent work of A. Schmidt summarized in Dr. Bauer's own admirable paper.

Electrodynamics

The discovery and interpretation of electrodynamic phenomena were the burden of the unique researches of Ampère (1820, *et seq.*, *Memoir*, 1826). Not until 1846, however, were Ampère's results critically tested. This examination came with great originality from Weber using the bifilar dynamometer of his own invention. Grassmann (1845), Maxwell (1873), and others have invented elementary laws differing from Ampère's; but as Stefan (1869) showed that an indefinite number of such laws might be constructed to meet the given integral conditions, the original law is naturally preferred.

Induction

Faraday (1831, 1832) did not put forward the epoch-making discovery of electrokinetic induction in quantitative form, as the great physicist was insufficiently familiar with Ohm's law. Lentz, however, soon supplied the requisite interpretation in a series of papers (1833, 1835) which contain his well-known law both for the mutual inductions of circuits and of magnets and circuits. Lentz clearly announced that the induced quantity is an electromotive force, independent of the diameter and metal and varying, *caeteris paribus*, with the number of spires. The mutual induction of circuits was first carefully studied by Weber (1846), later by Filici (1852), using a zero method, and Faraday's self-induction by Edlund (1849), while Matteuci (1854) attested the independence of induction of the interposed non-magnetic medium. Henry (1842) demonstrated the successive induction of induced currents.

Curiously enough the occurrence of eddy currents in massive conductors moving in the magnetic field was announced from a different point of view by Arago (1824–26) long before Faraday's great discovery. They were but vaguely understood, however, until Foucault (1855) made his investigation. The general problem of the induction to be anticipated in massive conductor is one of great interest, and Helmholtz (1870), Kirchhoff (1891), Maxwell (1873), Hertz (1880), and others have treated it for different geometrical figures.

The rigorous expression of the law of induction was first obtained by F. Neumann (1845, 1847) on the basis of Lentz's law, both for circuits and for magnets. W. Weber (1846) deduced the law of induction from his generalized law of attraction. More acceptably, however, Helmholtz (1847), and shortly after him Kelvin (1848), showed the law of induction to be a necessary consequence of the law of the conservation of energy, of Ohm's and Joule's law. In 1851 Helmholtz treated the induction in branched circuits. Finally

Faraday's "electrotonic state" was mathematically interpreted thirty years later, by Maxwell, and to-day, under the name of electromagnetic momentum, it is being translated into the notation of the electronic theory.

Many physicists, following the fundamental equation of Neumann (1845, 1847), have developed the treatment of mutual and self induction with special reference to experimental measurement.

On the practical side the magneto-inductor may be traced back to d'al Negro (1832) and to Pixii (1832). The tremendous development of induction electric machinery which followed the introduction of Siemens's (1857) armature can only be instanced. In 1867 Siemens, improving upon Wilde (1866), designed electric generators without permanent magnets. Pacinotti (1860) and later Gramme (1871) invented the ring armature, while von Hefner-Alteneck (1872) and others improved the drum armature. Thereafter further progress was rapid.

It took a different direction in connection with the Ferraris (1888) motor by the development of the induction coil of the laboratory (Faraday, 1831; Neef, 1839; Ruhmkoff, 1853) into the transformer (Gaulard and Gibbs, 1882–84) of the arts. Among special apparatus Hughes (1879) contributed the induction balance, and Tesla (1891) the high frequency transformer. The Elihu Thompson effect (1887) has also been variously used.

In 1860 Reiss devised a telephone, in a form, however, not at once capable of practical development. Bell in 1875 invented a different instrument which needed only the microphone (1878) of Hughes and others to introduce it permanently into the arts. Of particular importance in its bearing on telegraphy, long associated with the names of Gauss and Weber (1833) or practically with Morse and Vail (1837), is the theory of conduction with distributed capacity and inductance established by Kelvin (1856) and extended by Kirchhoff (1857). The working success of the Atlantic cable demonstrated the acumen of the guiding physicist.

Electric Oscillation

The subject of electric oscillation announced in a remarkable paper of Henry in 1842 and threshed out in its main features by Kelvin in 1856, followed by Kirchhoff's treatment of the transmission of oscillations along a wire (1857), has become of discriminating importance between Maxwell's theory of the electric field and the other equally profound theories of an earlier date. These crucial experiments contributed by Hertz (1887, *et seq.*) showed that electromagnetic waves move with the velocity of light, and like it are capable of being reflected, refracted, brought to interference, and polarized. A year later Hertz (1888) worked out the distribution of the vectors in the space surrounding the oscillatory source. Lecher (1890) using an ingenious device of parallel wires, Blondlot (1891) with a special oscillator, and with greater accuracy Trowbridge and Duane (1895) and Saunders (1896), further identified the velocity of the electric wave with that of the wave of light. Simultaneously the reasons for the discrepancies in the strikingly original method for the velocity of electricity due to Wheatstone (1834), and the American and other longitude observations (Walker, 1894; Mitchell, 1850; Gould, 1851), became apparent, though the nature of the difficulties had already appeared in the work of Fizeau and Gounelle (1850).

Some doubt was thrown on the details of Hertz's results by Sarasin and de la Rive's phenomenon of multiple resonance (1890), but this was soon explained away as the necessary result of the occurrence of damped oscillations by Poincaré (1891), by Bjerknes (1891), and others. J. J. Thomson (1891) contributed interesting results for electrodeless discharges, and on the value of the dielectric constant for slow oscillations (1889); Boltzmann (1893) examined the interferences due to thin plates; but it is hardly practicable to summarize the voluminous history of the subject. On the practical side, we are to-day witnessing the astoundingly rapid growth of Hertzian wave wireless telegraphy, due to the successive inventions of Branly (1890, 1891), Popoff, Braun (1899), and the engineering prowess of Marconi. In 1901 these efforts were crowned by the incredible feat of Marconi's first message from Poldhu to Cape Breton, placing the Old World within electric earshot of the New.

Maxwell's equations of the electromagnetic field were put forward as early as 1864, but the whole subject is presented in its broadest relations in his famous treatise of 1873. The fundamental feature of Maxwell's work is the recognition of the displacement current, a conception by which Maxwell was able to annex the phenomena of light to electricity. The methods by which Maxwell arrived at his great discoveries are not generally admitted as logically binding. Most physicists prefer to regard them as an invaluable possession as yet unliquidated in logical coin; but of the truth of his equations there is no doubt. Maxwell's theory has been frequently expounded by other great thinkers, by Rayleigh (1881), by Poincaré (1890), by Boltzmann (1890), by Heaviside (1889), by Hertz (1890), by Lorentz, and others. Hertz and Heaviside, in particular, have condensed the equations into the symmetrical form now commonly used. Poynting (1884) contributed his remarkable theorem on the energy path.

Prior to 1870 the famous law of Weber (1846) had gained wide recognition, containing as it did Coulomb's law, Ampère's law, Laplace's law, Neumann's law of induction, the conditions of electric oscillation and of electric convection. Every phenomenon in electricity was deducible from it compatibly with the doctrine of the conservation of energy. Clausius (1878), moreover, by a logical effort of extraordinary vigor, established a similar law. Moreover, the early confirmation of Maxwell's theory in terms of the dielectric constant and refractive index of the medium was complex and partial. Rowland's (1876, 1889) famous experiment of electric convection, which has recently been repeatedly verified by Pender and Cremieu and others, though deduced from Maxwell's theory, is not incompatible with Weber's view. Again the ratio between the electrostatic and the electromagnetic system of units, repeatedly determined from the early measurement of Maxwell (1868) to the recent elaborate determinations of Abraham (1892) and Margaret Maltby (1897), with an ever closer approach to the velocity of light, was at its inception one of the great original feats of measurement of Weber himself associated with Kohlrausch (1856). The older theories, however, are based on the so-called action at a distance or on the instantaneous transmission of electromagnetic force. Maxwell's equations, while equally universal with the preceding, predicate not merely a finite time of transmission, but transmission at the rate of the velocity of light. The triumph of this prediction in the work of Hertz has left no further room for reasonable discrimination.

As a consequence of the resulting enthusiasm, perhaps, there has been but little reference in recent years to the great investigation of Helmholtz (1870, 1874), which includes Maxwell's equations as a special case; nor to his later deduction (1886, 1893) of Hertz's equations from the principle of least action. Nevertheless, Helmholtz's electromagnetic potential is deduced rigorously from fundamental principles, and contains, as Duhem (1901) showed, the electromagnetic theory of light.

Maxwell's own vortex theory of physical lines of force (1861, 1862) probably suggested his equations. In recent years, however, the efforts to deduce them directly from apparently simpler properties of a continuous medium, as for instance from its ideal elastics, or again from a specialized ether, have not been infrequent. Kelvin (1890), with his quasi-rigid ether, Boltzmann (1893), Sommerfeld (1892), and others have worked efficiently in this direction. On the other hand, J. J. Thomson (1891, *et seq.*), with remarkable intuition, affirms the concrete physical existence of Faraday tubes of force, and from this hypothesis reaches many of his brilliant predictions on the nature of matter.

As a final commentary on all these divers interpretations, the important dictum of Poincaré should not be forgotten: If, says Poincaré, compatibly with the principle of the conservation of energy and of least action, any single ether mechanism is possible, there must at the same time be an infinity of others.

The Electronic Theory

The splendid triumph of the electronic theory is of quite recent date, although Davy discovered the electric arc in 1821, and although many experiments were made on the conduction of gases by Faraday (1838), Reiss, Gassiot (1858, *et seq.*), and others. The marvelous progress which the subject has made begins with the observations of the properties of the cathode ray by Plücker and Hittorf (1868), brilliantly substantiated and extended later by Crookes (1879). Hertz (1892) and more specifically Lenard (1894) observed the passage of the cathode rays into the atmosphere. Perrin (1895) showed them to be negatively charged. Röntgen (1895) shattered them against a solid obstacle, generating the X-ray. Goldstein (1886) discovered the anodal rays.

Schuster's (1890) original determination of the charge carried by the ion per gram was soon followed by others utilizing both the electrostatic and the magnetic deviation of the cathode torrent, and by Lorentz (1895) using the Zeeman effect. J. J. Thomson (1898) succeeded in measuring the charge per corpuscle and its mass, and the velocities following Thomson (1897) and Wiechert (1899), are known under most varied conditions.

But all this rapid advance, remarkable in itself, became startlingly so when viewed correlatively with the new phenomena of radioactivity, discovered by Becquerel (1896), wonderfully developed by M. and Madame Curie (1898, *et seq.*), by J. J. Thomson and his pupils, particularly by Rutherford (1899, *et seq.*). From the Curies came radium (1898) and the thermal effect of radioactivity (1903), from Thomson much of the philosophical prevision which revealed the lines of simplicity and order in a bewildering chaos of facts, and from Rutherford the brilliant demonstration of atomic disintegration (1903) which has become the immediate trust of the twentieth century. Even if the ultimate significance of such profound researches as Larmor's (1891) *Ether and Matter* cannot yet be discerned, the evidences of the transmutation of matter are assured, and it is with these that the century will immediately have to reckon.

The physical manifestations accompanying the breakdown of atomic structure, astoundingly varied as these prove to be, assume fundamental importance when it appears that the ultimate issue involved is nothing less than a complete reconstruction of dynamics on an electromagnetic basis. It is now confidently affirmed that the mass of the electron is wholly of the nature of electromagnetic inertia, and hence, as Abraham (1902), utilizing Kaufmann's data

(1902) on the increase of electromagnetic mass with the velocity of the corpuscle, has shown, the Lagrangian equations of motion may be recast in an electromagnetic form. This profound question has been approached independently by two lines of argument, one beginning with Heaviside (1889), who seems to have been the first to compute the magnetic energy of the electron, J. J. Thomson (1891, 1893), Morton (1896), Searle (1896), Sutherland (1899); the other with H. A. Lorentz (1895), Wiechert (1898, 1899), Des Coudres (1900), Drude (1900), Poincaré (1900), Kaufmann (1901), Abraham (1902). Not only does this new electronic tendency in physics give an acceptable account of heat, light, the X-ray, etc., but of the Lagrangian function and of Newton's laws.

Thus it appears, even in the present necessarily superficial summary of the progress of physics within one hundred years, that, curiously enough, just as the nineteenth century began with dynamics and closed with electricity, so the twentieth century begins anew with dynamics, to reach a goal the magnitude of which the human mind can only await with awe. If no Lagrange stands toweringly at the threshold of the era now fully begun, superior workmen abound in continually increasing numbers, endowed with insight, adroitness, audacity, and resources, in a way far transcending the early visions of the wonderful century which has just closed..

SECTION A—PHYSICS OF MATTER

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(*Hall* 11, *September* 23, 10 *a. m.*)

CHAIRMAN: PROFESSOR SAMUEL W. STRATTON, Director of the National Bureau of Standards, Washington.
SPEAKERS: PROFESSOR ARTHUR L. KIMBALL, Amherst College.
PROFESSOR FRANCIS E. NIPHER, Washington University.
SECRETARY: PROFESSOR R. A. MILLIKAN, University of Chicago.

THE RELATIONS OF THE SCIENCE OF PHYSICS OF MATTER TO OTHER BRANCHES OF LEARNING

BY ARTHUR LALANNE KIMBALL

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IT is evident at the outset that it is quite out of the question, in the time at our disposal, to discuss adequately the relation of the physics of matter to the other sciences, even if the speaker were endowed with the requisite omniscience.

For *matter* is the very stuff in which the phenomena of all the natural sciences are manifested, the chemist finds himself confronted at every turn with physical relations which must be taken into account, the astronomer finds his greatest triumph in exhibiting the universe that he explores with the telescope as an harmonious illustration of physical principles, the geologist also hardly faces a single question that does not demand the aid of physics or chemistry in its solution, and even in the biological sciences the laws of matter still condition the phenomena of life.

Perhaps a brief consideration of the interrelations of these sciences may aid us in a clearer perception of their dependence on the physics of matter.

There are *three* sciences that may be said to be especially fundamental, in that they deal with the elements of the universe of phenomena. These are *physics*, which, if we define it somewhat narrowly, deals with all the phenomena that can be exhibited *by* and *through the means of* any one kind of matter, as well as all interactions between different kinds of matter in which each preserves its separate identity; *chemistry*, which has for its province those special phenomena in which one kind of matter is broken up into two or more kinds, or in which the interactions between different kinds of matter result in the formation of a substance different from either of the constituents; and that phase of *biology* which is concerned with the study of the living cell and of the simplest conditions under which matter exhibits the phenomena of life.

It might have been said that *physics* deals with those phenomena exhibited by and through matter when molecular groupings of atoms are not disturbed, while *chemistry* deals with the phenomena of the formation and breaking-up of the molecules. But such a statement is based upon a theory of the structure of matter which in itself calls for explanation, and therefore the previous statement is preferred as being more general and avoiding the theoretical assumptions that are involved in those just given.

If it is asked what constitutes a particular kind of matter, why, for instance, water-vapor is said to be the same substance as water in the liquid form, it may be said that it is because one can be wholly transformed into the other, each is homogeneous, and remains unchanged in its properties during the transforming, and the transformation is unique.

Professor Ostwald has recently given a most interesting statement of the criterion by which a substance or chemical individual may be recognized without the need of any atomic hypothesis. We may summarize his presentation thus: Where two substances are combined as in solution, there will be one and only one proportion between the quantities of the substances for which, on change of state, such as evaporation or crystallization, the vapor or crystals will have the same composition as the remaining substance, while with a greater or less proportion of either ingredient, there will be a change of concentration with change of state. When such a combination retains this property under widely different conditions of temperature and pressure, it is known as a chemical individual or definite compound. If under *no* circumstances it can be broken up into two phases which differ in constitution, it is called an element.

Ostwald remarks, "The possibility of being changed from one phase into another without variation of the properties of the residue and of the new phase is indeed the most characteristic property of a substance or chemical individual, and all our methods of testing the purity of a substance, or of preparing a pure one, can be reduced to this one property."

But returning to our classification, it is seen that physics, chemistry, and biology are the three fundamental natural sciences, each having as its primary object not the mere arrangement and classification of phenomena, but the formation of such a concept of matter in those relations with which it deals, that the varied facts of observation appear as natural and inevitable consequences.

The other sciences are in a certain sense secondary to the three that have been mentioned. Each is concerned with the investigation of some system that is built up out of matter, and involves the same fundamental relations which are the objects of study for the primary sciences, but the secondary science finds its interest not in the materials of which the structure is made, but in the study of the resulting structure itself.

Thus astronomy seeks to describe and make out the past history and future development of the universe of sun and star and planet. The sciences of the earth are concerned with the history of the development of our planet, with the present phenomena of its interior, of its crust, of its surface, and of its atmosphere, while the secondary biological sciences have as their aim to trace the relations of the various forms of life and to follow out the developments of each.

But while each secondary science thus has an aim of its own quite distinct from that of the primary sciences, nevertheless it must be controlled and to some extent guided by the sciences of matter. Thus in almost every science chemical phenomena play a part which must be reckoned with, while physics, dealing as it does with the most universal phenomena of matter, underlies and conditions all the sciences without exception. Therefore it is to be expected that with the development of physics both in discovery and theory there should be a greater or less reaction on the other sciences, for in so far as they depend for their development on the laws of matter they are dependent on the labors of the physicist.

We might therefore expect to find in every science, if we only knew it well enough, a response to every considerable advance in physics. For the advances in a science result not from discovery alone, but from new points of view taken by those who are thinking on its problems; and the ideas of physics, bearing as they may be said to do on the raw material of the other sciences, must in a preëminent degree influence the thinking of workers in all fields.

It deserves to be emphasized that every science is an intellectual structure. Only as this is conceded will science be yielded the lofty and dignified position which is its due. Experiments may be multiplied, facts and data may be accumulated in bewildering numbers, but there is no science without the clear intellectual vision that sees the parts in their dependencies and relations one to another and catches glimpses of the larger unities that run through all.

They are mistaken who think the true scientist less an idealist than is the artist or student of literature, or who think the path of experiment mere drudgery in the accumulation of insignificant facts. The investigator lives in a world of ideas, and in every step of a difficult inquiry he has the buoyant consciousness that he is getting a deeper, truer insight into his science.

This intellectual character of scientific research is well illustrated in the enthusiasm which marked the news of Hertz's discovery of electromagnetic waves. The facts observed might easily have been thought to be in themselves insignificant: a slight spark observed between the ends of a bent wire near a discharging electrified system. There was no thought of a practical application, and yet a wave of almost unprecedented excitement spread among physicists the world over. Nor was it alone admiration for the skill, the insight and grasp of the great experimenter that won the victory, though this had its effect. It was mainly an exultant enthusiasm over the triumph of an idea, the unification of science in the confirmation of Maxwell's great theory.

It is clear, then, that physics may react on the other sciences in a variety of ways, in its *methods* and *appliances*, in its *discoveries*, and in its *ideas and generalizations;* and it is evident, therefore, that we must limit ourselves to a brief consideration of certain phases of the subject. I have, therefore, chosen to present very briefly some considerations relative to theories of matter, for here physics and chemistry come into the closest contact; also to touch upon some other relations of chemistry and geology to physics, that are of particular interest at this present time.

The fundamental problem in the physics of matter is the nature of matter itself. Of course we recognize at the outset the limitations that bound our attempts at a solution. We may hope to reach eventually some conclusion as to the structure of matter, whether homogeneous or molecular or grained, also as to the relative motions of the parts of the molecule and the law of variation of force between them with the distance. But if we seek to go farther and explain the forces acting in and between molecules in terms of what appear to be more simple and general laws, it seems inevitable that a medium must be assumed, the properties of which will depend on what is assumed as a primary postulate. If we accept, as is usually done, the postulate that forces in their last analysis can only be explained when referred to pressures exerted between contiguous portions of some underlying medium, it seems probable that a theory must be adopted something like the vortex atom theory of Lord Kelvin, with its continuous, incompressible, perfectly fluid medium in which vortically moving portions constitute the atoms, or Osborne Reynolds's theory of space as filled with fine hard spherical grains, in which, regions with nonconformity in arrangement, are the atoms of ordinary matter. Though it must be said that the assumed hardness of the ultimate spherules in the latter theory is a property which in itself needs explanation.

Perhaps, however, in laying down the postulate mentioned above we are pushing too far inferences from our superficial experience.

The idea that force must be a pressure between contiguous portions of substance is derived directly from the notion of the impenetrability of matter. This is why the incompressible medium of Lord Kelvin's theory seems so simple a conception; it is the naked embodiment of the idea of impenetrability associated with inertia.

It is entirely natural that such ideas as impenetrability and inertia, borne in upon us as they are by our experience of matter in bulk, should affect our theorizing, but it should never be forgotten that as fundamental postulates they have no more authority than any others that might be assumed that will coördinate the same facts of observation.

But passing from this more speculative region we find a pretty general agreement on the rough outlines of the structure of matter. With one notable exception most physicists and chemists agree in the idea that matter is atomic or molecular in structure, and that these molecules are in a state of more or less energetic translatory motion, bounding and rebounding from each other. This seems to be the mechanical hypothesis which coördinates the largest number of facts.

A portion of matter is conceived as in a condition of equilibrium under three pressures: the cohesive pressure due to mutual attraction between all molecules which are not farther apart than 50 to 100 millionths of a millimeter; the external pressure, which also acts to cause contraction; and the internal pressure, which balances the two former, and is due to a repulsive force called the force of impact, which is usually supposed to be exerted only between contiguous molecules.

In the solid and liquid states the cohesive pressure is usually very great compared with the external pressure. In case of gases it nearly vanishes. The force between molecules is thus conceived as an attraction which increases rapidly as they approach, until at a certain distance it is balanced by a repulsive force which, increasing still more rapidly, is the controlling force at all less distances.

Lord Kelvin has recently followed out a study of equilibrium conditions in a group of atoms which are assumed to have no mutual influence until within a certain distance, then to attract each other with a force that increases as they approach still nearer, rising to a maximum and then diminishing, and finally becoming a repulsion when the atoms are very near. He remarks, "It is wonderful how much toward explaining the crystallography and elasticity of solids, and the thermo-elastic properties of solids, liquids, and gases, we find without assuming in the Boscovitchian law of force more than one transition from attraction to repulsion."

The fundamental soundness of the conception of matter as having a grained structure of some sort seems to be established by the remarkable degree of agreement in the estimates by various physicists of the size of these ultimate particles, meaning by that the smallest distance between their centres as they rebound from each other, especially when it is considered that these results have been reached from so many different points of view, and are based on such a variety of physical data.

As to the structure of the atom itself a most remarkable theory has been recently developed. J. J. Thomson has marshaled the evidence in favor of the theory proposed by Larmor that matter has an electrical basis, and the theory has already been considerably developed by Lorentz and others. There appears to be reason for believing that the corpuscles of the Kathode rays are simply moving charges of negative electricity, their whole apparent mass being due to their relation to the ether, in consequence of which there is a magnetic field around the moving charge having energy dependent on the square of its velocity. The corpuscle, therefore, effectively has mass in consequence of this reaction between it and the ether.

The corpuscles are found always to carry the same charge, whatever the nature of the gas in which the Kathode rays are formed, and whatever the nature of the electrodes — the charge being the same as that given up by the hydrogen atom in electrolysis, while the mass of the corpuscle is about one one-thousandth that of the hydrogen atom.

The energy in the ether associated with the moving corpuscle depends on the size of the corpuscle as well as upon its charge, and it is found that to account for its apparent mass it must be of extremely small size relative to ordinary atomic dimensions.

Professor Thomson suggests that the primordial element of matter is such a negative electron combined with an equal positive charge, the latter being of nearly atomic dimension. An atom of hydrogen may be thought of as made up of nearly a thousand such pairs, the positive charge being distributed throughout a spherical region giving rise to a field of force within it in which the force on a negative corpuscle will be towards the centre and proportional to its distance from the centre. In this field of force the corpuscles are conceived as describing closed orbits with great velocities.

The internal energy of such an atom is conceived as enormous. In case of the atoms contained in a gram of hydrogen Thomson reckons about 10^{19} ergs as the energy received from mutual attractions in the formation of the atoms, an amount of work that would lift a hundred million kilograms, one thousand meters.

The whole mass of the atom is supposed to be due to the *negative* electrons or corpuscles which it contains. As to the *positive* charge, although it determines the apparent *size* of the atom, it appears to make no contribution to its mass.

When such an atom impacts against another, the corpuscles in each will be disturbed by the jar in their orbital motion, and there will be superposed oscillations which will cause radiation of energy.

If a corpuscle escapes from such an atom, the latter will be left with a positive charge, while if an additional free corpuscle is entrapped, the atom will have a negative charge. The conditions of stability of motion of the corpuscles in the atom would thus determine whether in case of electrolysis the substance would appear electro-positive or electro-negative.

J. J. Thomson, Drude, and others have discussed the electric conduction of metals from the standpoint of this theory. Drude states that in non-conductors only bound electrons are present, that is, positive and negative in combination; and that it is these that determine the dielectric constant of the medium and consequently its index of refraction and optical dispersion; while Langevin explains magnetism and diamagnetism.

Thus we have a theory already surprisingly developed which appears to be applicable to explain many of the properties of matter, though it is not clear that it can give an explanation of cohesion and gravitation. A theory of matter, to be accepted as final, must offer some explanation of the relation between the various elements. Many thinkers have been led to look for some primordial element from which the others are derived, influenced on the one hand by the present evolutionary ideas of biology, and on the other by comparison of spectra and by the remarkable tendency towards whole numbers observed in the atomic weights of the elements which Strutt has discussed from the standpoint of the theory of probabilities. Professor Thomson has accordingly shown how atoms of matter containing great numbers of corpuscles may have been evolved from a simpler primordial form containing fewer corpuscles. But though he has made clear how the hydrogen atom with its thousand corpuscles might be the surviving atom having the *least* number of corpuscles, it is not so clear why there might not be atoms having any number of corpuscles greater than that of hydrogen, within certain limits; why none should be found between hydrogen and helium for example. Some kind of natural selection seems to be needed to explain why some atoms having special numbers of corpuscles survive while intermediate ones are eliminated, though probably the answer is to be sought in the conditions of stability of the motions of the corpuscles.

It is an interesting question what would be the effect of change of temperature of the substance on the motions of the corpuscles in this theory. If the corpuscles in the atom were very numerous, all moving in the same orbit at equal distances apart, they would produce almost the effect of a circular current of electricity, — a steady magnetic field and no radiation; and it seems probable that in the actual case the radiation of internal energy is extremely small, and the total internal energy may be supposed to be so enormous compared with the energy of translation of the atom due to temperature that we may expect no appreciable change in the radiation of internal energy of the atom, whatever the temperature may be.

That component of the vibration of a corpuscle which is radial within the atom, and is set up by the impact of one atom against another, seems to furnish the great mass of radiated energy. This radiation must also react on the motion of the atom as a whole, taking away from the translatory energy of the atom.

The question how the Boltzmann law of partition of energy between the various degrees of freedom will apply to molecules made up of such atoms as are here conceived is an interesting and important one. Is it possible that the *cloud,* as Lord Kelvin calls it, resting on the kinetic theory of gases may be dissipated by the new theory?

This theory of the atom seems also to explain the possibility of the production of spectra of great complexity. It is to be hoped that Balmer's formula and Rydberg's laws of the grouping of lines in spectra may be shown to be the natural outcome of the system of vibration possible in such an atom.

We are startled at first by the very audacity of this theory, seeming as it does to upset the old point of view, and seek the explanation of matter and its laws in terms of the properties of ether and electricity, instead of trying to unravel the secrets of electricity and ether in terms of matter and motion.

Only a few years ago it was thought that the electromagnetic theory of light must be rationalized by giving a mechanical explanation of the various phenomena of the ether, or by showing at least that such an explanation was possible. Witness Maxwell's wonderfully ingenious mechanical model illustrating the phenomena of magnetism, induced currents, and the propagation of electromagnetic waves.

But is it necessary to regard the mechanical explanation as the only sound one? If electricity and ether are fundamental entities underlying all matter and material phenomena, is it not more logical to find a basis for the mechanical laws in some more fundamental laws of ether and electricity which must be accepted as the primary postulates?

In all this development of the atomic view of matter, chemistry and physics have gone hand in hand. The atomic theory of Dalton has been the basis on which both sciences have worked. Avogadro's law for gases has been reached not only by chemical evidence, but has been raised to the rank of a mechanical deduction from the kinetic theory. The significance of the arrangement of atoms in the molecule in determining chemical reaction was emphasized and developed by Kekulé, but it was not until 1874 that the space diagrams of molecules of van't Hoff and Le Bel marked a full appreciation of the possibilities of structure in explaining the differences of isomeric forms.

All of these physical and chemical developments of the atomic theory have been in accordance with a general method of scientific procedure which may be called the method of mechanical models. According to this method, an attempt is made to conceive a certain mechanism by which the various phenomena sought to be explained may be imagined to be brought about.

Such a theory of atoms, for example, if perfect, would exhibit all the properties of atoms as direct consequences of the assumed structure. This cannot, however, be taken as proof that the assumption is real, though for the purpose of our thinking such a theory would have all *the value* of reality, since all consequences deduced from it would conform to the facts of observation. And this suggests wherein the great value of such a theory lies, not alone in the large number of observations which it correlates and brings under a few general principles, but in that it suggests the application of experiments and tests of its sufficiency, thereby enlarging and making more precise our knowledge.

Perhaps the most remarkable instance of the application of this method was Maxwell's development of a mechanical model to illustrate the reactions in the electromagnetic field. Working from this model he developed the equations of the field, which later he deduced in a more general way. And Hertz speaking of them says, "We cannot study this wonderful theory without at times feeling as if an independent life and a reason of its own dwelt in these mathematical formulæ; as if they were wiser than we were, wiser even than their discoverer; as if they gave out more than had been put into them."

On which Boltzmann's comment is, "I should like to add to these words of Hertz only this, that Maxwell's formulæ are simple consequences from his mechanical models; and Hertz's enthusiastic praise is due in the first place, not to Maxwell's analysis, but to his acute penetration in the discovery of mechanical analogies." Such an example well illustrates the importance of the method.

But of recent years, the influence of quite a different method has been strongly marked in chemical research. A method in which certain general laws are established and then applied to particular cases by a process of mathematical reasoning, deducing conclusions quite independently of the particular details of the operation by which they are brought about. This method is well illustrated in Professor J. J. Thomson's work on the application of dynamics to problems in physics and chemistry, and in the deductions based on the laws of thermodynamics that have marked the development of the new physical chemistry.

It is under the influence of this method that Professor Ostwald has been led to propose a theory of matter which does not recognize the necessity of any atomic structure whatever. In a recent address, he says, "It is possible to deduce from the principles of chemical dynamics all the stoichiometrical laws; the law of constant proportion, the law of multiple proportion, and the law of combining weights." And he continues, "You all know that up to this time it has only been possible to deduce these laws by the help of the atomic hypothesis. Chemical dynamics has, therefore, made the atomic hypothesis unnecessary for this purpose and has put the theory of the stoichiometrical laws on more secure ground than that furnished by a mere hypothesis." And then farther on he continues, "*What we call matter is only a complex of energies which we find together in the same place.* We are still perfectly free if we like to suppose either that the energy fills the space homogeneously, or in a periodic or grained way; the latter assumption would be a substitute for the atomic hypothesis." And then he adds, "Evidently there exists a great number of facts — and I count the chemical facts among them — which can be completely described by a homogeneous or non-periodic distribution of energy in space. Whether there exist facts which cannot be described without the periodic assumption, I dare not decide for want of knowledge; only I am bound to say that I know of none."

It is interesting and remarkable that this challenge to the atomic theories of matter should come from the side of chemistry, the very science for which the atomic theory of Dalton was conceived. Especially is it remarkable, in view of the measure of success that has attended the explanation of the differences between such forms as right and left rotating tartaric acids on the basis of molecular structure. And it is difficult to see how it is possible to give any satisfactory explanation of these differences, simply on the basis of the laws of energetics applied to a conception of matter as homogeneous.

With reference to the view that "*What we call matter is only a complex of energies which we find together in the same place,*" it may be said that we recognize different forms of energy only in association with matter or ether; as heat, light, chemical energy, kinetical energy, etc. Hence the term, "a complex of energies," can only mean the total energy in a given region, unless we recognize some vehicle, as matter or ether, in which the special manifestations of energy may exist. This seems to be admitted tacitly by Ostwald himself, for a little farther on he says, "The reason why it is possible to isolate a substance from a solution is that the available energy of the substance is at a minimum." He thus distinguishes between the available and the total energy of a portion of matter. But this discrimination can have no meaning unless it is granted that a portion of the energy of a substance is not available. If we ask why it is not available, the answer may be that when a substance passes from one state to another at constant temperature the work that it can do is less than its total intrinsic energy as a consequence of the laws of thermodynamics. The case must therefore be one to which the second law of thermodynamics can apply. That is, it must involve flow of energy by some such process as heat conduction.

It might perhaps be successfully argued that the very existence of such a process implies grained structure of some sort to which a statistical law may apply. However this may be, it is certainly difficult to conceive of energy as existing apart from some vehicle, matter or ether or both as you will; but to conceive of this sublimated energy as in part available and in part non-available is surely quite beyond attainment.

It is with great diffidence that we dissent from the expressed views of one who has done so much for the advance of physical chemistry, and our excuse for entering on the discussion must be that as the latest utterance with regard to matter, and coming from one who has won the right to have his views given a respectful consideration, it seemed more fitting to present this brief and imperfect discussion than to pass them by without comment.

One of the most important reactions of physics upon the other sciences has resulted from the extension of the *thermodynamic* laws to chemical problems which has marked the new physical chemistry, a science which has sprung into being within the last seventeen years and has already, under the leadership of van 't Hoff, Ostwald, Arrhenius, and Nernst, attained a surprising development, and is making itself felt in many other lines of scientific activity, notably in electrochemistry, geology, and biology. The starting-point in this development was the idea conceived by van 't Hoff that Avogadro's law might be so extended as to apply to the case of substances in solution. Just as a gas expands and fills the containing vessel exerting a pressure against its walls, so a salt dissolved in a liquid diffuses uniformly throughout the liquid and exerts a pressure within the liquid tending to expand it. This osmotic pressure, so called, had been measured in certain cases by Pfeffer and de Vries, but it remained for van 't Hoff to show that, as in case of a gas, the pressure was proportional to the absolute temperature and to the number of molecules of the dissolved substance contained in unit volume.

As has so often happened before, the study of the apparent exceptions to the rule led to a second great advance, the theory of electrolytic dissociation proposed by Arrhenius, to account for the observation that in solutions of electrolytes the osmotic pressure was greater than that reckoned on the basis of the number of molecules present, but was to be explained by their dissociation into ions; thus reaching the same conclusion which Clausius had announced in 1857, but affording a method by which the precise amount of the dissociation might be measured. Additional evidence in favor of this theory was afforded by the studies of the electrical conductivity of dilute solutions of electrolytes made by Kohlrausch.

All this was accompanied by an increasing realization of the important relations that might be established by an application of the laws of thermodynamics to chemical problems. Thus van 't Hoff showed in his paper of 1887 that the depression of the freezing-point of a liquid due to a substance in solution depended directly on the osmotic pressure and could be used to measure it; a result which had already been experimentally reached by Raoul.

In this field, Professor J. Willard Gibbs, in whose recent death the world of science has lost a most profound thinker, was a pioneer. His most important contributions to the subject were in two extraordinary papers, *On the Equilibrium of Heterogeneous Substances.* The first of these related to chemical phenomena, while the second was concerned especially with capillarity and electricity.

To quote from a recent writer, "The most essential feature of Gibbs's discoveries consisted in the extension of the notion of thermodynamical potential to mixtures consisting of a number of components, and the establishment of the properties that the potential is a linear function of certain quantities which Gibbs has called the potentials of the components, and that where the same component is present in different phases, which remain in equilibrium with each other, its potential is the same in all the phases, besides which the temperatures and pressures are equal. The importance of these results was not realized for a considerable time. It was difficult for the experimentalist to appreciate a memoir in which the treatment is highly mathematical and theoretical, and in which but little attempt is made to reduce conclusions to the language of the chemist; moreover it is not unnatural to find the pioneer dwelling at considerable length on comparatively infertile regions of the newly explored territory, while fields that were to prove the most productive were dismissed very briefly."

"It was largely due to Professor van der Waals that two new and important fundamental laws were discovered in Gibbs's paper, namely, the phase rule and the law of critical states."

The phase rule has been the guiding principle in some most important studies of chemical equilibrium. It furnishes a clue by which the polymorphism of such substances as sulphur and tin may be scientifically investigated and the conditions of equilibrium between the different polymorphic forms determined. The studies of the case of ferric chloride by Roozeboom, and of the crystallization out of sea-water of the contained salts by van 't Hoff and Meyerhoffer indicates the great value of the phase rule in bringing scientific order out of the complicated relations of the various components and phases involved.

Speaking of this department of physical chemistry, van 't Hoff remarked, "Since the study of chemical equilibrium has been related to thermodynamics, and so has steadily gained a broader and safer foundation, it has come into the foreground of the chemical system, and seems more and more to belong there." And Ostwald says in answer to the question, "What are the most important achievements of the chemistry of our day? I do not hesitate to answer: chemical dynamics, or the theory of the progress of chemical reaction, and the theory of chemical equilibrium."

These statements, coming from two masters in the field, are most significant of the importance of the introduction of these ideas into chemistry.

The conceptions and methods of physical chemistry have also been most strongly felt in the field of electrochemical theory. To the question what is the nature of electrolysis, Faraday and Hittorf and Clausius had each contributed important elements of the final answer, then came Arrhenius with the theory of electrolytic dissociation, which has proved so fruitful of consequences, not only in the domain of chemistry, but also in biology and in physics.

One of the most interesting scientific questions connected with electrochemistry is the relation between electromotive force and electrolytic separation, and the development of the theory of the voltaic cell. The question of the seat of electromotive force in the cell was for many years the very storm-centre of physical discussion; but from the standpoint of electrolytic dissociation Nernst has supplemented the work of Helmholtz and Gibbs, and out of all has come a theory which, while not perfect, seems to be in its main features on the solid foundation of the conservation of energy and the laws of thermodynamics.

Another important service for which the world of science is indebted to physics is the determination of the absolute zero of temperature in terms of degrees of the ordinary centigrade scale. About a century ago, Dalton, in his new chemical philosophy, adopts −3000° C. as the probable zero of temperature. While Lavoisier and Laplace make various estimates of the zero ranging from 1500 to 3000 degrees below the freezing-point of water. But when the doctrine of energy became firmly established together with the kinetic theory of gases, it was natural that the condition of a gas in which the particles had no energy of motion, and hence no pressure, should have been taken as indicating the absolute zero. But it was Clausius and Lord Kelvin who based firmly on the laws of thermodynamics the absolute scale of temperature, as we know it to-day.

The absolute zero of temperature has to the physicist all the fascination that the North Pole has to Arctic explorers, and is probably even more difficult to attain. Yet steady progress has been made in conquering the difficult territory that lies toward this goal. The experimental efforts to liquefy the more refractory gases showed that far lower temperatures than had previously been reached must be employed; and step by step, following the suggestions of thermodynamics, the means of attaining low temperatures have been improved, at first cooling by adiabatic expansion of more compressible gases, then aided by the sudden expansion of the gas itself which had been compressed and cooled, and then by a continuous self-intensive action, in which the cold produced by the expansion of one portion of the compressed gas was made use of to cool the still unexpanded gas as it approached the point of expansion.

The mere record of the temperatures reached marks a series of triumphs of ingenuity and perseverance. Thus Faraday, in 1845, reached a temperature of -110 by the use of solid carbon dioxide and ether evaporated at low pressure. Pictet in 1877 reached -140, and liquefied oxygen under pressure. Olszewski in 1885 obtained a temperature of -225 by the evaporation of a mass of solid nitrogen. In 1898 Dewar obtained liquid hydrogen boiling at -252, or only 20.5 above the absolute zero, and later by boiling at reduced pressures he was able to obtain -259.5 or 13.5 degrees absolute scale, at which point hydrogen is frozen solid.

The attainment of these low temperatures has not alone made possible investigations of the greatest interest to the physicist, such as studies of the magnetic and electric properties of bodies as they approach the absolute zero, but has enabled the effect of extreme cold on chemical actions to be determined, and has led to the interesting conclusion that "The great majority of chemical interactions are entirely suspended." Though it has been shown by Dewar and Moissan that in case of solid hydrogen and liquid fluorine, violent reaction still takes place even at that small remove from the absolute zero.

A very interesting field has also been opened to biological research, in the effect of extreme cold on the vitality of seeds and micro-organisms. It was found, for example, that barley, pea, and mustard seeds steeped for six hours in liquid hydrogen and thus kept at a temperature of minus 252 degrees, showed no loss of vitality. So, also, certain micro-organisms, among others the bacilli of typhoid fever, Asiatic cholera, and diphtheria, were kept by MacFadyen for seven days at the temperature of liquid air without appreciable loss of vitality. It has been suggested by Professor Travers that, "It is quite possible that if a living organism were cooled only to temperatures at which physical changes, such as crystallization, take place with reasonable velocity, the process would be fatal, whereas, if they were cooled to the temperature of liquid air no such change would take place within finite time, and the organism would survive."

Also the study of the various combinations of carbon and iron that may exist in steel, and the conditions of equilibrium that exist between them has proved a most important investigation in the field of what van 't Hoff calls solid solutions.

Geology, dealing as it does with the greatest variety of physical processes, such as changes of state, fusion, crystallization, solution, conduction of heat, radiation, with complications depending on variations of pressure and temperature, presents many problems for the solution of which the resources of modern physics must be taxed. The fusing-points of the different chief minerals of the earth's crust, the effect of great pressure on their fusing-points and modes of crystallization, the crystallization of the various elementary minerals out of a fused magma also studied at different pressures, the effect of pressure not only on fusing-points, but on the viscosity and rigidity of minerals at high temperature, the heat conductivities of the various substances making the bulk of the earth's crust, all these are questions that must be thoroughly studied to enable the geologist to determine the probable condition both of temperature and pressure which prevailed during the formation of a given rock mass, and to throw light on the great problem of geology, the age of the earth.

To this latter question, physics has already given a tentative answer. Lord Kelvin's discussion, based on the assumption of the earth as a mass cooling from a uniform high temperature, points to a period of between twenty and one hundred million years, within which geologic changes in the crust of the earth must have occurred; while Helmholtz and Kelvin's deduction of the time during which solar radiation can have been of such an intensity that life conditions on the earth were possible gives about twenty million years as the limit.

But later investigations giving new data as to the properties of the materials of the earth's crust, as to the laws of variation of radiation with temperature, and as to absorption and radiation by the solar and earth's atmospheres, will all contribute to modify and make more precise these methods. Already some progress in this direction has been made. A few years ago, Clarence King gave a most interesting and ingenious rediscussion of Kelvin's cooling of the earth method, making use of the determinations made by Barus of the fusing-points of diabase at different pressures, and gives as the most probable result of the method the period of twenty-four million years, a period in close agreement with that found by Helmholtz and Kelvin from the radiation of the sun.

It should be remarked, however, that in discussing the state of things in the earth's interior, where the pressures so far transcend anything that can be approached in the laboratory, such constants as melting-points should be looked on with great suspicion.

Assuming Laplace's law of distribution of density in the earth, the pressure at a depth of one two-hundredth of the earth's radius is 8600 atmospheres, while at the centre of the earth it becomes more than three million atmospheres. Now the largest pressures that have been used in high temperature experiments are less than three thousand atmospheres. It is evident, then, that any conclusion as to melting-points from laboratory data must be violent exterpolations, if deduced for the enormous pressures at depths greater than one one-hundredth of a radius within the earth, where the pressure will be over 17,000 atmospheres.

But not only is there necessarily great uncertainty as to the fusing-points at these great pressures, but it seems probable that such a process as fusion marked by sudden increase in liquidity can hardly take place at all. In the phenomenon of fusion, the equilibrium of a substance may be regarded as conditioned by the external pressure, the cohesive pressure, and the internal pressure due to the translatory kinetic energy of the molecules, which may be called the kinetic pressure. In a state of equilibrium, the external pressure plus the cohesive pressure must equal the kinetic pressure, the last tending to produce expansion, while the two former act to cause contraction. At ordinary atmospheric pressures in the liquid and solid state, the cohesive pressure is enormously greater than the external pressure. In water at ordinary temperatures it is estimated about 6500 atmospheres, while in a solid such as steel it may have a value of perhaps 18,000 atmospheres. And not only is this cohesive force great relatively to the external pressure, but it decreases with great rapidity as the substance expands. Under these conditions it is easy to see that a slight rise in temperature with consequent expansion and weakening of the cohesive pressure while the kinetic pressure is increased may bring the substance to a point of transition, a melting-point or boiling-point where great changes occur within narrow limits of temperature.

But if we conceive the external pressure to be so great that the cohesive pressure is relatively insignificant, then we should not expect to find any sharply marked changes of state for small changes of temperature or pressure.

To make the case definite assume a temperature of 1000 degrees absolute scale, and a pressure of 1,000,000 atmospheres, and suppose the cohesive pressure is 10,000 atmospheres. Under these circumstances a rise in temperature of ten degrees or a one per cent increase in temperature may be expected to produce a one per cent increase in the kinetic pressure at the original volume; but as the external pressure is constant and the cohesion is insignificant, we may expect a one per cent increase in the volume in which the molecular motions take place or an increase in the mean distance between molecules of one third of one per cent. Such an expansion will be accompanied by slightly lessened cohesive force, less rigidity, and less viscosity, probably; but nothing like a sudden change of state is suggested. The fact that at pressures greater than the critical pressures there can be observed no sharp transition from the liquid to the gaseous state with rise of temperature is quite in accord with the above considerations, and it seems probable that in case of solids under great pressure nothing like melting will be observed, but rather a gradual loss of rigidity or transition to great viscosity, and that the viscosity will decrease steadily with rise in temperature.

But a new aspect is now given to the problem of the age of the earth by the discovery of radioactivity and its attendant phenomena. The earth, instead of being thought of as a cooling body, is now conceived as having within itself a source of almost unlimited energy. Locked up in each atom is believed to be a store of energy so vast that the breaking down of comparatively few of them in the radioactive process will supply the known outflow of heat from the earth.

Rutherford has shown that the observed dissemination of radioactive substances in the earth's crust is probably sufficient to account for the outflow of energy from its surface. Thus the method of estimating the age of the earth from the consideration of it as a cooling body, a method which until lately seemed to physicists to be based on essentially sound premises, and deserving of confidence because of its greater simplicity as compared with the methods by which geological and biological estimates are obtained, is now by the very progress of physics itself abandoned as unreliable.

So also has the study of radioactivity thrown new light on the question of the maintenance of the sun's heat. It is now seen that possible atomic transformations accompanied by the liberation of the vast stores of energy locked up within the atoms of matter may permit an enormous extension of the time during which the sun may have been radiating with something like its present intensity.

In conclusion it may be remarked that a new world is opened to the investigator by the discovery of radioactivity. The atoms of matter are no longer thought of as necessarily fixed and unchangeable. Besides the older problems of matter questions now arise as to evidences of atomic disintegration and change from more complex to less complex forms, and also the possible development of more complex atoms from simpler ones.

Already we begin to see the effect of these recent discoveries and ideas on other departments of science. The clue at last seems to have been found to those long-standing enigmas of nature, thunderstorms, the Aurora Borealis, the zodiacal light, and the tails of comets. But these achievements belong perhaps rather to the realm of the physics of the ether and of the electron, than to that of the physics of matter.

PRESENT PROBLEMS IN THE PHYSICS OF MATTER

BY FRANCIS EUGENE NIPHER

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In dealing with the subject allotted to me by the officers of the Congress, I must say that I have not presumed to solve the problems which present themselves at this time, nor do I feel competent even to state many of them. But it is instructive, in a time like this, to attempt a general survey of some of the great questions of the day, with a view of noting their bearing upon the knowledge of the past. We are continually made to feel that all of our inquiries and results must be reëxamined, and our conclusions broadened and modified by new phenomena.

Charles Babbage, whose last published work was, if I mistake not, a review of the London Exposition of 1851, in the Ninth Bridgewater Treatise, gave incidentally, by way of enforcing his thoughts, a review of his earlier work on calculating-machines. His work covered the simple case of a machine composed of wheels and levers, capable of computing the successive terms of any series. The simplest case is an arithmetical series, the differences between the successive terms being unity. This is the device which we now use in the street-cars for counting fares. He asserted the possibility of making a machine, capable of computing the terms of such a series, or of any other, continuing the operation for thousands of years; and pointed out that the machine may be so designed that it will then compute one single arbitrary term, having no relation to the series which had preceded. It may then resume the former series, or it may begin computing a geometrical series, or a series of squares or cubes of the natural numbers. A scientific investigator, who is not permitted to see the mechanism, begins to observe and record the series of numbers which are being disclosed on the dials. He soon learns the mathematical law of the series. He observes the time-sequence of the successive terms, and computes the date when this order of things began. He then makes use of his knowledge of other machinery, and makes a working drawing of the hidden mechanism which produces these results. He verifies his work by years of subsequent observations. With what amazement does he finally behold that single arbitrary term! With what amazement does he then see the machine begin to compute the squares or the cubes of the numbers it had previously disclosed! The date when that machine was created and set to work has been rudely called in question by the new and seemingly lawless behavior of which it appears to be capable. And yet the observer still feels that the principles of mechanism have not been shaken by this unlooked-for disclosure. He again begins his work, with broader conceptions of the plan of this machine. And his subsequent work is along precisely the same lines, and by the same methods as his previous work.

It is in exactly this way that all scientific work has proceeded, and I wish to point out a few interesting cases of this kind. I find it impossible to do this without presenting the present aspect of these problems in connection with the work of the past. This plan gives a perspective which not only adds to the interest but to the clearness of the presentation.

The nebular hypothesis was an attempt by Kant, Laplace, and Herschel to trace the evolution of the solar system from a glowing mass of incandescent vapor or gas. As the theory was considered and developed, an immense number of correlated phenomena were found to be in harmony with this hypothesis, and a few discordant phenomena were also found. The operation was, moreover, based on a few fundamental and well-established laws, governing the present condition of the system; such as gravitation, radiation of heat, etc. The case became more and more convincing, as the knowledge of the last century was applied. All of this caused the astronomers and physicists to find it very easy to give to the hypothesis their tacit assent.

Later, Sir William Thomson, now Lord Kelvin, took up the question of underground temperature, and determined the limit in time since which the earth must have begun to solidify. He also assumed that the present order of things had come down to us from the past, and that the present order of things consisted in the radiation of heat from a cooling earth.

The time-interval which Kelvin thus determined was in entire harmony with the nebular hypothesis, but the results were received with something like consternation by geologists, and those who had followed Darwin in the study of the evolution of organic life upon the earth. Afterwards Kelvin sought to show that the process of solidification might have required but a short interval of time, and the evolutionists have found that evolution goes on by steps or sudden changes rather than by a continuous succession of imperceptible increments.

The geologists have never been reconciled to Kelvin's results, and their protests have of late seemed to be on the increase. Of late the situation has changed in various ways. The discovery of radioactive matter in wide diffusion in the earth's crust has reopened the whole question of underground temperature as related to the age of the earth and its past history. Nevertheless, if the nebular theory in any form, or any similar theory, represents the process of evolution of the solar system, a large amount of heat due to gravitational contraction must have resulted, and must have been disposed of by radiation.

During several years I have been giving attention to the conditions of evolution of a gaseous nebula. The equations of equilibrium for such a mass have been developed.[1] A cosmical mass of gas was assumed, satisfying everywhere the Boyle-Gay-Lussac law, capable therefore of expanding, of being compressed, and of transmitting pressure, and having a centre towards which it gravitates.

Such a mass of gas is a simple heat-engine. The piston face is any spherical concentric surface. The load on the piston is the weight of superposed layers, external to the piston face. The radially inwardly directed pressure is exactly that required to balance the outward pressure of the inclosed mass. As radiation and contraction proceed, the load on the piston increases, in a perfectly definite way, due to increase in weight of each element of mass as it approaches the gravitating centre. Whatever may be the nature of the gas, as determined by the numerical value of the Boyle-Gay-Lussac constant, at some time in its history contraction will have proceeded until some fixed or definite mass shall have been compressed within a fixed volume of definite radius. The equations show that the pressure at the surface of this mass, that is to say, the load on the piston, will then be entirely independent of the nature of the gas.

The difference between gases will only be shown in the time required for them to reach this assumed stage in their gravitational history. A gas which permits the heat of compression within the piston face to escape most quickly into the refrigerator external to the nebula will reach this stage most quickly. When this has been done, pressures and densities at the piston face are wholly independent of the nature of the gas. The total work of compression done on the mass within the piston face up to this time is also independent of the nature of the gas. But the temperatures at the piston face will be inversely as the numerical value of the Boyle-Gay-Lussac constant.

It is evident, therefore, that the law of contraction cannot be indeterminate as in the case where the load is imposed by the hand of man. There is, therefore, in addition to the Boyle-Gay-Lussac law, another definite relation between any two of the three variables involved in that law. The application of well-known equations of thermodynamics led to the result that the density at any such piston face was directly proportional to the *n*th power of the pressure. The value of n is found to be 0.908 for all gases like oxygen, hydrogen, nitrogen, and air. The operation is, therefore, one lying between isothermal and isentropic compression, and near to the former. The specific heat of gravitational compression is therefore negative. The unit mass of gas at any point rises in temperature during compression, and for a rise of temperature of 1°C., it gives off by radiation a definite amount of heat.

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[1] *Transactions*, Academy of Science of St. Louis, XIII, no. 3; XIV, no. 4.

If, now, such a nebula be supposed to extend to an infinite distance from the gravitating centre, the mass of the nebula will be infinite. Pressure, density, and temperature then all become zero at an infinite distance. Suppose such a nebula to have reached such a stage in its contraction that the mass of our solar system, 1.99×10^{33} grammes, is internal to Neptune's orbit, then it turns out that the pressure there will be about what it is in Crookes tube, 1.74×10^{-7} atmospheres. The density will be far less than in a Crookes tube, viz.: 1.40×10^{-13} C. G. S. The temperature for a hydrogen nebula will be 3000°C., and for other gases it will be higher in inverse ratio as the value of the Boyle-Gay-Lussac constant.

If the mass of the nebula be made finite, the conditions become still more interesting. Let the condition be imposed that the mass of the nebula is that of our solar system, and that it has so contracted that Neptune's mass only is external to Neptune's orbit. Then the temperature at Neptune's place drops to about 1900°C., for hydrogen,[1] and both pressure and temperature become very much less than before. $P\,1.49 \times 10^{-10}$; $d\,1.93 \times 10^{-15}$. The thickness of the spherical shell which would contain Neptune's mass is about a million miles (1.65×10^{-11} cm.). At the external surface of this nebula, the condition imposed makes P, d, and T zero, as the equations show. Nevertheless, a large fraction of Neptune's mass would be gaseous and far above its critical temperature. It seems to me impossible to think of a nebula having such properties generating by any reasonable rotation a system of planetary bodies. With Neptune's mass on the surface of such a nebula consisting of matter having a density and pressure less than a thousandth of these values in a Crookes tube vacuum, how could we conceive of this matter being gathered into a single planet?

A much more reasonable hypothesis is one discussed by G. H. Darwin in 1889, in the Philosophical Transactions of the Royal Society.[2] Darwin discussed the properties of a swarm of solid meteoric masses, and gives very strong proof of the proposition that a system of planetary bodies may originate in this way, although he is very cautious and conservative in stating conclusions. The great importance of this theory of planetary origin from the standpoint of planetary geology and the evolution theory seems to demand that it should receive more attention than it has yet received. The temperature of the great mass of such a swarm will be very much lower than in the case of the gaseous nebula. The larger part of such a mass will approach absolute zero in temperature. According to this hypothesis, even Mercury may have been solid when it separated from the parent mass, although in its later stages a large mass might become a gaseous nebula, as the sun now is. But in case of a body like our earth, of such relatively small size, and so far removed from the heated core, there does not seem to be any necessity for the assumption that it was ever in a fused condition.

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[1] In a nebula of mixed gases, each gas will, of course, have its own temperature, as is well understood.

[2] On the "Mechanical Conditions of a Swarm of Meteorites," and on "Theories of Cosmogony," *Phil. Trans.* 1889.

In view of these new developments, it seems peculiarly important that a discussion of the limits of maximum temperature which the mass of our earth has reached in the past should now be taken in hand again. Suppose a swarm of meteorites to fill the space internal to the moon's orbit, having a total mass equal to that of our earth. Assume that the mass is in rotation, so that the moon is about to separate from the parent mass. It would probably be too radical to assume that each element of mass has either the same actual velocity or the same angular velocity. Various hypotheses, more or less probable, are possible. Assume an initial temperature approaching zero absolute. It seems clear that the highest temperature reached in passing to the present condition of things may be far below the temperature of fusion.

A body falling directly from the moon's distance to the earth will develop 59/60 of the kinetic energy it would acquire in falling from an infinite distance. The earth is yet being bombarded by meteoric matter having such velocities. But the operation is taking place so slowly that the heat has time to become dissipated by radiation, so that no appreciable rise in temperature of the earth results. To what extent may this condition have held in the past? Darwin discussed the tendency of the larger masses in such a swarm to accumulate towards the centre. It is a kind of sorting process. These larger masses would be in general of a metallic character. The more brittle rocks of smaller density would therefore form the outer layers of our earth. May not the heterogeneous character of our so-called igneous rocks be explained in this way? And the shrinking of the earth would then perhaps be in part the flowing of this porous mass into continuity. And it may incidentally be pointed out that the existence of the belt of meteorites known as the asteroids is a most significant indication of the conditions which must have existed at a certain stage in the history of our solar system.

The problems of the present which have aroused general interest are those which pertain to the physical constitution of matter. And here we are at once confronted with the question, What do we mean by matter? How is matter to be recognized? Of late we have been hearing such phrases as "the electrical theory of matter." There seems to be a marked tendency towards the idea that matter and its properties are alike electrical phenomena. Some even intimate that the molecular theory of gases, and the atomic theory of the chemist are tottering to a fall. We have long known that matter in motion is a form of energy. This energy of moving matter is continually being converted into molecular or atomic vibration, and then escapes from us, apparently, forever, in the form of ether waves. We have also long known that electricity in motion is a form of energy, and that the energy so manifesting itself is also all finally converted into heat, and then into ether waves.

Now this parallel certainty suggests an electrical theory of matter, but it also suggests, equally, a material theory of electricity. And so far from being antagonistic, these two theories are identical. There is nothing whatever to show that electricity has ever been separated from something which has what we have been accustomed to call mass. Rowland[1] found that when the charged sectors on his rotating disk were rotated, a magnetic field was produced, corresponding to that produced by a current of electricity. If the motion of the matter which carries the positive electric charge is in a positive direction, the field is the same as that produced when a negative charge is moved in a negative direction.

Rutherford has recently found phenomena of radioactive matter which have a most vital interest in connection with Rowland's work. The α and β particles which are shot off from such matter are moving in the same direction, and they are oppositely deflected in a magnetic field. They behave like superposed or perhaps juxtaposed electric currents of opposite sign flowing in the same direction. If in these radiations the α and β particles were moving in opposite directions, then in a magnetic field they would be deflected in the same direction. This at once raises a question concerning the nature of an electric current in a conducting wire. Let us assume that we start with the positive and negative charges on the terminals of the Holtz machine. What is it that is taking place when the terminals are joined by wires leading to a galvanometer? We get a current which we are wont to say is due either to a positive current flowing in a positive direction, or to a negative current flowing in an opposite direction. If we cease to apply work to the rotating wheel, it comes to rest, and the potential of the conducting wire becomes uniform throughout. Its extremities which terminate in front of the charged inductors are therefore so charged as to produce this uniform potential in the presence of these charged inductors, and the polarized glass of the rotor. The ends of the conductor are therefore oppositely charged. There is on its surface a neutral line of no charge. During the motion of the rotor these opposite charges are oppositely directed in the conductor. They are continually being added together. Equal quantities of unlike signs are continually being added together. Are we to assume that equal currents of unlike signs are superposed? Is a positive current in a positive direction identical with a negative current in a negative direction? Mathematically we should say yes. The resulting current, moreover, is uniform throughout the circuit, when measured by its external electromagnetic effects. We may loop in calibrated galvanometers at any point in the circuit, and they tell the same story. But what do the results of Rowland and Rutherford teach us? The β particles carry the negative charge. The negative charge is part and parcel of something which has a positive mass. The α particles are perhaps a combination of more β particles in combination with other particles having (or being) a positive charge of greater numerical value. We have found long ago that the products of an explosion are not necessarily composed of matter in its most elementary form. But these α particles are also part and parcel of something which has a positive mass.

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[1] *American Journal of Science*, [3] xv, 30–38, 1878.

Are we to think of this conductor as being the seat of some action by which positive masses are being urged in a positive direction and positive masses are also being urged in an opposite direction? Are we to think that the mass of such a conductor, carrying a direct current, is slowly increasing, and that after many thousands of years this increase will become appreciable, resulting, perhaps, in a clogging of the conductor, and a decrease in its conduction? In that case a current of positive electricity moving in a positive direction is not a current of negative electricity moving in a negative direction. In that case the nature of positive and negative currents of electricity flowing in opposite directions is fundamentally different from that of the flow of heat and cold in opposite directions, for it involves the motion of masses in opposite directions. It would be interesting to examine whether the long-continued use of a conductor carrying a continuous current may not result in conferring upon it radioactive properties. The results of J. J. Thomson[1] on the phenomena shown by a Geissler tube 15 meters in length are very significant in this connection. He finds the positive luminescence to travel in a direction opposite to that of the cathode stream in the Crookes tube, with a velocity somewhat more than half that of light. The older results of Wheatstone[2] also show that the current from a Leyden jar travels in opposite directions within the conductor which joins its coatings. The middle point of the conductor is last reached by the discharge. If the discharge is maintained and a steady current is finally produced, this current must apparently consist of positive and negative electricity flowing in opposite directions.

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[1] *Recent Researches in Electricity and Magnetism*, p. 116.
[2] *Phil. Trans.*, Royal Society, London, 1834.

If air be pumped out of one boiler and into another, two kinds of pressure are thus generated. If these pressures are added together, by connecting the boilers by means of a conductor, these pressures are added together, and both disappear. If we tap these charged boilers, the discharge from one will attract, and from the other will repel, an uncharged testing sphere. If the testing sphere be itself charged, we shall find that like charges repel, if both are positive, and attract, if both are negative.

It is unnecessary here to enlarge upon the well-known differences between the positive and negative terminals of an exhausted tube. All of these phenomena will finally be helpful in arriving at the nature of the difference between positive and negative electricity. But I will refer to certain phenomena which do not seem to be so well known. Every one is familiar with the small points of light which may often be seen dancing in a crazy fashion over the cathode knob of the Holtz machine. A similar appearance can be seen on the negative carbon of a direct current arc, and in the negative bulb of the mercury vapor-lamp. These points of light may be made to pass from the cathode knob of the Holtz machine to the surface of a photographic dry-plate, exposed in open daylight.[1] Separate the knobs so that no spark will pass. Place the plate near or between them. Connect the knobs with two small metal disks, each armed with a pin-point, so bent that it makes contact with the film. The point of the pin may rest upon the short mark of a lead pencil, drawn upon the film, the pins pointing towards each other on the plate. Points of light, like the so-called ball-lightning discharges, will come from the cathode terminal and successively travel slowly over the plate, leaving a blackened trail of reduced silver behind. By means of a lead pencil held in the hand with the point near the cathode pin-point, these discharges may be induced to make their appearance on the film, and may be deflected into various directions after they have appeared. When left to themselves these minute specimens, of what may perhaps be called ball-lightning, tend to follow the lines of the field, but their paths are somewhat affected by the paths of prior discharges. If one of these points of light is seen on the pin which arms the cathode terminal, there will usually be none upon the film of the dry-plate. It may be brought upon the plate by holding a pencil-point near it.

These ball discharges come from the cathode and travel to or towards the anode. They cannot be induced to come from the anode.

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[1] *Transactions* of the Academy of Science of St. Louis, x, no. 6.

or to travel against the negative current. The anode terminal has a visible discharge which appears to pass from it, and the photographic plate at the anode looks somewhat like a picture of a relief map of the delta formation at the mouth of a river.

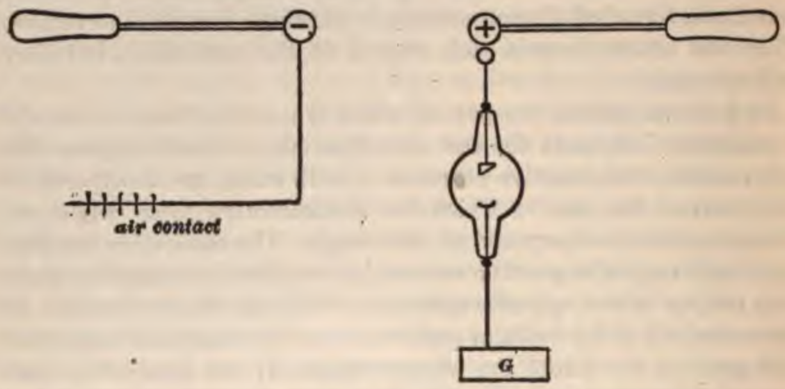
If a conductor be laid upon the plate between the two pin-points, there are then two gaps in the circuit. Each has an anode and a cathode. This conductor may be a metal disk armed with pins 180 degrees apart, which face the discharge points. It may be a pencil-mark upon the film or even a spot of reduced silver on the film. The same discharge will start from the cathode terminal of this intermediate conductor and will travel slowly in the negative direction.

With an induction coil giving an eight-inch spark, these ball discharges can be formed on the surface of wood. In all cases it is evident that chemical work is being done by the slowly advancing ball or point of light, and it is interesting to observe that it is the cathode discharge only which seems to be active. The reason for this may be partly electrical and partly chemical. The anode terminal of the machine may be grounded on a gas-pipe, and the cathode terminal only armed with a point, and the plate may be placed far away from the machine, connection being made between its cathode terminal and the pin-point on the film, with the same results. It may be added that these plates may be of the most sensitive character, and may be freely exposed to daylight for days before they are used. They may also be developed in the light in a bath not very strongly alkaline. The plate will develop clear, with the discharge tracks dark. The picture will not reverse photographically. It probably would do so if the plate were exposed to direct sunlight while the electrical exposure is made.

With an induction coil having an alternating potential on its terminals, these ball discharges may be obtained from both terminals. They will travel towards each other if on the same plate, but they will not unite.

In a closed circuit, one part of which is moved across the lines of a magnetic field, as in the case of a dynamo, we must suppose that the positive and negative currents, if both exist, are superposed in that part of the wire in which the electromotive force originates. The currents are superposed at their origin. The same ether machinery which urges the positive current in one direction urges the negative current in the opposite direction. With the Holtz machine, we have one half of the machine positively and the other half negatively charged. If the knobs are widely separated, and conductors each armed with the pin-point be led off in opposite directions, each terminating on the film of a photographic plate, the cathode will deliver a ball discharge upon its film, while the anode will not. The machine terminal which is not being used may, if desired, be grounded on a gas-pipe. If a pencil-mark be made upon the plate near the anode it will be acted upon inductively, and a ball discharge will pass from it to the anode pin-point. The positive discharge will go in the opposite direction from the pencil-mark, but it leaves no trace. It appears that this ball discharge upon the surface, which results in a destruction of the insulation of the surface, is a characteristic of the negative current.

What would be the result if a suspended Maxwell coil were to be looped into either of these unipolar circuits? Would this case necessarily give the same result that Maxwell obtained?[1] Of course we know that the result which Maxwell sought to detect is very small. We are more particularly concerned with the nature of the action than with the magnitude of the result. If the a particles are so large that they can contribute little or nothing to the current through a metallic conductor, then the positive current may practically be left out of consideration. But it seems doubtful whether the a particles are ultimate in their character, and here is where experimental work is yet needed. It would be exceedingly interesting to study these ball discharges upon a photographic plate under diminishing pressures, as they gradually become a cathode discharge, in a Crookes tube. A Crookes tube may be connected by only one of its terminals to the Holtz machine. The free terminals of the machine and tube may be connected to wires hung on silk fibres and making contact with many pointed ground plates hung on long silk fibres in air. The terminals are then in fact grounded on the dust particles in the air. Either one of these air contacts may be replaced by a ground on the gas-pipe. In all of the possible arrangements covered in this description the tube will give excellent X-ray pictures.



One of these arrangements is represented in the annexed figure, where the cathode terminal of the tube is grounded on the gas-pipe, and is therefore at zero potential. The ground contact of the tube may be replaced by an air contact, and the negative terminal of the machine may then be grounded on the gas-pipe if desired. In none of these cases are the positive and negative currents delivered by the machine superposed in the X-ray tube. In all these cases X-ray effects are obtained, but in some respects the tube behaves very differently when in the positive current from what it does in the negative. In the negative unipolar circuit, the cathode terminal of the tube is in direct communication with the negative terminal of the machine. When in the positive circuit, the anti-cathode terminal of the tube is in direct communication with the positive terminal of the machine, and the cathode terminal is acted upon inductively across the Crookes tube vacuum. In the negative current the luminous appearances are normal and stable. When in the positive current, the discharge may be made to cease by holding the hands near the bulb, and the luminous glow is affected by the motion of neighboring bodies. The discharge is much more unstable. If the observer approaches the suspended grounding device the face and the hands are covered by luminous points of light, which characterize the cathode terminal. This phenomenon is very striking under these conditions. Ball discharges may be drawn from such point discharges on a metal point to a photographic plate, moving on the plate towards the anode wire or contact plate suspended in air. It is apparent that the wire, when considerably removed from other bodies, is discharging upon the dust particles in the air.

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[1] *Electricity and Magnetism*, II, p. 201.

In 1879 Spottiswoode and Moulton[1] published a paper containing a great array of experiments upon the spark discharge through gases. They there dealt with unipolar discharge, and their conclusions are well worthy of notice in this connection. They conclude that "the independence of the discharge from each terminal of the tube is so complete that we can at will cause the discharges from the two terminals to be equal in intensity but opposite in sign (as in the case of the coil) or of any required degree of inequality (as in the case of the coil with a small condenser). Or we can cause the discharge to be from one terminal only, the other terminal acting merely receptively (as in the case of the air-spark discharge with the Holtz machine); or we can cause the discharge to pass from one terminal only, and return to it, the other terminal not taking any part in the discharge; or finally, we can make the two terminals pour forth independent discharges of the same sign, each of which passes back through the terminal from whence it came." This work was done before the Crookes tube had appeared. It is certainly interesting to observe that when a high degree of rarefaction has been reached, the activity within the tube is represented by the cathode stream, even when the terminal from which it comes is acted upon only inductively. The remarkable thing is that the X-ray effects and the luminosity of the tube should then be so great. The unipolar positive discharge in the positive direction and the unipolar negative discharge in the negative direction give, in the same time, an X-ray picture of the same intensity, when developed in the same bath, although in the latter case the cathode is only acted on inductively.

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[1] *Phil. Trans.*, 1879.

Unquestionably the great problem of to-day is the determination of the nature of positive electricity and its relation to what is left when the β particles have been removed. When the cathode particles have left the induced cathode terminal it is positively charged, and communicates that charge to the dust in the air, or to neighboring bodies. It does this, however, by a similar inductive action, and the ball discharge traveling over the photographic plate suggests that here also the negative particles are the active ones.

The few who have search-lights have of late been throwing them upon the great mass of experimental work on the discharge through gases, published during the last generation. It is most instructive to remember that the Crookes tube was known for seventeen years before Roentgen discovered that something was going on outside of it. A repetition of some of the work done on spark discharge, and in particular the work of Wheatstone, in the light of what is now known, would be likely to yield results of the greatest value. It would be of particular value to study by the Wheatstone method the unipolar discharges of the Holtz machine.

A few words only may be added respecting radioactive phenomena.

We have long been familiar with the changes in matter, of a character such as may perhaps be described as spontaneous. Many crystals slowly lose their water of crystallization. They give off emanations. They explode very slowly. Now emanations, like all other matters and things, have individual peculiarities which enable us to recognize them. The emanation from crystallized sodium carbonate is also given off by all animals and plants, and is evidently a very useful and widely diffused substance. There are many substances which go to pieces and give off energy. They explode. Many of them give off more energy per gram per second than any radioactive body, while radium gives off more energy per gram than any other body. The radium explosion also goes on at a lower temperature than that of any other body. It hardly seems to be warranted, to say that the action is the same at the temperature of freezing hydrogen as at ordinary temperatures, for it does not seem that any high degree of precision has been attained in such a measurement. And certainly it can hardly be claimed that we know what these radioactive bodies would do at a temperature below 16 degrees absolute.

Seven years ago, an attempt was made in my laboratory to obtain

X-ray effects from explosions. High-grade gunpowder was loaded by strong compression into rifle-shells designed for a 40-grain charge. This powder was discharged from a heavy rifle, against an oak target six inches from the muzzle of the gun. The target was faced with thin plates of aluminum, which required frequent renewal. The concussion was sufficient to extinguish a gas-flame seven feet from the line of discharge. The plate used was one which would yield distinct X-ray effects from an exposure of one second to a Crookes tube, operated by an eight-plate Holtz machine. The dry plate was placed behind the target, and was subjected to the discharge of twenty-five pounds of powder, the operation requiring the spare time of the experimenter for forty days. The result was negative. No fluorescent effects could be detected by an observer behind the target. A rapid-fire gun might yield different results.

The same experiment was made with a thousand copper shells loaded with mercury fulminate. They were exploded in twos, one being fired electrically, the other being exploded by the concussion. The first shell was laid upon a wooden block resting on a two-inch plank. The second shell, to be exploded by it, was laid upon it with a heavy iron bolt-head just above. No metal was interposed between the explosive and the photographic film beneath the plank, and it was necessary to replace the block by a fresh one at each explosion. These explosions were so violent that a photographic plate of glass was shattered by the shock at almost every shot, and the windows thirty feet distant were perforated by bits of copper which occasionally escaped through the surrounding screens. A sensitive film of gelatine was used, on which the shadow picture was expected, but none was obtained. There is yet some reason to expect positive results from experiments of this kind. It may well be that explosives differ in this respect as in others. An investigation of the products of such explosions by the electrical means now used in the study of radioactive bodies is a wide and most inviting field, which is likely to aid in the explanation of radioactive phenomena.

Some of the products of explosion in the case of radium and uranium are more nearly elementary in character than other bodies yield, and some of the products are more elementary than others.

Now there is nothing unusual in finding here and there a substance which has some property to a very exalted degree. The diamond is such a case. Iron is vastly more magnetic than any other substance. All substances are magnetic. A group consisting of iron, cobalt, nickel, etc., are more magnetic than the great body of substances, and iron heads the list. There is nothing more remarkable in finding a group of radioactive substances with one which enormously surpasses all others than there is in finding an Academy of Science with some member surpassing all the others in some particular direction.

The relations which have been found to exist between atoms and molecules are no more disturbed by the behavior of radioactive substances than by the explosion of nitroglycerine. We have learned that what we have provisionally called atoms are, at least in some cases, as has long been believed, very complex in their structure. We should hardly expect an architect to lose confidence in houses, if he finally learns that the bricks with which he is familiar are not the final elements in their structure. That the bricks are made up of molecules, and the molecules of atoms, and the atoms of electrons, and that some houses have been observed to fall into pieces and give off energy, would hardly affect the usefulness of houses which do not fall to pieces, even if inertia is shown to be an electromagnetic phenomenon. And I think we should all remember that the proposition that matter has mass is fundamentally different from the proposition that a mass of matter has inertia. If inertia can be explained to be an electromagnetic quantity, and if it can be measured in new units, we have not changed the properties of matter. It is still matter, and it still has both mass and inertia. If inertia is an electromagnetic phenomenon, it may be measured in terms of the fundamental units in which all electromagnetic quantities are measured, — the units of length, time, and mass.

Formerly a force was measured in terms of the unit of mass only. People talked about a force of one pound. Later it was discovered that a force could also be measured in terms of the pound, the foot, and the second. At this time we did not hear any intimation that matter had had its day and was about to be abolished.

In physics we now think we have reached the domain of small things. But the electron may also be a very complex structure. If we accept Poynting's view of the nature of electromagnetic induction, the electron in a conductor is acted upon by a distant and moving electron, through a medium external to the conductor. The experimental verification of this is very convincing. In addition to this complex machinery we have to deal with machinery of gravitation.

We may always assume that nature is everywhere complex and ingenious. A visitor to our solar system, who should begin to study it from our earth, might begin with physical astronomy. He finally comes to chemistry, to zoölogy, and the phenomena of life, to governmental organization, to the moral and religious influences which dominate the lives and actions of men, to the simultaneous jurisdiction of state and federal courts within the same territory. By the time he had come to know this world as we know it, he would conclude that this universe of ours, which he first perceived as a faint and distant speck of light in the blazing firmament of stars, is, after all, very wonderful, and very much more complex than was at first believed. In arriving at our present ideas of the mechanism through which matter reacts on matter, we have not reached them by finding that the old ideas must be renounced, in order to explain some new phenomenon which is apparently out of harmony with the explanation previously made. It is rather that each new development has confirmed what had gone before, has made it seem more reasonable, and has filled in some gap in the knowledge of the past. The ether, which only a few years since was assumed to exist because it seemed to be necessary, has become more and more centrally important, and has finally come to monopolize most of the attention of those who would seek to understand matter. It is no reproach to modern ideas concerning the physics of matter, that they are complex. The fact that they are also harmonious and beautiful, and that they furnish an explanation of why a mass of matter has inertia, and promise the explanation of other long-standing puzzles, converts the accusation of complexity into a crowning glory.

SECTION B — PHYSICS OF ETHER

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(*Hall* 11, *September* 23, 3 *p. m.*)

CHAIRMAN: PROFESSOR HENRY CREW, Northwestern University.
SPEAKER: PROFESSOR DEWITT B. BRACE, University of Nebraska.
SECRETARY: PROFESSOR AUGUSTUS TROWBRIDGE, University of Wisconsin.

THE ETHER AND MOVING MATTER

BY DEWITT BRISTOL BRACE

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THE question whether the luminiferous ether passes freely through matter or participates in the translation of the same, considered as a moving system, stands to-day without positive answer, notwithstanding the numerous experimental attempts and the varied hypotheses which have been made since the discovery of aberration by Bradley in 1726. The simple explanation of this phenomenon on the corpuscular theory may have caused the century of delay in the closer examination of the question until it became necessary to consider it from the standpoint of undulations in an ether. As compared with the many efforts to examine the question in the second or ether period we have perhaps but two belonging to the first or corpuscular period. Boscovich, in 1742, reasoning from this theory on the ground of a difference of velocity in air and water, proposed to examine the aberration of a star with a telescope whose tube was filled with water. This experiment was not carried out till long after by Airy in 1872, who found that the variation in the aberration was absolutely insensible. Arago, in the second instance, reasoning on the same theory, concluded that the deviation produced by a prism would vary with the direction of the earth's motion; but he was unable to detect any such change, a result verified later by more delicate means in the hands of Maxwell, Mascart, and others. This experiment, which demonstrated the absence of any effect of the earth's movement on refraction is of great historical interest. This negative result, which to Arago was inconsistent with the corpuscular theory, suggested to Fresnel the important hypothesis of a quiescent ether penetrating the earth freely but undergoing a change of density within the medium proportional to the square of its index and being convected in proportion to this excess of density, which would give an apparent velocity to the ether of $(1-\mu^{-2})\nu$, instead of the velocity of the earth. Stokes suggested, as a simpler idea, that we suppose the ether is not convected but passes freely through the earth, being condensed as it passes into a body in the ratio of 1 to μ^2, so that its velocity within the refracting medium becomes $(1-\mu^{-2})\nu$, from the law of continuity.[1]

Babinet in the second-century period attempted to test Fresnel's theory by examining the interference of two rays traversing a piece of glass, the one in the direction of the earth's motion and the other in the opposite direction. Stokes showed that a negative result was not contrary to the theory of aberration, since the retardation would be the same as if the earth were at rest.

He showed further, what Fresnel had not proven to be true in general, that on Fresnel's theory the laws of reflection and refraction for single refracting media are uninfluenced by the motion of the earth. In fact, Rayleigh has shown that, in using terrestrial sources, no optical effect can be produced by any system of reflecting or refracting optical surfaces moving as a rigidly connected system relatively to the ether, if we take into account the Döppler "effect," and neglect quantities of the second order of the aberration. Since, as Stokes says, the theory of a quiescent ether may be dispensed with, and as there is no good evidence that the ether moves quite freely through the solid mass of the earth, he proposes to explain the phenomenon of aberration on the undulation theory of light, upon the supposition that the earth and the planets carry a portion of the ether along with them, so that the ether close to their surfaces is at rest relatively to those surfaces and diminishes in velocity till at no great distance in space there is no motion. Cauchy had previously discussed the theory of a mobile ether, and had proposed to explain aberration by a shearing of the wave-fronts due to the translatory motion of the medium, but he did not develop his method sufficiently to explain how much the aberration would be.

On the other hand Stokes has specifically indicated his assumptions and formulated his conclusions. He examines the displacements of a wave-front in its passage from the ether at rest, across the region of transition to the ether in the neighborhood of the observer, which is at rest relatively to him. Adopting the same method which is used in the case of an ether at rest in determining the wave-front at any future time from that of a given one at any instant, he shows,

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[1] If x is the velocity of the ether relative to the moving matter, and the density of ether within it is μ^2, the density of free ether being unity, we have from the law of continuity $\nu=(\nu-x)\mu^2$ and hence,

$$x \quad \frac{\nu\mu^2-\nu}{\mu^2}=(1-\mu^{-2})\nu$$

on the one condition, viz. that the motion of the ether is differentially irrotational, that if we neglect the square of the aberration and of the time, the change in direction of the ray as it travels along is *nil*, and therefore the course of a ray is a straight line, notwithstanding the motion of the ether. Following out the analysis on this supposition, a body, a star for example, will appear displaced toward the direction in which the earth is moving through an angle equal to the ratio of the velocity of the earth to that of light, when moving normal to the star's direction. This rectilinearity of propagation of a ray, which would likely seem to be interfered with in the motion of the ether, is the tacit assumption made in explaining aberration. If the physical causes, in consequence of which the motion of the ether becomes irrotational, could be adduced, the theory of Stokes would satisfy completely aberration and the negative results of the many and various experimental investigations which have thus far been made and whose validity is unquestioned, whether in refraction, interference, diffraction, rotary polarization, double refraction, induction, electric convection, etc. In an ordinary fluid, tangential forces proportional to the relative velocities destroy the irrotational condition in a steady state of motion. If we suppose these forces to be diminished indefinitely we obtain now a motion totally different from that for the steady state when these forces are assumed to be absent initially; and hence such a motion would be unstable. When, however, tangential forces depending on relative displacements in the ether are considered, it becomes possible to explain the irrotational condition. Any deviation from this state, for example at a surface of *slip*, would be dissipated away into space with the velocity of light by means of transverse vibrations. He illustrates such apparent incompatibilities in physical states by successive dilutions of gelatine. Such a medium shows elastic tangential forces for small constraints, and yet apparent fluidity for motions through it, mending itself as soon as dislocated. He regards these qualities as consistent and self-sufficient to explain the phenomena in question. Against the view of Stokes, Lorentz raises objection to his assumptions concerning the ether motions in the neighborhood of the earth, which he considers inconsistent, a difficulty which he is unable to set aside. Larmor demurs against an appeal to a highly complex medium, such as pitch, for studying the behavior of a simple one like the ether. A time-rate much shorter than the time of relaxation will of course provide approximate rigidity, while a time-rate much longer will provide approximate fluidity, but this requires inevitable dissipation. This objection would be valid for a viscous solid, but such Stokes apparently did not have in mind, since he specifically proves such a case unstable. A solid like pitch is a very different type of solid from that of a vesicular solid like jelly. An ether after the model of a viscous solid would always contain the viscous terms, so that even for the high time-rates of light-waves there would be dissipation however small. Such a condition, it can be proven, would give coloration to the remote members of the stellar system; a fact inconsistent with observation. On the other hand, a soft vesicular solid like gelatine may not necessarily contain the time-factor, and yet be so soft that dislocation may occur even with constraints of the order of aberration, but not of the square of that order. Such an ether without a *time of relaxation* factor would fulfill completely the conditions of a luminiferous ether, if, as Stokes tried to show, it could be reconciled with the phenomena of aberration and the motions of the heavenly bodies. The method of double refraction shows that a solution of gelatine of one part in a thousand is rigid, while at the same time it appears as mobile as water, and its rate of flow through small tubes does not vary largely from the same. This experiment illustrates very markedly Stokes's example. When such a solution is continuously dislocated between two surfaces in relative motion, the same double refraction is present, indicating that the stress is still active during dislocation. Also a metal, like copper, shows a similar stress while being strained beyond its elastic limit. If this takes place by *slip* or dislocation throughout the mass which, though irregular, may give a mean uniformity for sensible dimensions, such a medium might serve as our model. Any deviation from perfect regularity in molecular distribution and activity we might anticipate would give such minute irregular dislocations at the limit of elasticity. Such a medium would thus transmit completely any disturbance within this strain limit.

It is difficult, however, to conceive of the transmissions of a disturbance across a surface of dislocation. For many ordinary media, we should expect at such a surface total reflection. If we suppose such a transmission of disturbance, its mode is not apparent, even if we suppose a thin lamina in rotational motion which would diffuse at least a portion, if not all, of the incident disturbance. Similar difficulties would arise if we assume the ether a solid which becomes fluid under stress and thus allows bodies to pass through it (as, for example, through a block of ice, as Fitzgerald suggested). While such solutions may seem highly artificial and do violence to our convictions, the consequences of a quiescent ether may, when fully developed and tested, demonstrate its impossibility and command a more extended examination into the structural qualities of an all-sufficient medium than the single case of an essentially vesicular medium like jelly brought forward by Stokes and in a different form as a contractile ether by Kelvin. The theory of Fresnel of a quiescent ether in space presupposes a change of its density proportional to μ^2 within a ponderable medium, and a convection coefficient

$(1-\mu^2)\nu$. This hypothesis satisfies the phenomena of aberration and the uniformity of the laws of reflection and refraction of a body, whether in motion or at rest, and, as already mentioned, does not affect interference, as Stokes showed, so far as the earth's motion is concerned. That the ether apparently is carried along within moving matter not with its full velocity, but diminished to the extent indicated by Fresnel's coefficient of convection, Fizeau demonstrated in his famous interference experiment with streaming water, repeated later with greater refinement by Michelson and Morley. The significance of this experiment in its bearings on the question of the drift of the ether has perhaps been overestimated. In fact, neglecting the square of the aberration, it is exactly what we should expect from the dynamical reaction of a moving material system on a periodic disturbance, propagated through it without reference to the motion of translation of the interpenetrating medium, but simply to the frequency of the vibration impressed upon the system by this ether. Thus if we transform the ordinary differential equations of motion of the material system from fixed to moving axes, the form of the solution contains Fresnel's convection coefficient as a factor exactly, neglecting quantities of the second order of the aberration. This experiment cannot then be adduced as a positive result in favor of a quiescent ether. On account of its physical consequences, however, it should be extended to the case of gases and to absorbing substances, using light corresponding to the natural frequences of the latter if possible. Although negative results have heretofore been obtained with a gas, yet, with high pressures and greater dimensions and velocities, the test is within present experimental limitations. Results with solid bodies are still lacking, but a preliminary examination of the problem encourages us to expect successful results, at least with double-refracting substances. Reasoning in a similar manner as on the dynamical reaction of a moving system, we should look for the acceleration of a circularly polarized ray propagated coaxially within a rapidly rotating medium. This may possibly be brought within experimental limits. Again we have the important experiment of Lodge on the effect of moving masses upon the motion of the ether near them. This experiment, like that of the preceding one of Fizeau, is a first order test, *i. e.* the effect to be observed would arise from a change in the first power of the aberration factor. Two interfering beams were sent around several times in opposite directions between two rotating steel disks, and the effect on the bands noted from rest to motion or reversal. With a linear velocity not far from one two-hundredth that of the earth's orbital motion, and a distance of some ten meters or more, no influence on the interfering rays could be detected, thus making the effect, calculated from the aberration factor if the ether were carried around between the disks, something like twice the limit of observation. Lodge estimates from this experiment that the disks must have communicated less than the eight-hundredth part of their velocity to the ether. It is to be noted that the masses of these disks were not great, being only some two or three centimeters thick and about one meter in diameter. If we suppose the ether to be set in motion by means of reactions of a viscous nature, the experiment would be conclusive. To this extent, that the ether is not viscous, the test seems to be valid, but as there are other modes conceivable by which such movement might be brought about, it is not conclusive. If now we have to give up the notion of a quiescent ether, it will be necessary to suppose such motions are engendered in some way depending on the mass of the moving system, which we might imagine to be the fact in the case of the earth and the surrounding ether (possibly as, Des Coudres suggests, through gravitational action). It would be desirable to repeat this experiment, using great masses, and also testing to a much higher degree of sensibility (the third order would be possible) by means of double refraction. Michelson has recently attempted to determine directly whether the velocity of the ether diminished as we recede from the earth, but with negative results. He sent two interfering rays in opposite directions around the four sides of a rectangle of iron piping from which the air had been exhausted, the same being in a vertical east and west plane, the horizontal length of which was 200 feet and the height 50 feet. Assuming an exponential law for the variation in the velocity of the ether as we recede from the earth, he finds that if the earth carries the ether with it, this influence must extend to a distance comparable with the earth's diameter. The negative result in many of the experiments on refraction and interference which different investigators have obtained and which apparently follow on the assumption of a mobile ether have been usually experiments capable of giving only second order effects instead of the first order effects looked for, which, as mentioned above, are quite as consistent with a quiescent ether, as Stokes and Rayleigh have shown. Among these may be mentioned the experiments of Hoek, Ketteler, Mascart, and others on interference in ponderable media, over opposite paths relatively to the earth's motion; as also those of the two latter with double-refracting media. All of the experiments were first order tests, and hence should give negative results on either theory, since, with a terrestrial source of light, the phenomena are independent of the orientation of the apparatus neglecting second order effects.

The positive results of Fizeau and of Ångström have not been confirmed and should not be seriously considered. In the experiments of the latter, the variation of the position of the Frauenhofer lines, as obtained by a grating when observed in directions with and opposite to the earth's orbital motion, has never been noted since, beyond the anticipated displacement calculated from the purely kinetical principle of Döppler. The experiments of the former, as a first order test, on the rotation of the plane of polarization of a ray after passing through a pile of plates has perhaps offered the greatest difficulty to the exponents of both theories in reconciling the observations with the results which should follow from each theory. In this experiment, performed in 1859, the optical systems was mounted so as to be rotated about a vertical axis alternately from east to west, or *vice versa*. This system consisted of the usual polarizing nicol or sensitive tint-system and analyzing nicol between which were placed several piles of plates and compensating systems for producing the rotations and the magnifying of the same, and also for compensating for the rotary dispersion and elliptic polarization of the transmitted light which was polarized in an azimuth of 45°. In a series of observations extending over some time the mean of the rotations of the plane of polarization showed a maximum excess in the direction toward the west at noon and at the time of the solstice. It is to be noted that light from a heliostat was reflected into the system alternately by two fixed mirrors when the system was rotated. This required an interruption and readjustment of the heliostat during a single observation, *i. e.* from east to west and west to east, the difference in the setting of the analyzer in the two positions to give the same field of view being, of course, the effect sought for. Fizeau refers to the irregularities arising from successive settings of the heliostat. The calculated effect was much below that which could have been observed directly with the usual polarizing system. To magnify any such effect, a second system of plates was used which gave an amplification as high as eighty times. Thus any residual rotation from whatever cause would receive the corresponding amplifications. Now, in experiments with polarizing systems using sunlight as a source of illumination, it has frequently been noted that any shift in the direction of the light through the apparatus, either due to a change in the direction of the beam (arriving, say, from the heliostat) or to a shift in the optical system itself, produced a change in the field of view, whether with a half-shade system or otherwise. In the former the match was destroyed, the change being of an order much greater than that which Fizeau anticipated from calculation. Further, with such limited beams of light, a mere shift of the eye may produce an effect of similar magnitude. Hence, in all polariscopic experiments where sunlight is used, it is absolutely essential that, during any single observation, the ray of light pass through the system and into the eye over exactly the same path. This Fizeau failed to carry out, and this is entirely sufficient to explain the very great discrepancies in his various series of observations, and probably the apparent constant difference in the results of his settings in the two directions.

In fact, Fizeau himself has stated since that his observations were not absolutely decisive. While the test is now probably within experimental limits with the more highly refined half-shade systems, other modes of experimenting on different optical principles with greater sensibilities have given negative results, thus disproving the existence of a phenomenon which Fizeau's experiment apparently established, and making a repetition of this experiment, which is of doubtful execution, unnecessary.

The effect of the motion of a natural rotative substance through the ether on the rotation of the plane of polarization is of considerable importance in its bearings on certain controverted points in some of the recent theories of a quiescent ether. Mascart, who first studied the problem in the case of quartz, was unable to detect any difference in the rotation when a ray was propagated in and against the direction of motion of the earth. This variation in the total rotation, which he could detect, was one part in 20,000, or one part in 40,000 on reversal. This experiment as thus carried out corresponds to a first order effect. Rayleigh quite recently has repeated this experiment with a sensibility five times as great, and obtained negative results, likewise. The impossibility of obtaining quartz in sufficient quantity and purity, or natural rotary liquids of sufficient power, to attain the extreme limit of polariscopic possibilities seems to make even an approximation to a second order effect entirely improbable, although the higher frequencies might be used, where the power may be ten times as great. On the other hand, the effect of the mechanical rotation of such a medium on the circular components is, however, probably not beyond experimental possibilities in polariscopic work.

On the electrical side several first order experiments have been made which likewise have given negative results. Des Coudres has attempted to determine the difference in the induction on each of two coils placed symmetrically, with respect to a third coaxial coil between them. On compensating for the effects of each on the galvanometer when the axis of the system was in the direction of drift, and then reversing the direction of the system, no influence on the galvanometer could be observed. The effect which should be observed corresponds to the second order of the aberration. However, without compensating factors, the theory of induction phenomena shows that second order effects should be looked for in systems moving through the ether. The same may be said of other electrical experiments.

The difficulties in formulating a theory which will explain the results of all experiments involving tests to the first order of sensibility only on the assumption of either a quiescent or a convected ether, are much easier met than when second and higher orders have to be taken into consideration. Here we find what, at first sight, appear as rather startling assumptions; but it is only in this manner that present observational facts can be reconciled with a quiescent ether. With each advance in experimental refinement, theory has had to adapt itself by the adoption of new hypotheses. This has now been done up to second order phenomena for a quiescent ether. Thus far, however, no hypothesis has been brought forward to adapt specifically the theory of a quiescent ether to observations which have already been carried up to the third order of the aberration constant.

The first second order experiment was carried out by Michelson and Morley, and was an optical test in which the method of interference of two rays passing over paths mutually at right angles to one another was used. The apparent intent of the originators of this experiment was initially to look for a first order change in the aberration factor by means of a second order interference effect. The difficulty in reconciling the negative results of this test has, however, given rise to hypotheses involving second order dimensional factors, so that from this point of view it becomes a second order experiment. It could not, however, show a first order change in the velocity of the moving system, which latter, referred to the velocity of light, is taken as a magnitude of the first order, and hence the former change would count as a second order magnitude. In this experiment the entire system was mounted on a float so that the optical system could be rotated consecutively through all quadrants of the circle while the interference bands were being continuously observed. If now the difference in time of passage of one of the rays, say along the line of drift, and the other at right angles to it, is calculated on the basis of a moving ether, we find it to be equivalent to the time of passage over a length corresponding to a diminution of this length, in the direction of drift, proportional to the square of the aberration. Their results show that had there been an effect, it must have been probably sixteen times, certainly eight times, less than that calculated. It is understood that Morley and Miller will soon report as the result of a repetition, during the present year, of this experiment on a much larger scale, that, if there is any effect, it must be one hundred times less than the calculated value. This result is entirely consistent with a moving ether, but seemingly contradictory to a quiescent ether, as proposed by Fresnel. Apparently, then, either some condition in the fundamental hypothesis of such a medium has been overlooked, or a supplementary hypothesis must be imagined. Similar hypotheses were conceived of by both Lorentz and Fitzgerald independently, shortly after the publication of the experiments of Michelson and Morley in 1887. They assume that a contraction in the direction of motion takes place in a system moving through the ether, so that this dimension is reduced by a fraction of itself equal to one half the square of the constant of aberration. This of course, as an assumption, merely suggests a compensation to meet an apparent residual effect, and would be of no significance if it were impossible to incorporate such a condition into a consistent theory of ethereal action. This has been done by Lorentz and by Larmor in their theories of moving systems. Lorentz, who was the first to develop a satisfactory theory of a quiescent ether, assumes that, in all electrical and optical phenomena taking place in ponderable matter, we have to deal with charged particles, free to move in conductors, but confined in dielectrics to definite positions of equilibrium. These particles are perfectly permeable to the ether,so that they can move while the ether remains at rest.

If now we apply the ordinary electromagnetic equations of a system of bodies at rest to a system having a constant velocity of translation in addition to the velocities of its elements, the ether remaining at rest, the displacements of the electrons arising from the electric vibrations in the ether and the electric and magnetic forces are the same functions of the new system of parameters as for the case of rest, if we neglect quantities of the second order of the aberration. This theorem assumes that the distance of molecular action is confined to such excessively small distances that the difference in their local times would have no effect. An exception to this may be found in a rotary substance like quartz which, as mentioned above, has been examined by Mascart and Rayleigh to the first order with negative results, which seems to warrant the conclusion that the molecular forces are themselves altered by translation. This theory of Lorentz seems capable, then, of explaining the uniformly negative results of all the first order tests which have been described previously, without, however, necessarily establishing it finally, since we have not yet studied its adaptability to second and higher orders of the aberration.

The suggestion of a contraction, as stated above, lends itself in a similar manner and under like restrictions to that for the first order transformation. This requires the introduction of a second coefficient differing from unity by a quantity of the second order as did the coefficient used in the first transformation, but differing from the latter in that it is left indeterminate from the fact that there are no means as yet for giving it a definite value. Introducing these new parameters we again obtain a set of equations in which the velocity of translation does not explicitly appear. Such a moving system has therefore its correlate in a system at rest, the former having changed into the latter through the assumed contraction the moment motion begins. The occurrence of these coefficients as factors in the electric forces and the accelerations arising from the electric vibrations in the ether in the expression for the corresponding system at rest, necessitates that if the degree of similarity required is to exist in the two systems, the electrons must have different masses depending on whether their vibrations are parallel or perpendicular to the velocity of translation. This startling conclusion of Lorentz is borne out by what we now know of the dependence of the effective mass of an electron upon what is taking place in the ether. Such an hypothesis as this would require that Michelson and Morley's experiment should always give a negative result.

Of electrical experiments on the drift of the ether we have one second order test carried out very recently by Trouton at the suggestion of the late Professor Fitzgerald. The latter, reasoning on the condition of a magnetic field produced by a charged condenser moving edgewise to the drift of the ether, and the consequent additional supply of energy of such a system on charging, thought that this might produce a mechanical drag on charging and an opposite impulse on discharging, just as might occur if the mass of earth were to become suddenly greater. This experiment was carried out in the form of a condenser mounted upon an arm carried by a delicate suspension, with negative results. A second and more sensitive test was made later in a modified form by Trouton and Noble. Since. edge on to the drift, we have a magnetic field, while at right angles it vanishes, the energy will vary with the azimuth, and we shall have a maximum in an azimuth of 45°. A delicate suspension carrying the armature of a condenser showed no movement, although the calculated effect was ten times the limit of observation. The negative results of these experiments may be accounted for on like assumptions with that of the Michelson and Morley experiment, namely a contraction or change in the dimensions of the condenser producing corresponding changes in density and potential difference of the charge.

The assumption of a contraction suggests at once, from what we know of transparent media, the anisotropic state which such media are thrown into under dimensional strain. Rayleigh has examined this question in the case of water, carbon disulphide, and glass without result. In the case of glass his sensibility was several times the calculated second order effect, and much more in case of liquids.

The degree of refinement to which the polariscopic test lends itself is perhaps beyond that of any other instance in physical application. Here then is an opportunity to examine the question beyond what theory has anticipated, and the test has been carried so as to reach safely a third order effect, with negative results. The experiments as performed by the writer consisted in sending a beam of sunlight plane polarized at 45° to the horizon, through 28.56 meters of water in a horizontal direction and examining the same by a sensitive elliptic analyzer. On rotating the entire system from the meridian, where the one component of vibration to the drift was parallel and the other perpendicular, into a plane at right angles to the meridian where both components would be at right angles to the drift, and therefore where no differential effect would be produced, no change in the field of view could be detected. Had there been a total difference of 7.8×10^{-13} of the whole velocity between the components, the effect would have been manifest. We may, therefore, conclude that there is no third order effect. How well the various theories of a quiescent ether will lend themselves to this further adaptation remains to be seen, but undoubtedly by properly choosing the coefficients it may be done; however, any theory which does not contain explicitly the exact and complete adaptation to all orders of the aberration must certainly impress itself as highly artificial in its successive auxiliary hypotheses and approximations.

Larmor, in reference to his theory, says, "It is, in fact, found that the Maxwellian circuital equations of æthereal activity, in the ambient æther referred to axes moving along with the uniform velocity of convection, v, can be reduced to the same form as for axes at rest up to and including $\left(\frac{v}{V}\right)^2$ but not $\left(\frac{v}{V}\right)^3$ by adopting certain coefficients." "If, then, matter is for physical purposes a purely æthereal system, if it is constituted of simple polar singularities or electrons, positive and negative, in the Maxwellian æther, the nuclei of which may be either practically points or else small regions of æther with internal connections of pure constraint, the propositions above stated for the first order are extended to the second order of $\frac{v}{V}$ with the single addition of the Fitzgerald-Lorentz shrinkage in the scale of space and an equal one in the scale of time, which, being isotropic, is unrecognizable." "On such a theory as this the criticism presents itself, and was in fact at once made, that one hypothesis is needed to annul optical effects to the first order; that when these were found to be actually null to the second order, another hypothesis had to be added: and that another hypothesis would be required for the third order, while in fact there was no reason to believe that they were not exactly null to all orders. Such a train of remarks indicates that the nature of the hypothesis has been overlooked. And if indeed it could be proved that the optical effect is null up to the third order, that circumstance would not demolish the theory, but would rather point to some finer adjustment than it provides for; needless to say the attempt would indefinitely transcend existing experimental possibilities." And further, "up to the first order the electron hypothesis, that electricity is atomic, suffices by itself, as Lorentz was first to show." "Up to the second order, the hypothesis that matter is constituted electrically — of electrons — is required in addition."

The necessity in view of the present experimental data for leaving indeterminate the units of transformation is here illustrated in the theory of Larmor.

In the most recent discussion by Lorentz, the necessity of a general treatment is shown for not only the second but also the higher orders. In a consideration of transparent media, his theory attempts to show that translation would not alter interference, diffraction, or polarization. He would thus, by means of the assumption of so-called "Heaviside ellipsoids" as the shape of electrons, explain the negative results of optical experiments, as well as the observations of Kaufmann on Becquerel rays.

Attention should also be called to the recent theory of Abraham, who gives as the ratio of the axes of the moving electron $1-\frac{4}{5}\left(\frac{v}{V}\right)^2 : 1$, omitting fourth and higher orders. This would give a residual in double refraction of $\frac{1}{5}\left(\frac{v}{V}\right)^2 = 2 \times 10^{-9}$ for transparent media, which he acknowledges is difficult to reconcile with the experimental results which show no double refraction to the first order beyond this.

SECTION C — PHYSICS OF THE ELECTRON

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(*Hall 5, September 22, 3 p. m.*)

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THE RELATIONS OF PHYSICS OF ELECTRONS TO OTHER BRANCHES OF SCIENCE

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THE remarkable fertility shown by the new idea, based on the experimental fact of the discontinuous corpuscular structure of electrical charges, appears to be the most striking characteristic of the recent progress in electricity.

The consequences extend through all parts of the old physics; especially in electromagnetism, in optics, in radiant heat; they throw a new light even on the fundamental ideas of the Newtonian mechanics, and have revived the old atomistic ideas and caused them to be lifted from the rank of hypotheses to that of principles, owing to the proper relation which the laws of electrolysis have established between the discontinuous structure of matter and that of electricity.

Without seeking here to run through the whole field of their applications, I hope to indicate upon what solid foundations, both experimental and theoretical, rests at present the notion of the electron so fundamental to the new physics; to indicate the points which seem to require more complete light, and to show how vast is the synthesis which we can hope to attain, a synthesis whose main lines only are fixed to-day.

Under actual and provisional form, this synthesis constitutes an admirable instrument of research, and owing to it the questions extend in all directions. There is there a kind of New America, full of wealth yet unknown, where one can breathe freely, which invites all our activities, and which can teach many things to the Old World.

I. *The Electromagnetic Ether*

(1) *Fields and Charges.* One can say that the combined efforts of Faraday, Maxwell, and Hertz have resulted in giving us a precise knowledge of the properties of the electromagnetic ether, and of light; of a medium, homogeneous and void of matter, whose state is completely defined, with the exception of gravitation, when we know at any point the direction and magnitude of the electric and magnetic fields.

I insist, for the present, on the possibility of arriving at a conception of fields of force, as well as the related idea of electric charges, independently of all dynamics; I wish by this to imply only a knowledge of the laws of motion and of matter.

The two fields possess this property, that their divergence is zero in all parts of the ether; that is to say, the flux of electric and magnetic force is rigorously zero across a closed surface which does not contain any matter in its interior. It is in fact always matter in the ordinary sense of the word which contains and can furnish the electric charges around which the divergence of field exists whose direction varies with the sign of the charges.

In extreme cases where the electric charges appear to be most completely separated from their material support, as in the case of the cathode rays for example, the experimental fact of the granular structure of these rays and the complete indestructibility of their charge, the fact finally that cathodic particles are charges possessing the fundamental property of matter, inertia, and experiencing acceleration in the electromagnetic field, these facts do not allow us to distinguish their charge from the so-called free charge of ordinary electrified matter.

Furthermore, we shall come to the idea not only that there can be no electric charge without matter, but that, in fact, there can be no matter without electricity, an aggregation of electrical centres of the two kinds. Electrons, analogous to the cathode particles, possess almost all the known properties of matter by the fact alone that these centres are electrified. We shall see within what limits this conception can be considered sufficiently known, and if it is necessary to superimpose other properties on those which result from electrically charged centres in order to obtain a satisfactory representation of matter; the ether alone, on the contrary, never contains any electricity.

If experiment obliges us to admit the existence of electric charges, positive and negative, from the flux of electric force different from zero across a closed surface drawn entirely in the ether and containing matter, it is otherwise for the magnetic field. Experiment has never furnished an instance where a closed surface drawn in the ether was traversed by a magnetic field different from zero. One interesting phenomenon observed recently by P. Villard in the effect of an intense magnetic field on the production of the cathode rays, appears to receive a simple explanation in the hypothesis of free magnetic charges; but it is not certain that this hypothesis is necessary.

(2) *The Equations of Hertz.* The two fields, electric and magnetic, of which the ether can be the seat, are related to one another in such a manner that one of them can exist only on the condition that the other varies; all variations of an electric field produce a magnetic field; it is the displacement current of Maxwell: and all variations of the magnetic field produce an electric field; this is the phenomenon of induction discovered by Faraday. These two relations are expressed by Hertz's equations; they sum up completely our knowledge of the electromagnetic medium, and from these it results that all disturbances of this medium are propagated with the velocity of light. Hertz had the glory of proving this fact experimentally.

(3) *Energy.* We can now say that the ether is the seat of two distinct forms of energy, the electric and the magnetic, capable of transformation from the one into the other, *but only through matter as an intermediary, that is to say, by means of the electrified centres which it contains.*

In the ether alone, in fact, in the free radiation which it propagates, the electric and magnetic fields, transverse with respect to the direction of propagation, represent always equal energies in each element of volume, without oscillation of the energy from one form to the other. In the presence of matter, on the other hand, the electric energy can exist alone, and it is the motion of electrified centres which allows the transformation into magnetic energy, and *vice versa. Matter only can be the source of radiation.*

It is necessary, to the two preceding forms of energy, to add gravitation, which corresponds probably to a third mode of activity of the ether, whose connection with the two others is still obscure.

I insist here on the point that the principle of equivalence of various forms of energy, as far as the process allows of measurement, can be attained independently of all dynamical notions, by the process of using solely material systems in equilibrium.

One can find some information on this subject in a recent exposition by M. Perrin.[1]

(4) *The Theory of Lorentz.* The ether being thus completely known to us from the electromagnetic and optical point of view, the problem which follows as a continuation of the work of Maxwell and of Hertz is that of the connection between ether and matter, inert matter, the source and recipient of the radiations which the ether

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[1] I. Perrin, *Traité de chimie Physique. Les Principes.* Gauthier-Villars, Paris.

transmits. The connection sought for is furnished us by the electron or corpuscle, an electrical centre movable with respect to the ether, and carrying with it its divergent electric field.

This was the fundamental idea which caused Lorentz to conceive of the possibility of a relative displacement of electrified centres of divergence of the electric field, and of the ether considered as immovable. This displacement takes place without any change in the amount of the charge, that is to say, that the surface which is displaced in the ether with the electron is crossed by an electric flux which is completely invariable. It is the fundamental principle of the conservation of electricity, which will perhaps absorb the principle of the conservation of matter, as we cannot have matter without electricity. It is, however, probable that electricity alone is not sufficient to constitute matter.

We have actually no very precise information of the relative displacement of charges and of the ether, of electrified centres in an immovable medium, no tangible form under which we can conceive it. The attempts which have thus far been made to obtain a concrete representation, in order to give a material structure to the ether, have all been sterile of results. Perhaps there is a difficulty which belongs to the actual constitution of our minds, habituated by our secular evolution to think through matter, unable to form a concrete representation which is not material; also it seems scarcely reasonable to seek to construct a simple medium such as the ether by considering it to spring from a complex and various medium like matter. I believe it will be necessary to think *ether*, to conceive of it independently of all material representations, by means of those electromagnetic properties which put us in contact with it. I will return to this point later in reference to the mechanical theories of the ether.

If the electric charge is assumed to have a volume distribution in a portion of the medium, the principle of the conservation of electricity, and also the possibility of relative displacement of electricity and ether, makes it necessary for us, in this portion of space, to modify the equations of Hertz relative to the displacement current by the addition of a convection current, a necessary consequence of the existence of a displacement current connected with a motion of charges, and implying the production of a magnetic field by the motion of electrified bodies across the medium. This consequence of Hertz's equations has now received complete experimental confirmation.

Moreover, the experimental facts impose on these movable charges a discontinuous, granular structure, and lead to the idea of the electron as a singular region of the ether, carrying a charge equal to that of the hydrogen atom in electrolysis, but of different sign, and distributed on the surface or in the volume of the electron according as the intensity of the electric field is supposed to present, or not, a discontinuity when it crosses the surface which limits the volume occupied by the electron. Inertia, of electromagnetic origin, which we are about to refer to a similar centre, is opposed also, under the difficulty of its becoming infinite, to the hypothesis of a finite electric charge condensed in a point without extension.

The various considerations, more and more precise, all converging toward this notion of the atomic structure of charges, form the starting-point of all recent works on electricity.

II. *The Atom of Electricity*

(6) *The Electron.* The remarkable laws of electrolysis discovered by Faraday establish an intimate and necessary connection between the atomic structure of matter and that of electricity. They were sufficient to lead Helmholtz to conceive the latter as constituted of distinct, indivisible portions, elements of charge, all identical from the point of view of the quantity of electricity which they carry, and differing only in the sign. This elementary charge is equal to that carried by a monovalent atom or radical in electrolysis; a polyvalent atom or radical carries an equivalent number of such charges.

It was Johnstone Stoney who first used the word electron to designate atoms of electricity as distinct from matter, with which they combine to furnish the electrolytic ions. The presence of similar electrons combined with material atoms allows us to represent certain peculiarities of the spectrum, the existence of doublets of like frequencies; the electron, in motion, is thus considered as the origin of the emission of all luminous rays.

(7) *Gaseous Conductors.* But there are the researches on the electrical conductivity of gases, which have presented to us in a forcible manner the idea of electrical atoms, which have made this notion more tangible by allowing us to count these electric centres, to lay hold of them individually, and to measure for the first time the charge of each of them in absolute value.

As early as 1882, Giese, in observing the peculiarities of the conductivity of gases escaping from flames, the departure from Ohm's law, the impossibility of drawing from the gas, whatever might be the electric field employed, more than a limited amount of electricity of each kind, the progressive recombination of the free charges in the gas, had expressed in a precise manner the idea, that as in electrolytes the free electric charges in a gas are carried by distinct positive and negative centres in limited numbers, capable of moving in opposite directions under the action of an external electric field in order to discharge the electrified body which produces the field.

It is difficult, in fact, to conceive how, on the hypothesis that the charges are distributed in a continuous manner in space, a mass of gas electrically neutral could furnish a limited quantity of electricity of each kind, decreasing with the time by progressive recombination if one delays the establishment of the electric field in the gas.

It is indeed necessary to admit, for the two electricities, a discontinuous structure in order to allow their coexistence without completely neutralizing one another. The progressive recombination of the charged particles or ions of two kinds would produce this neutralization at the moment of their mutual collisions.

The phenomena of the saturation current, of the limited quantity of free electricity in a gas, were obtained under conditions most favorable to experimental study, when, immediately after the discovery of Roentgen rays and like radiations, one had recognized their property of making the gas they traversed a conductor of electricity. The limited charge which we can extract from a gas thus modified, the velocity, finite and easily measured, with which they move under the action of an electric field, their progressive recombination, are interpreted in an admirable manner on the hypothesis that the radiations, as well as the intense heat agitations in a flame, dissociate a certain number of the molecules of the gas into electrified parts carrying charges of opposite kinds.

(8) *The Phenomena of Condensation.* We know how the phenomena of condensation of supersaturated water vapor in the presence of a conducting gas, already referred by R. von Helmholtz to the presence of ions, has given the preceding hypothesis a brilliant confirmation. As a result of the researches of J. J. Thomson, Townsend, C. T. R. Wilson, and H. A. Wilson, these droplets of visible water, each formed by condensation around an electrified centre, bring forward a tangible witness to the existence of these centres, and furnish a means of measuring the individual charge, present on each drop of water formed, and equal to about 3.4×10^{-10} electrostatic units of electricity according to the recent measurements of J. J. Thomson and H. A. Wilson.

The fundamental idea in these kinds of measurements, applied for the first time by Townsend to the charged drops which are produced in the presence of saturated water vapor in recently prepared gases, consists in deducing the mass of each drop from its velocity of fall under the action of gravity by means of Stokes's formula, which gives the frictional resistance of a sphere moving through a viscous medium, and which expresses the velocity of fall in terms of the radius of the drop and consequently of its mass. We can obtain from this the electric charge carried by each drop if we know the ratio of this charge to the mass.

This ratio can be obtained, as was done by Townsend and J. J. Thomson, by measuring or calculating the total mass of water carried by the droplets, considered as uniform, as well as the total quantity of electricity carried by the ions which have served as centres for the formation of the drops. The charge thus obtained by Townsend was found to be 3×10^{-10} electrostatic units for each centre in the case of gases of electrolysis, and to 6.5×10^{-10} by J. J. Thomson from the first series of measurement on gases ionized by Roentgen rays.

H. A. Wilson obtained the ratio of charge to the mass of a drop more simply by comparing the velocity of fall under the action of gravity alone with the velocity of fall in a vertical electric field. He obtained thus directly the ratio sought for. This method has the advantage of showing that the electric charges are really carried by the drops, and of separating those drops which carry a single elementary charge from those which, by diffusion of the ions toward one another, carry a double or triple charge.

Wilson gives as the mean result of his measurements 3.1×10^{-10}, a value very near to that of Townsend.

A second series of experiments by Professor J. J. Thomson, in which he used radioactive substances as sources of ionization more constant than the Crookes tube, and in which he took care to cause the drops to form on all the ions present in the gas, by producing a supersaturation of the water vapor by a rapid expansion of sufficient magnitude to cause the condensation on the ions of both kinds, gave as a mean result 3.4×10^{-10}, a value in complete agreement with the other two experimenters. The principles of thermodynamics account perfectly for the influence of electrified centres on the condensation of water vapor: the electric charge of a drop in fact diminishes the pressure of water vapor in equilibrium with it. Moreover, the least supersaturation found necessary, by C. T. R. Wilson, for the formation of drops of water on the ions, which are the same whatever may be the means of producing them (Roentgen rays, Becquerel rays, brush discharge, action of ultra-violet light on metal negatively charged), allows us by purely thermodynamical reasoning to calculate approximately the charge carried by each of the ions, and this calculation, entirely distinct from direct measurement, gives in the case of the positive centres a value of 4×10^{-10} E. S. units.

(9) *The Radiation Integral.* More surprising still is the result recently obtained by H. A. Lorentz, who succeeded in basing a precise measurement of the elementary charges carried by the electrified centres present in metals on the experimental study of the radiation integral or black body radiation.

We will see how the emission and absorption of heat- and light-waves by matter are dependent on the presence in it of electrons in motion. The ratio, for a radiation of given wave-length, between the emissive and absorptive power, a ratio independent of the nature of the substance, represents the emissive power of the radiation integral, which bolometric measurements give directly.

Now this ratio can be calculated, as Lorentz has shown, for wavelengths which are long in comparison with the mean path of free electrons in the metal, as a function of the charge carried by each of them. The comparison of these results with those of Kurlbaum furnishes an entirely new method of obtaining this charge, and gives 3.7×10^{-10} E. S. units.

(10) *The Kinetic Theory.* Finally, the last confirmation, which states more precisely still our knowledge of the electric atom, and our confidence in this fundamental idea, Townsend, through comparing by the simple reåsoning of the kinetic theory the velocities of ions in a gas under the action of an electric field with their coefficient of diffusion through the interior of the gas, two quantities directly measurable by experiment, has been able to demonstrate the identity of the charge of one of these gaseous ions with the electric atom of Helmholtz, the charge of a monovalent atom in electrolysis.

From this comes directly a new confirmation of the values previously obtained, for it allows us to know, owing to Townsend's results, the charge on an atom in electrolysis, and from it to deduce immediately the constant of Avogadro, the number of molecules contained in a given volume of a gas. The results are well in agreement with the values of this constant (in general a little greater), which we can directly deduce from the kinetic theory of gases.

Here is an important group of concordant indications, all of absolutely distinct origin, which show without doubt the granular structure of electric charges, and consequently the atomic structure of matter itself. The measurements which I have just enumerated allow us to establish, in great security, the hypothesis of the existence of molecular masses.

I seek to point out here this extremely remarkable result, which belongs without doubt to some fundamental property of the ether and of the electrons, that all these electrified centres, whatever may be their origin, are now identical from the point of view of the charge which they carry.

It is necessary for us to penetrate further into their properties, into their relations with material atoms, to determine their relative sizes, in order to add among others to the more exact ideas which we possess in this field, that the electrons, or negative cathode corpuscles, are all identical not only from the point of view of their charge, but also from the point of view of their dynamic properties and of their masses. We are unhappily not so well informed in regard to the positive centres.

III. *Inertia and Radiation*

(11) *The Electromagnetic Wake.*[1] Before going farther it is important to point out what we can draw from the point of view to which we have now come. Electrified centres, whose existence is experimentally proven, whose charge we know in absolute units, are movable with respect to a fixed ether defined according to the equations of Hertz, without its having been necessary for us to have recourse to dynamic principles to arrive at this point of view.

To what extent can the known properties of matter be deduced from these two ideas of the electron and the ether, and is it necessary to add something to them in order to build up a synthesis? We are going to see rapidly and definitely from our idea of the electron, how it is sufficient to represent at the same time the inertia of matter, its dynamic properties, also how it can emit and absorb the radiations which the ether transmits.

The possibility of conceiving of inertia, mass, not as a fundamental idea, but as a consequence of the laws of electromagnetism, is a conception which owes its origin to an important memoir published in 1881 by Professor J. J. Thomson.[2] He studies there, basing his assumptions on the existence of the displacement currents of Maxwell, the electromagnetic field accompanying an electrified sphere in motion. This motion implies a change in the electric field at a point fixed with respect to the medium, and this displacement current immediately produces a magnetic field according to the ideas of Maxwell. The necessity of a convection current is pointed out later. The magnetic field thus produced, identical with that of an element of current parallel to the velocity of the moving charge, is proportional at each point to that velocity, at least, if it does not approach too nearly to that of light.

The creation of a magnetic field at the time of setting the charged centre in motion implies an expenditure of energy, energy of self-induction of the convection current, proportional to a first approximation to the square of the velocity, for those velocities which are small compared to the velocity of light. It is thus an expression of the same form as that of ordinary kinetic energy. A part, at least, of the inertia of an electrified body, of its capacity for kinetic energy, is thus a consequence of its electric charge.

Moreover, the magnetic field thus produced, and the electric field as well, modified by the velocity as it approaches more nearly to that of light, constitute around the electrified centre in translation a wake which accompanies it in its translation through the ether without change so long as the velocity remains constant. It is besides necessary that an external action should intervene in order to modify the energy of this wake and consequently to increase or diminish the velocity. This implies, in the absence of all other kinetic energy than this of electromagnetic origin, corresponding to the production of the wake, by the law of Galileo on the conservation of the velocity acquired, in the absence of action of all external fields of force, that an electrified centre possesses inertia by the fact alone that it is electrified.

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[1] *Le Sillage Electro-magnétique.*

[2] J. J. Thomson, *Phil. Mag.* t. 11, p. 229. 1881.

It is the immovable ether, the electromagnetic medium, which serves as a fixed support for the axes with respect to which the principle of inertia is applicable, and of which the ordinary mechanics limits itself in affirming the existence by saying: there exists a system of axes, determined by a nearly uniform translation with respect to which the principle of Galileo is exactly verified.

(12) *The Absolute Motion.* If we are able, from the actual point of view, to conceive of the ether as supporting these Galilean axes, it does not necessarily follow that the electromagnetic phenomena enable us to arrive at this absolute motion. It seems, on the contrary, so far, that static experiments, carried on in a material system by an observer carried along with it with a uniform motion of translation, do not allow, whatever may be the degree of accuracy of observation, the detection of a relative motion of the ether with respect to matter.

Larmor, and more completely Lorentz, have shown that there exist in the system actions of electromagnetic origin; it is possible to establish in a complete manner a static correspondence (relating to the positions of equilibrium or to the black fringes in optics) between the system in motion and a system fixed with respect to the ether, by means of a change of variables which preserves for the equations of the medium for a moving system the exact form which they possess for a system at rest.

The two systems differ from one another in that the moving system is slightly contracted compared with the fixed system in the direction of the resultant motion by an amount always very small, proportional to the square of the ratio of the velocity of motion to the velocity of light. This contraction affects equally all the elements of the moving system, *i. e.* the electrons themselves, if we admit with Lorentz that the interior actions of these electrons are solely electromagnetic actions or are modified in the same manner by the translation, — with the result that observation cannot prove this contraction any more than it can prove the general dragging of the ether. These elements behave as though they belonged to a corresponding fixed system. Thus is found an explanation of the negative results of experiments undertaken to show the absolute motion of the earth, by Michelson and Morley, Lord Rayleigh, Brace, Trouton, and Noble, if one admits that all the internal forces of matter are of electromagnetic origin, and that the energy is entirely divided between the two fields, electric and magnetic.

We shall see, however, farther on that it is difficult to eliminate in this way all other forms of energy, all other forces, such as gravitation; and it would then be necessary to admit with Lorentz, in order that the correspondence between the two systems should actually subsist, that in the moved system the forces and masses of different origins are modified exactly as the electromagnetic forces and masses, an hypothesis too complicated and arbitrary in the actual state of the question.

But this does not seem to be a necessary consequence: it appears probable that these actions, foreign to electromagnetism, and necessary at the interior of the electron in order to give stability and in order to represent gravitation, and which are probably connected with one another, do not intervene in a sensible manner in the negative experiments referred to above, and that everything transpires as if the electromagnetic forces alone played a rôle, alone existed.

We shall see farther on that perhaps experiments of another kind than those referred to here, for example, some dynamic measurements bringing in a relative motion of the system moved, or some static experiments bringing in gravitation, would enable us to understand the absolute motion, the axes bound to the ether, instead of conceiving simply of their existence.

(13) *Electromagnetic Inertia.* The problem of the electromagnetic wake accompanying an electrified sphere or ellipsoid in the ether has been taken up since J. J. Thomson by Heaviside and Searle.

Max Abraham has shown their results to consist approximately of a numerical factor when, instead of supposing the body to be a conductor having a surface charge, we suppose its charge to have a uniform volume distribution.

Among the more important results contained in this solution of J. J. Thomson's problem, I will point out these: that in the case of a conducting sphere, the charge remains uniformly distributed on the surface whatever may be the velocity, and that in all cases the electric field at a distance tends to become more and more concentrated in the equatorial plane with respect to the direction of the velocity in proportion as this velocity approaches that of light.

Moreover the kinetic energy which it is necessary to expend at the moment of putting it in motion in order to create the electromagnetic wake ceases to be proportional to the square of the velocity, and increases indefinitely as the velocity approaches the velocity of light-waves; the law of the increase of this kinetic energy with the velocity, the energy of self-induction of the current to which the charged body in motion is equal, may be easily deduced by Searle's solution.

Without any other hypothesis than that of its electric charge, the electron is found to have inertia defined as capacity for kinetic energy, but with a particular law of variation of this as a function of the velocity, and this inertia appears to approach infinity as the velocity approaches that of light.

The behavior of this law depends very little on the hypothesis made as to the form of the electron and the distribution of the electric charge which it carries. In all cases it is found to be impossible to give the electron a velocity equal to that of light, at least permanently.

Instead of considering with Max Abraham the electron to be spherical at all velocities, Lorentz admits it to be spherical when at rest and to have a uniform distribution of charge; but if all internal forces are solely electromagnetic or act as such, we have the view that the electron is flattened in the direction of motion by a quantity proportional to the square of the ratio $\left(\beta=\frac{v}{V}\right)$ of its velocity to that of light, becoming an ellipsoid of revolution, the equatorial diameter remaining equal to that of the original. This leads, as we shall see, to a law of inertia different from that of an invariable sphere.

We shall likewise see that it does not appear to be necessary to assign to the electrons, the negative ones at least, any other inertia than this in order to account for the dynamic properties of the cathode rays; however, experiments are not yet sufficiently exact to allow us to infer the form of the electron itself, which depends on the law of the variation of the kinetic energy with the velocity.

(14) *Two Problems.* We have examined, so far, only the case of an electron in uniform motion in the absence of any external electromagnetic field capable of modifying the motion of the electron by giving it an acceleration.

The general problem of the connection between the ether and the electron, which probably represents the most important of the connections between ether and matter, is double.

In the first place, what is the electromagnetic disturbance in the ether accompanying any given motion of the electrons whatsoever?

In the second place, what motions would free electrons have if displaced in an external magnetic field superimposed on that which constitutes their wake?

(15) *The Velocity Wave — The Acceleration Wave.* We actually possess all the elements necessary for the solution of the first problem, in which the motion is uniform in a particular case. Lorentz has given in a very simple form the general solution by the use of a delayed potential.

Each element of the charge in motion is determined by its position, its velocity, and its acceleration at the time T, the electric and magnetic fields at the time $T+t$, on a sphere having for its centre the position at the time T and for radius the path passed over by light during the time t.

Lorentz has given in this way the expressions for the two electric and vector potentials from which the fields can be deduced by the well-known formula. The complete expressions for these fields have been given for the first time, I believe, by Lenard; I obtained them independently at the same time as Schwartzschild by putting them in the following form.

The expressions for the two fields consist of two parts: the first depends solely on the velocity of the element at the time T and contributes to form the wake (*sillage*) which accompanies the electron in its motion; I shall call this the *velocity wave.* This velocity wave, which exists only in the case of uniform motion, has its electric field always directed toward the position which the element of charge will occupy at the time $T+t$, if it had retained from the time T the velocity which it had at that moment. Schwartzschild calls this position the point of aberration. It coincides with the true position of the moving element at time T if the motion has been uniform. The other part of the two fields is proportional to the acceleration projected on the direction of propagation, and the directions of the two fields are there perpendicular to one another, and perpendicular to the radius, at the same time the two electric and magnetic fields represent equal energies per unit volume; they have all the characteristics of a *radiation* which is freely propagated in the ether. I shall call this part the *acceleration wave.* Moreover, the intensities of the fields in this case vary inversely as the distance from the centre of emission, the energy represented by this wave does not tend toward zero as the time T increases indefinitely; there is thus energy radiated to infinity by the acceleration wave.

The velocity wave, on the contrary, in which the fields vary inversely as the square of the radius Vt, does not carry any energy to infinity: the energy of the velocity wave accompanies the electron in its motion and corresponds to its kinetic energy.

(16) *Radiation implies Acceleration.* We can conclude from this that when an electrified centre experiences an acceleration, and only then, it radiates to infinity in the form of a transverse wave, electromagnetic radiation, a definite quantity of energy, proportional per unit of time to the square of the acceleration.

The origin of electromagnetic radiation, of all radiation, is, then, in the electron undergoing acceleration. It is through the electron that matter acts as the source of Hertzian or light waves. All acceleration, all change which takes place in the state of motion of electrons, result in the emission of waves. The character of the emitted waves changes naturally according as the acceleration is abrupt, discontinuous, or periodic.

In the first case, realized, for example, in the sudden stopping of the negative electrons, or corpuscles, by the anti-cathode, the radiation consists of an abrupt pulse whose thickness is equal to the product of the velocity of light into the time taken to stop them, and which gives us a good representation of the Roentgen rays or of the rays from radioactive substances.

If the acceleration is periodic, on the contrary, as in the case when the electron revolves around an electrified centre of opposite sign to itself, the acceleration is periodic, and the radiation emitted constitutes a light-wave whose length is determined by the period of revolution of the electron.

The solution of the first of the two fundamental problems thus appears complete and raises no difficulty.

IV. *Dynamics of the Electron*

(17) *Maxwell's Idea.* The inverse problem is less simple. It consists in finding the motion, the acceleration which a movable electron experiences in electric or magnetic fields of given intensities; it is, properly so to speak, the problem of the dynamics of the electron.

The equations which solve this problem ought to consist, like the equations of ordinary dynamics, of two kinds of terms: one of these dependent on the external fields, which produce their actions on the electron, and are analogous to the external forces in dynamics; the other, representing forces dependent on the motion itself, and producing a resistance to motion, similar to the forces of inertia.

The terms corresponding to external actions, the forces, have been obtained by Lorentz following a method which was the natural continuation of Maxwell's idea as to the possibility of a mechanical explanation, otherwise indeterminate, by the facts of electromagnetism. The analogy to the equations of electrodynamic induction, and to the equations of Lagrange, appeared to justify such an explanation, and it was natural to continue to look upon the ether-electron system as a mechanical system, and to apply to the motions of electrified centres Lagrange's equations, deducing thus the forces exerted on the electrons by its electric and magnetic energies considered as corresponding to the potential and kinetic energies of a mechanical system, substituted in the ether. We are thus led to apply to the medium, ether, in consideration of the fundamental notions of force and mass, which they imply, the equations of material dynamics, deduced from principles founded on observations of matter only, always taken in mass and without an appreciable amount of radiation.

(18) *Ether in Matter.* We extend thus, by a bold deduction, these principles to a region for which they have not been designed, and thus admit implicitly the possibility of a material representation of the ether. However, as I have already pointed out, an attempt at such a representation raises many difficulties, and the efforts so far made to extend these principles in a more precise manner have not been successful. The most profound attempt, that of Lord Kelvin, the gyrostatic ether, lends itself rigorously only to the representation of the propagation of periodic disturbances in the ether, but makes impossible the existence of a permanent deformation, necessary, however, for the representation of a constant electrostatic field. The gyrostats would turn back again at the end of a finite time, and the system would cease to react against a deformation which has been imposed. Moreover, it would appear impossible to include in this conception the permanent existence of electrons, centres of deformation in the medium.

To get around this difficulty, Larmor had occasion, in the material image which he proposed for the ether, to superimpose on the gyrostatic system of Lord Kelvin the properties of a perfect fluid, of which the displacements representing the magnetic field should be at each instant irrotational in order not to produce an electric field by the rotation of the gyrostats present in the medium. But a great difficulty is added to the preceding: if the motion of a fluid satisfies at every moment the condition of being irrotational for infinitely small displacements, it is not so for finite displacements, and a magnetic field could not continue to exist without giving rise to an electric field.

I believe it impossible to overcome these difficulties and to give a material image of the ether, whose properties are entirely distinct, and probably much more simple than those of matter.

(19) *Action and Reaction.* Let us, however, retain this view in order that we may meet new difficulties. By means of Lagrange's equations Lorentz obtains two external forces acting on each electron in motion, two terms representing the action of the electromagnetic field.

One force is parallel to the electrostatic field; it is the ordinary electric force, due to the superposition of the electric field produced by the electron on the external electric field: the other is perpendicular to the direction of the velocity of the electron and of the external magnetic field; it is the electromagnetic force analogous to the force of Laplace exerted by a magnetic field on an element of current, and due to the superposition on the external magnetic field of the magnetic field produced by the electron during its motion. This double result includes all the elementary laws of electromagnetism and of electrodynamics, if we consider the current in ordinary conductors as due to the displacement of electrified particles.

We easily see that the forces thus obtained, exerted on the electrons by the ether, *i. e.* on the matter which contains them, do not satisfy the principle of the equality of action and reaction, if we consider all the forces which act at the same moment on all the electrons constituting matter. In the case of a body which radiates in an unsymmetrical manner, a recoil, an acceleration, is produced which is not compensated at the same moment by an acceleration set up in another portion of the matter. Later, at the time that the emitted radiation meets an obstacle, the compensation is made (but only in a partial manner if all the radiation is not absorbed) by means of the pressure which the radiation exerts on the body which receives it; a pressure whose existence is shown by experiment.

The equality of action and reaction has never been verified in similar cases, and it adds no difficulty to this subject if we do not seek to extend the principle beyond the facts which suggested it.

(20) *Quantity of Electromagnetic Motion.* If we could nevertheless realize this extension of the principle, an extension somewhat arbitrary, we should be led not only to apply this principle to matter, but to suppose the ether to have a quantity of motion which would be that of a material system to which we compare it.

Poincaré has shown that this quantity of electromagnetic motion ought to be, at every point in the ether, in direction and in magnitude, proportional to Poynting's vector, which gives at the same time a definition of the energy transmitted through the medium.

By starting with this idea of the quantity of electromagnetic motion, Max Abraham has been able to calculate the terms, put to one side by Lorentz, which depend on the motion of the electron itself, its force of inertia, by the variation of the quantity of electromagnetic motion contained in its train. He was led for the first time, by the form of the terms which represent this force of inertia, to the notion of an unsymmetrical mass as a function of the velocity.

(21) *Quasi-Stationary Motion.* The calculation can be completely made only in the case, always realizable from the experimental point of view, where the acceleration of the electron is so small that its train can be considered at each instant as identical with that of an electron having the actual velocity, but whose motion has been uniform for a long time. This is what Abraham calls a quasi-stationary motion. In this case, the train is entirely determined at each moment by the actual velocity of the electron, also the quantity of electromagnetic motion which it contains, and consequently the variation of this quantity which represents the force of inertia. The condition of quasi-stationary motion is simply that in the neighborhood of the electron, where the quantity of electromagnetic motion is localized, the wave of acceleration may be neglected in comparison with the velocity wave.

(22) *Longitudinal Mass and Transverse Mass.* We find under these conditions that the force of inertia is proportional to the acceleration with a coefficient of proportionality analogous to mass, but which is here a function of the velocity, and increases indefinitely, like the kinetic energy, as the velocity tends to approach that of light. Moreover, this electromagnetic mass differs for the same velocity, according as the acceleration is parallel or perpendicular to the direction of the velocity. There is, corresponding to the direction, a longitudinal and a transverse mass. Mass is then no longer a scalar quantity, but has the symmetry of a tensor parallel to the velocity. No experimental fact yet allows us to verify this dissymmetry of the mass of the electrons, which becomes evident only when the velocity is of the same order as that of light, but the variation of the transverse mass with the velocity has been proven by Kaufmann for the β rays of radium, which consist of particles identical with the cathode rays. It is sufficient to compare the deviations of these rays in the electric and magnetic fields perpendicular to their direction in order to deduce, by application of the equations of the dynamics of the electron, their velocity and the ratio of the charge to the transverse mass of the particles which compose them. This ratio decreases as the velocity increases, and, if we consider as fundamental the principle of the conservation of electricity, we conclude from it an actual increase of the transverse mass according to a law easy to compare with that which the theory gives for the electromagnetic mass.

(23) *Matter of the Philosophers.* But, before discussing the result of this comparison, I wish to point out a logical difficulty raised by the course which we have followed: we are accustomed to consider as fundamental the ideas of mass and force, built up in order to represent the laws of motion of matter; we, *a priori*, conceive of mass as a perfectly invariable scalar quantity.

Now, let us suppose the possibility of a material representation of the ether: we apply to it the equations of material dynamics, and we are led to admit for the electrons, which form a part of matter, and consequently for matter itself, a dissymmetrical mass, tensorial and variable.

To what, then, should the equations of ordinary dynamics apply, and what are the ideas considered as fundamental which they imply? To an abstract matter, the matter of the philosophers, which could not be ordinary matter, since it is inseparable from electric charges, and which is probably made up of an agglomeration of electrons in periodic motion, stable under their mutual actions? Or to the ether? But we have no idea of what can be its mass or motion.

It is, indeed, rather the ether which it is necessary to consider as fundamental, and it is then natural to define it initially by those properties of it which we know, that is to say, by the electric and magnetic fields, which it is possible to arrive at, as I have already remarked, without admitting at any time the laws of dynamics, the ideas of mass and force under their ordinary form. We will find this last to be a derived and secondary idea.

V. *Electromagnetic Dynamics*

(24) *Change of Point of View.* It seems thus much more natural to reverse the conception of Maxwell and to consider the analogy which he has pointed out between the equations of electromagnetism and those of dynamics under Lagrange's form as justifying much more the possibility of an electromagnetic representation of the principles and ideas of ordinary, material mechanics, than the inverse possibility.

It is necessary then for us to solve our second problem, that of the dynamics of the electron, of its motion in a given external field, without having recourse to the principles of mechanics, by purely electromagnetic considerations.

Hertz's equations, which permit a solution of the first problem, are here not sufficient, and we have need of a more general principle, which assumes not the motion of the electrons given, but that determines it.

(25) *The Law of Stationary Energy.* We will use this principle under a form indicated by Larmor, and which we can look upon as a generalization of the known laws of electrostatics and of electrodynamics. We know that the distribution of electric charges and electric fields in a system of electrified bodies is always such that the electrostatic energy W_e, contained in the medium modified by the field, is a minimum. The analogous principle holds for the magnetic field produced by currents of given intensities. The energy W_m localized in the magnetic field is less for the real distribution of it than for all other distributions satisfying the condition that the integral around a closed line is equal to 4π times the intensities of the currents inclosed by the line.

If displacements are possible, the conductors maintained at constant potential are in stable equilibrium if the electrostatic energy is a maximum, and the currents of given intensities are likewise in stable equilibrium if the energy of their magnetic field is a maximum. In all cases of maxima and minima, an infinitely small modification of the system from the configuration of equilibrium produces a zero variation in the energy: it is stationary.

(26) *General Principle.* When, instead of remaining permanent, the state of the system is variable, and if there are represented necessarily at the same time the two kinds of fields, we seek to find how,

as in the permanent case, an expression which remains stationary, that is to say, the variation of which is zero when supposed slightly modified, can start from its real state. We are thus led to replace the energies W_e, W_m, which play this rôle in the permanent case, by an integral taken with respect to the time, and which represents not the sum of the energies, since this quantity, equal to the total energy, ought to remain constant if only electromagnetic action come in, but their difference:

$$\int_{t_0}^{t_1}(W_e - W_m)dt,$$

an integral which remains stationary for all virtual modifications of the system, such modifications being subject to the condition of disappearing at the limits t_0 and t_1 of the integral, exactly as in the analogous principle of Hamilton in mechanics. The principle of zero variation just announced, and which we will consider as the result of an induction based entirely on electromagnetic principles, allows us in fact to find three of Hertz's equations, if we admit the three others as an imposed interconnection of the system, and furnishes in the most simple manner the solution which we have obtained for the first problem by means of these equations. Moreover, the motion of the electrons supposed given only at the times t_0 t_1 comes into the integral, and the condition that this must be stationary allows us to find the law of the motion during the interval, by starting from a principle whose signification is purely electromagnetic. We obtain thus exactly the results of Max Abraham; the equations of motion contain terms which depend first on the motion of the electron, and are proportional, in the hypothesis of quasi-stationary motion, to its acceleration, having coefficients that are functions of the velocity which we will call the longitudinal and transverse masses of the electron; also some terms depending on the charge, and on the external fields, which we will call the forces, and we find that they coincide with those given by Lorentz. The external motion of the electron is thus determined by the actual electromagnetic state of the system.

(27) *The Process in the Electron.* In order to simplify the analysis and to avoid considering the motion of rotation of the electron, I will consider it as a cavity in the ether; the volume integrals which express the energies W_e, W_m of the electric and magnetic fields extend only over the space external to the surface which bounds the cavity. We can suppose as a special condition outside of the electric charge that the form of this surface is fixed, spherical for example, due to an unknown action of nature, and we find the equations of Abraham for the longitudinal and transverse masses of a spherical electron.

But we can suppose a more simple condition, implying only a fixed volume of the cavity on account of the incompressibility of the external ether; if we seek, then, what is, in the case of uniform translation, the form that the electron would spontaneously take in order to satisfy the condition of zero variation, we find precisely the oblate ellipsoidal form assumed by Lorentz, with this difference, that the equatorial diameter increases with the velocity instead of remaining constant, as Lorentz considers it; this constancy implies a diminution of the volume as the velocity increases. The equations which express in this case the variation of the longitudinal and transverse mass with the velocity are different from those of Abraham and Lorentz, although giving always an indefinite increase of the two masses as the velocity approaches that of light.

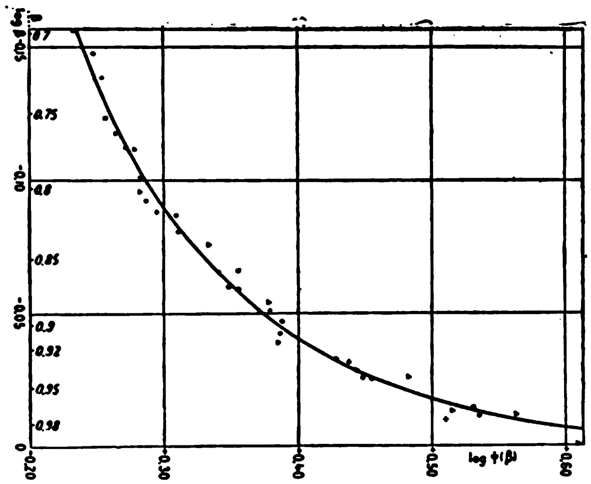
The equations thus obtained for the ratio $\frac{m}{m_0}$ of the transverse mass m, the only one so far accessible to experiment, to the mass m_0 for very small velocities, as a function of the ratio $\beta=\frac{v}{V}$ of the velocity of the electron to that of light are:
(1) Invariable spherical electron,

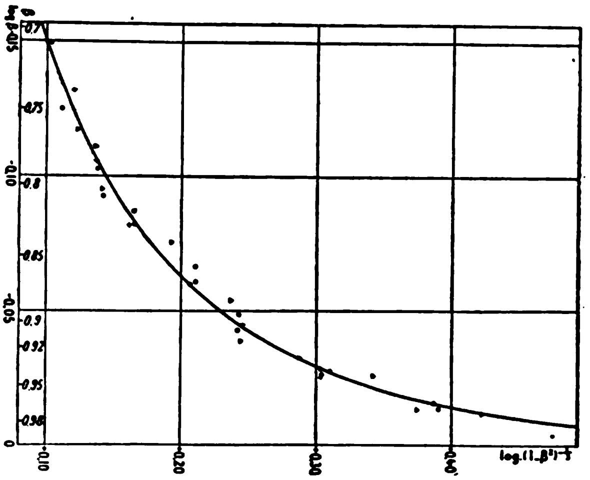
$$\frac{m}{m_0}=\frac{3}{4}\psi\ (\beta)=\frac{3}{2\beta^2}\left[\frac{1+\beta^2}{2\beta}L\frac{1+\beta}{1-\beta}-1\right]$$

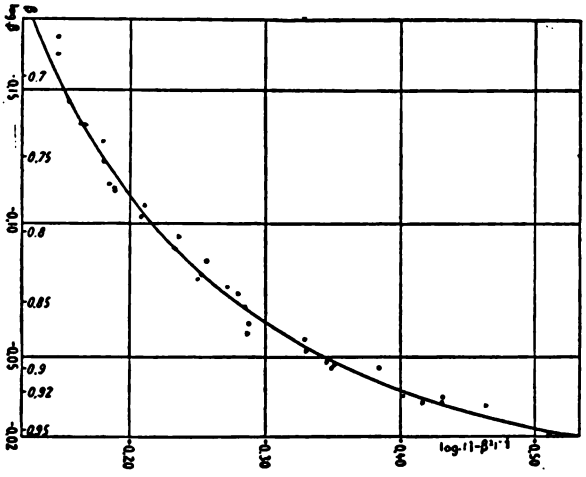
$$\text{(2) Variable Electron}\begin{cases}\text{Equatorial diameter constant } \dfrac{m}{m_0}=(1-\beta^2)^{-1/2}\\[2ex] \text{Volume constant} \qquad\qquad\qquad \dfrac{m}{m_0}=(1-\beta^2)^{-1/3}\end{cases}$$

(28) *Comparison.* The researches of Kaufmann are not yet exact enough to determine which of these equations represents most nearly the experimental variation of the ratio $\frac{e}{m}$ with the velocity. In order to make the comparison, I have used a process similar to that of Kaufmann, who eliminated the two electric and magnetic fields used to deviate the β rays, seeking to obtain the best concordance possible between the experimental variation of $\frac{e}{m}$ and the theoretical variation calculated on the hypothesis that the mass is entirely electromagnetic.

In order to make this elimination, I draw the two experimental and theoretical curves representing $\frac{e}{m}$ as a function of β, on logarithmic coördinates, and seek for what relative positions of the curves we obtain the best correspondence. The results are given for the three theoretical equations and the same series of experimental values. The experimental points corresponding to four different series are given by Kaufmann, and we see that they correspond equally well with the three theoretical curves.


log β
β
-0.15
-0.10
-0.05
0
0.7
0.75
0.8
0.85
0.9
0.92
0.95
0.98
-0.20
-0.30
-0.40
-0.50
-0.60
log ψ(β)


log β
β
-0.15
-0.10
-0.05
0
0.7
0.75
0.8
0.85
0.9
0.92
0.95
0.98
-0.10
-0.20
-0.30
-0.40
log (1-β²)^-½


log β
β
-0.15
-0.10
-0.05
-0.02
0.7
0.75
0.8
0.85
0.9
0.92
0.95
-0.20
-0.30
-0.40
-0.50
log (1-β²)^-1

The more important values from the point of view of choice of equations are those corresponding to values of the velocity very near to that of light, and which amounted to ninety-five per cent of it in Kaufmann's experiments. But the β rays are then very little deviated, and exact measurements are extremely difficult.

It would be extremely important to determine the longitudinal mass by the use of an intense electric field parallel to the velocity of the electron, furnishing to it a known energy and producing a variation of the velocity, which if measured would give the longitudinal mass.

(29) *Matter and Electrons.* But if the accuracy of experiment is not sufficient to determine completely the law, the agreement with the equations, obtained by supposing the mass to be entirely electromagnetic, is so good that we can reasonably conclude that cathode particles constituting the β rays have no mass other than that due to their electric charges or the train which they carry with them in their motion through the ether.

It is interesting to extend the same result to ordinary matter by conceiving it as made up of an aggregation of electrons of both signs; it is unreasonable on the other hand to apply to two phenomena so nearly identical as inertia of ordinary matter and that of the cathode particles, two entirely distinct explanations, of which the one, the electromagnetic explanation, is definite and confirmed by experiment, while the other remains entirely unknown.

The inertia of a similar aggregation of electrons should be equal to the sum of the partial inertias because of the great distance of the electrified centres from one another compared to their radii, which one can calculate by supposing all their inertia electromagnetic.

In these conditions, the trains of the different electrons do not interfere appreciably, and we find thus the law of the conservation of inertia as a consequence of the conservation of the electrons in the transformations to which matter is subject. But the theory is not incompatible, on account of the interference of trains, with a slight disagreement between the inertia of an assemblage and the sum of the partial inertias.

The complexity of the atomic system to which we are led, each atom of the molecule containing probably a very great number of electrons, seems also to be a necessary consequence of the complexity of the luminous spectrum sent out from the atoms, by the electrons which they contain, when an external disturbance displaces the system from its state of stable periodic motion. In such a state the radiations emitted by the various electrons on account of the acceleration which keeps them in their intermolecular orbits compensate one another almost completely from the point of view of energy radiated; so that there is in general no decay of the periodic intermolecular motion.

This conception, this electronic theory of matter in which matter becomes, at least partially, synonymous with electricity in motion, appears to account for an enormous number of facts, which increase constantly under the efforts of physicists impatient to contemplate in a less primitive form the synthesis which it promises to bring forward.

(30) *Stability of the Electron.* The fundamental conception, that of the electron, does not go without raising difficulties still further, besides the impossibility already pointed out of representing to ourselves by material images its displacement with respect to the ether. It seems necessary to admit something else in its structure than its electric charge, an action which maintains the unity of the electron and prevents its charge from being dissipated by the mutual repulsions of the elements which constitute it. The form of the electron is determined by some relation which insures its stability, the condition of incompressibility of the medium being insufficient, since the spherical form corresponds only to unstable equilibrium for an electrified body of given volume in which no force opposes the deformation.

This condition, which belongs to some fundamental property of the medium, determining the charge carried by the electrons, all identical from this point of view, is perhaps closely connected with the third mode of activity of the ether, a third form of energy, the gravitational form, of which our principle of stationary energy ought to take account by the addition of terms to those expressing the electrostatic energy, but of infinitely smaller magnitude.

(31) *Gravitation.* Gravitation remains obstinately outside of our electromagnetic synthesis; the Newtonian forces not only do not appear to be propagated with the velocity of light, but also it seems difficult to found them on electromagnetism without modifying profoundly our fundamental ideas in regard to field and quantity of electricity and the possibility of an attraction of one aggregation of neutral electrons for another aggregation of the same nature.

It appears probable that gravitation results from a mode of activity of the ether and a property of electrons entirely different from the electromagnetic mode, and we must admit besides electric and magnetic energies, a third distinct form, that of gravitation.

It remains to understand how it is possible, and what is the significance of the equivalence, the passage of this third form into one of the first two. Also we are no more capable of understanding, outside of the formal equations which express it, the connection between the electric and magnetic energies themselves and their transformations, the one into the other, by means of the electrons.

(32) *An Experiment Necessary.* It does not seem impossible to connect the forces of cohesion with electromagnetism, especially

from the point of view of the mutual attractions which orientation causes in the constitution of crystalline media, on account of the complex electric and magnetic fields which surround a system of electrons in its immediate vicinity.

Gravitational forces alone remain distinct, superimposed on the electromagnetic forces, and no difficulty comes from this on account of the negative results of the experiments undertaken to show the absolute motion of the earth.

The negative results can be explained, as we shall see, if all the internal forces of matter are of electromagnetic origin; but gravitational force, alone different, can be superimposed on them without introducing an appreciable modification of this result, for its intensity is extraordinarily small compared to electromagnetic actions, even if there is no mutual compensation between them, and in all the experiments in question, interference of light or equilibrium of an elastic system, the gravitational forces play no appreciable rôle.

It would be, indeed, important to obtain a condition in a case of equilibrium where the forces of gravity would play an important part, and if the equilibrium remains independent of the total motion to nearly the second order, if we could only observe the mutual motion to this order of precision, it would be necessary to conclude that the forces of gravitation also are modified by motion of translation in the same manner as the electromagnetic forces, since the equilibrium between the two kinds of forces is not disturbed, and this would be an important indication of the necessity of an electromagnetic representation of gravitation. We would be able, for example, if the sensibility allowed it, to perform the experiment of Trouton and Noble by suspending the condenser with a bifilar to the pan of a balance instead of by an elastic fibre.

Since this test has not been made, since experiments designed to show the absolute motion have not involved weight, it would be more reasonable to consider gravitation as a force distinct from electromagnetic action, which acts at the interior of the electrons in order to insure their stability, without its being possible actually to imagine in what manner we can seek a more profound knowledge of the ether and of the electrons which it incloses.

It does not seem, in any manner and for many reasons, that this can be of the nature of a material and mechanical representation of the ether.

VI. *Cathode Rays*

(33) *The Ratio e/m.* Before examining the consequences involved in the electronic conception of matter, I should like to examine a few points relative to the electrons of two kinds. Those which we know the better, the more intimately, are the negative electrons, which

are always identical with one another in all their properties, whatever may be the matter which has furnished them. We have already seen how the direct measurement of the charge leads always to the same result, The mass, both the longitudinal and transverse mass, having the same value for small velocities, can be determined by the measurement of the ratio of the charge to the mass.

The results obtained for this ratio in the case of cathode rays show some quite marked divergences when different methods of measurement are employed. The first values were given by J. J. Thomson by combining the magnetic deviation of the rays with a measurement of the energy which they possess by means of the heat produced in a thermoelectric couple which receives them, or by combining this magnetic deviation with the deviation in an electrostatic field. The ratio $\frac{e}{m}$ furnished by this second method, the more accurate of the two, is approximately 10^7 electromagnetic units C.G.S.

Another method first pointed out by Schuster was used successively by Kaufmann and Simon. It consists in combining the magnetic deviation with the measurement of the difference of potential under which the rays are produced, considering that this difference of potential is that which exists between the cathode and anode. This hypothesis admitted, the method is capable of great accuracy, and the results which it gives appear to agree with the limiting values, for small velocities of the ratio $\frac{e}{m}$ for the β rays, although the method employed by Kaufmann in this last measurement is different from that of Schuster. The number obtained by Simon is 1.865×10^7, nearly double that of J. J. Thomson. The explanation proposed by the latter for this disagreement, according to which the cathode rays are not produced by the total difference of potential between the cathode and the anode, but originate in a region situated in front of the cathode, does not, however, appear satisfactory, since it does not account for the constancy of the results of Kaufmann and Simon when the conditions of the experiment, the difference of potential in particular, were varied between large limits.

A means of deciding the question would consist in performing a type of experiment already used by Lenard, by subjecting the cathode rays, after their production, to a supplementary and known fall of potential, and determining by the modification which would result in their magnetic deviation the initial fall of potential under which they had been produced.

(34) *The Cathode Corpuscle.* However it may be, we can, owing to the results of Kaufmann, affirm the identity of the cathode rays already found independent of the gas and the electrode contained in the Crookes tube, with the β rays of radium. The measurements

by J. J. Thomson and Lenard of the negative charges emitted by a negatively charged metallic surface under the action of light and of those spontaneously emitted by incandescent bodies also show an identity with the cathode rays. Wehnelt has recently shown that the oxides of the alkaline earths possess in an extraordinary degree this property of spontaneously emitting cathode rays at high temperatures, and furnishes a means of performing, on this particular kind of rays, simple and exact measurements.

Finally, we know that the magnitude of the Zeeman effect, in the case where the spectrum lines considered present the appearance of a normal triplet, leads to the conclusion that the light corresponding to these lines is emitted by negatively electrified centres, present in matter and having the same ratio $\frac{e}{m}$ as the cathode rays.

Moreover, the magnitude of this ratio, one thousand to two thousand times greater than for the hydrogen atom in electrolysis, leads us, as a consequence of the identity of charges established by Townsend, to consider the mass of the cathode corpuscle as one thousand times smaller at least than an atom of hydrogen; a result in perfect agreement with the conception which makes material atoms an agglomeration of electrons of two kinds. On the hypothesis that the mass is entirely of electromagnetic origin, the knowledge of the ratio $\frac{e}{m}$ gives for the electron a sufficiently small radius (10^{-13} centimeters about) in order to be, conformably to our conception also, negligible in comparison with atomic dimensions.

(35) *Flames.* The small mass of the cathode corpuscle, and the possibility of separating from matter electrified centres a thousand times smaller than the smallest atom, is confirmed by the mobility of the negative ions in flames. We obtain enormous mobility compared to that observed in gases at ordinary temperatures, and the methods of the kinetic theory of gases permits us to calculate, by means of this experimental mobility, that the movable negative centres in flames have a mass about a thousand times smaller than the hydrogen atom, and should consequently be identical with the cathode corpuscles. At ordinary temperatures the negative ions are less mobile because the cathode corpuscles surround themselves with neutral molecules by simple electrostatic attraction, and form an agglomeration which the feeble agitation allows to remain stable.

VII. *Positive Electrons — a Rays*

(36) *Goldstein Rays. a Rays.* Our knowledge of the structure of positive charges is much less advanced than for the negative. Two important cases show us the existence of positively charged particles, besides the positive ions in conducting gases, which at ordinary temperatures consist of an agglomeration of neutral molecules around

a charged centre: these are the Kanalstrahlen of Goldstein, an efflux of positive charges toward the cathode, the electric and magnetic deviations of which lead to values for the ratio of $\frac{e}{m}$ varying between wide limits, but always several thousand times smaller than for the cathode rays. The mass of these positive centres is of the order of that of the atoms. The α rays of radioactive bodies, easily absorbed, and particularly easy to observe in the case of polonium and the active bismuth of Marckwald, appear to be, in fact, Kanalstrahlen. The mass of the positively charged particles which constitute these rays is of the same order as that of the hydrogen atom, and their velocity does not exceed 20,000 to 25,000 kilometers per second, so that it is impossible to verify whether their mass is entirely electromagnetic or not. Can we consider them as electrons as simple as the negative corpuscle itself, or are they of much more complex structure; are they, for example, atoms or molecules which have lost a cathode corpuscle?

(37) *Electrons or Atoms.* On the first hypothesis, the great mass of the positive centres would lead us to assign them dimensions much smaller than the cathode corpuscles themselves, the electromagnetic mass of an electrified sphere being inversely proportional to its radius. One is thus led to the result that an electron possesses inertia, I will not say weight, inversely proportional to its radius. H. A. Wilson thinks to find an argument in favor of this conception of a very small and consequently very inert positive electron in the observation that the α rays are much less easily absorbed than the β rays of the same velocity.

Many other reasons lead us to adopt the contrary hypothesis that an α particle is very complex and little different from an atom. Rutherford has given serious reasons for identifying the α particle with the helium atom deprived of a cathode corpuscle; also Stark gives experimental reasons referring to the emission spectra of positive centres in vacuum tubes, which imply a complex structure. Finally the theory of the disruptive discharge attributes the production of cathode rays in part at least to the impact against the cathode of particles which constitute the Goldstein rays; an electron smaller than the cathode particle itself seems scarcely able to produce a surface disturbance sufficiently intense, while on the other hand, an atom, unable to penetrate another atomic structure, and projected with a high velocity, would produce by its impact a considerable local disturbance.

(38) *The Positive charge of the α Rays.* It is perhaps by this considerable disturbance produced by the α or canal rays in matter which they meet that one can explain the interesting fact that the positive charge of the α rays has not been directly shown so far by the negative charge which a polonium salt should spontaneously

acquire if it emits only α rays. However high may be the vacuum around a piece of radioactive bismuth, or polonium, it does not acquire any charge, and loses rapidly, on the contrary, its positive or negative charge. Possibly one might explain this discharge by the ionizing action of the α rays on the gas, however rare. The passage of α particles, projectiles of large dimensions, through the surface of radioactive bodies from which they come, can play the same part as the impact of Kanalstrahlen on the surface of the cathode, and cause the emission of cathode rays of very little penetrating power, whose presence would suffice, added to that of the α rays, to prevent any permanent charge of the radioactive body, whatever may be its sign.

(39) *The Positive Electrons.* If the positive centres, as we know, ought not to be represented as free electrons, it seems, however, necessary to admit the presence of probable electrons which cause the neutralization of the negative charges in the atomic structure, but which for some reason come out of this structure with extreme difficulty, contrary to what is the case for the negative centres. Moreover, it would appear necessary in order that the theory of metals, which ascribes their conductivity to the presence of free electrified centres moving under the action of a field can take account of all the facts, the Hall effect in particular, of variable sign in different metals, that the centres of two kinds coexist in the metal, free to move about in all directions. These positive centres do not appear to be the metallic atoms themselves, necessarily immovable in order to maintain the solid framework of the metal. It is possible that the positive electron, which no known action in a gas can maintain separate from the atomic material, may be free in large numbers in the entirely different medium which constitutes the metal. Many problems present themselves here on the subject of the nature of the positive charges.

VIII. *Theory of Matter. Radioactivity*

(40) *Atomic Instability.* Let us examine now a little more closely the consequences to which we are led by the conception of matter as made up of electrons of two signs, of atoms formed of electrified bodies in motion under their mutual actions. From the first, — outside of gravitation, whose intensity is infinitely small compared to the electromagnetic forces in the interior of atoms which determine all the physical and chemical changes of state, — the elementary laws of action reduce to the forces of Lorentz, which allow us, as we have seen, to calculate the acceleration to which an electron is subjected as function of the electric and magnetic fields produced by the other electrons at the point where the first electron is situated. In the case

where the acceleration is sufficient for it to radiate an appreciable energy to a distance by means of the acceleration wave, it is probably necessary to bring in, by other terms in the equation of motion of the electron, some forces by which it can receive again the energy which it radiates, and which disappear in the case of quasi-stationary motion. It does not seem, however, in any experimental case that these corrective terms can become appreciable.

From the same point of view, the electrons in periodic motion in the material atom are necessarily subject throughout their closed orbits to accelerations which are accompanied by a radiation of energy borrowed from the internal electric and magnetic energy of the atom. This radiation must be extremely small, as in the simple case of several cathode corpuscles circulating at equal distances in the same orbit, and can be compensated for by energy obtained from external radiation. We can suppose that this continual radiation, much more important naturally when the atom, as the result of external shock, is displaced from its most stable equilibrium, is a cause of decay to the atomic structure and which at the end of a certain length of time ought necessarily to give the structure a fundamental rearrangement, as a top falls when its rotation has sufficiently diminished in velocity. A condition of instability is thus reached, the consecutive rearrangement being accompanied by a violent projection of certain electrified centres from the atom. This conception furnishes at least an image of radioactive phenomena, and the successive transformations in the life of an atom, an hypothesis of which has been advanced by Rutherford. It seems, however, that it is not necessary to admit a probable decay of atomic structures, sensible only for radioactive substances. The fact that the dispersion takes place as a function of the time according to a rigorous exponential law, the quantity which is destroyed in a given time being exactly proportional to the quantity present, seems to indicate that the substance not destroyed remains identical with itself. Perhaps the reorganization of the atomic structure might result from its accidental passage through a particularly unstable configuration, the probability that a like configuration should be reproduced being independent, in the mean, of the previous history of the atom, and the mean life of the latter would be short in proportion as this probability is great.

(41) *Internal Energy and Heat set Free.* A very simple calculation shows also that the stock of energy represented by the electric and magnetic fields surrounding the electrons contained in an atom is sufficiently great to supply for ten million years the evolution of heat discovered by Curie in the radium salts. As it appears now well established that the mean life of a radium atom is of the order of a thousand years, it results that the ten-thousandth part only of this reserve of energy is utilized during this especially active period in

the life of the atom. There is then no difficulty in conceiving how the enormous evolution of heat by radium can be ascribed to its internal energy.

No atom being free from this loss of energy due to the radiation of the electrons, one ought to expect on this hypothesis of decay a universality of radioactive phenomena, the atoms which we consider as actually stable suffering only an extraordinarily slow waste.

IX. *Electric Properties*

(42) *Polarization.* It remains now to show in a few words how the preceding conceptions lend themselves easily to a representation of the principal electric and magnetic properties of matter and make possible for the first time a theory of the disruptive discharge and of metallic conduction.

A common property of all forms of matter is electrostatic polarization arising from the variation of the specific inductive power with the nature of the substance.

This polarization results in a manner quite natural by the modification which an external electric field produces in the motions of the electron which constitute the atom. This modification is caused in the mean by an excess of positive centres on the side where the field tends to displace them and by an excess of negative centres on the opposite side. The system takes then on the average an electrostatic polarization.

(43) *Corpuscular Dissociations.* If the electric field becomes sufficiently intense, as, for example, during the passage of one of those brief pulsations which constitute the Roentgen rays, or during the passage through the atomic structure of an a or β particle of very great velocity, the modification produced may be very great, a cathode corpuscle may be separated from the structure which remains positively charged; there is produced thus a corpuscular dissociation which explains the conductivity acquired by insulating mediums under the action of Roentgen or Becquerel rays, and which manifests itself especially in gases, where the electrified centres thus freed can move more easily, although by electrostatic attraction on the neutral molecules, electrically polarizable, they surround themselves with a group of molecules which accompany them during their motion.

It seems well established that the negative ions in particular, also produced in a gas, have a cathode corpuscle for centre, since the penetration of cathode rays into a gas produces in it negative ions identical with those of Roentgen rays, at least from the point of view of their mobility or of their power of condensing supersaturated water vapor. It seems, nevertheless, important to make sure, by measuring the mobility of ions produced by different causes in the interior of gases, whether the differences which appear to exist are real and are caused by the difference in the molecules which adhere to them, or are due to the electrified centres which serve as the nuclei for them.

(44) *Mobility and Recombination.* It is equally important to be able, by measurement of mobility, to follow the modification which a change of temperature produces in the size of the agglomeration and to connect the ions observed at ordinary temperatures with the incomparably more mobile ions which we observe in flames, and which appear to be made up of single electrical centres, cathode corpuscles and perhaps α particles.

The rate of recombination of ions is as yet not well known in respect to the variations with pressure and temperature, although it certainly plays an essential part in the phenomena of disruptive discharge through gases at low pressures; it would be desirable if this point were better fixed.

(45) *Ionization by Impact.* Every actual theory of the disruptive discharge rests on the conception that the impact of an electrified particle in sufficiently rapid motion against a molecule can cause corpuscular dissociation.

This idea was a natural consequence of the known fact that cathode and Becquerel rays, made up of similar particles, make a gas through which they pass a conductor. If the corpuscular dissociation produces in the gas, separated from the molecule, a cathode corpuscle and a positive residue, these fragments can, if a sufficiently intense electric field exists in the gas, acquire a velocity great enough to act as β or α rays and cause from point to point a rapid increase in conductivity.

Townsend has shown how this consequence is capable of exact experimental verification, and he has found that between certain limits of velocity, each impact between the cathode corpuscle and a molecule results in a corpuscular dissociation of the same kind. The velocity acquired ought not, however, to exceed a certain limit beyond which the negative corpuscle or β particle passes through the atomic edifice without producing a sensible disturbance in it.

In order that a disruptive discharge may exist without an external cause to maintain the production of the first electrified centres, it is necessary that the positive centres should be able, like the negative, although with more difficulty, to produce corpuscular dissociation at the moment of their impact with the molecules, as this latter causes the conductivity produced in gases by the α rays.

Townsend has been able, in support of this hypothesis, to determine the exact moment when the disruptive phenomenon is produced, and to analyze the mechanism of it.

In addition to this fundamental conception of ionization by impact, the theory of the disruptive discharge has yet much progress to make.

The extremely varied aspects which this discharge takes, the production of striations, an explanation of which was first given by J. J. Thomson, the influence of a magnetic field on the conditions of the discharge, the phenomena that are produced when the electrodes are only of the order of a micron apart, where the molecules do not appear to take part in the production of the spark, are many of the essential points which to-day attract attention.

(46) *The Electric Arc.* By the side of the ordinary disruptive discharge, by brush or spark, the electric arc, with an entirely different aspect, brings in the new phenomenon of the emission of cathode corpuscles by the surface of incandescent bodies. This incandescence of the electrode, of the cathode especially, is, in fact, characteristic of the arc discharge; the cathode is raised to a sufficiently high temperature by the impact of the positive ions which flow toward it, so that the corpuscles present in the electrode, and which give it its conductivity, experience a true evaporation and carry the greater part of the current. In fact, a filament of incandescent carbon is able to emit, at a much lower temperature than that of the voltaic arc, cathode corpuscles representing a current density of two amperes per square centimeter.

(47) *Evaporation of the Cathode.* This phenomenon, known under the name of the Edison effect, is very general and has been connected in a quantitative manner by Richardson on the fundamental hypothesis of the kinetic theory with the presence of freely moving cathode particles in the interior of conductors.

At ordinary temperatures this emission of corpuscles is diminished to such an extent that electrostatics is possible and a metal can keep a permanent charge. Every corpuscle present in the metal is immersed in a medium of high specific inductive capacity, and a finite amount of work is necessary to make them pass from this medium to a region where the specific inductive capacity is equal to unity. Only the corpuscles having a sufficient velocity would be able to supply this work on leaving the conductor, and their number, absolutely negligible at ordinary temperatures, increases with extreme rapidity with the rise in temperature. Richardson has shown that the variation obtained by experiment agrees very well with that predicted by theory.

(48) *Metals.* The spontaneous dissociation of atoms which the kinetic theory implies, the separation of electrified centres free to move in the interior of the metal, is a consequence of the high specific inductive capacity of the medium, of the ease of electrostatic polarization of metals, owing to the ease with which the metallic atoms lose corpuscles in order to remain positively charged. The potential energy of an electrified particle in such a medium is much smaller than anywhere else, and conformably with the laws of the distribution of

energy given by the kinetic theory, the free particles ought to be more numerous in it.

(49) *Chemical Phenomena.* It is by an action of the same kind that water, of great specific inductive capacity (smaller, however, than that of metals) causes the electrolytic dissociation of salts that are dissolved in it; it would be of great interest to determine the relation between this electrolytic dissociation, especially of liquid conductors, and the corpuscular dissociation common probably to gases and metals.

In electrolytic dissociation, the cathode corpuscles lost by the metallic atoms, instead of remaining free as in corpuscular dissociation, remain united to an atom or to a radical to form the negative ion in electrolytes. This question touches the relations between our actual ideas and chemistry, relations still very obscure, and which it would be very important to clear up. The electric dissociation produced in gases by Roentgen rays does not appear connected with any chemical modification; however, in air all intense ionization is accompanied by the formation of ozone. Here is a domain almost entirely unexplored.

X. *Magnetic Properties*

(50) *Ampère and Weber.* However, the complex phenomena of magnetism and diamagnetism have seemed so far to lead us to expect more difficulties, although the electrons gravitating in the atom in closed orbits furnish at first sight a simple representation of the molecular currents of Ampère, capable of turning under the action of an external magnetic field in order to give birth to induced magnetism, or of reacting by induction, according to the idea of Weber, against the external field so as to make the substance diamagnetic.

Those who have tried to follow out this idea have found it so far sterile; independently, different physicists have come to the conclusion that the hypothesis of electrons in undiminished motion cannot furnish a representation of the permanent phenomena of magnetism or diamagnetism.

I am enough of a parvenu to attempt to show, contrary to the preceding opinion, that it is possible to give, by means of the electrons, an exact signification to the ideas of Ampère and Weber, to find for para- and diamagnetism completely distinct interpretations, conforming to the laws experimentally established by Curie: weak magnetism, an attenuated form of ferromagnetism, varies inversely as the absolute temperature; on the other hand diamagnetism is shown to be, in all observed cases with the exception of bismuth, rigorously independent of the temperature. The theory which I propose takes entire account of these facts and clears up at the same time the complex question of magnetic energy.

I shall give here only the principal results of this work which will, be published in full elsewhere.

(51) *Molecular Currents.* An electrified particle of charge e moving with a velocity v is equivalent to a current of moment ev. One easily deduces from this that a molecular current made up of an electron which describes in the periodic time t an orbit inclosed by the surface S is equivalent from the point of view of the magnetic field produced to a magnet of magnetic moment $M = \frac{eS}{t}$ normal to the plane of the orbit.

There would be a corresponding current for each of the electrons present in a molecule, and the magnetic moment resulting from these would be zero or different from zero, according to the degree of symmetry of the molecular structure.

(52) *Diamagnetism.* If on a group of such molecules we superimpose an external magnetic field, all the molecular currents experience a modification independent of the manner in which the superposition is obtained, whether by the establishment of the field or by motion of the molecule in a preëxisting field. The direction of this modification, due to the induction experienced by the molecular currents, corresponds always to diamagnetism, the increase of the magnetic moment being $\Delta M = -\frac{He^2}{4\pi m}S$ in the case of a circular orbit. H is the component of the magnetic field normal to the plane of the orbit and m the mass of the electron which describes the orbit.

(53) *The Magnetic Energy.* When the molecule is supposed immovable, the work necessary for the modification of the molecular currents is furnished by the electric field produced, according to the equations of Hertz, during the establishment of the magnetic field.

In the opposite case, where the modification is due to the motion of the molecules, the work is furnished to the molecular currents by the kinetic energy of the molecule or by the action of neighboring molecules. The diamagnetic modification produced at the moment of the establishment of the field continues in spite of the molecular agitation.

This modification is manifested in three distinct ways:

1. If the resulting motion of the molecules is zero, the substance is diamagnetic in the ordinary sense of the word, and the order of magnitude of the experimental diamagnetic constants is in good agreement with the hypothesis of molecular currents circulating in intra-molecular paths.

This conception leads to the law of independence established by Curie between the diamagnetic constants and the temperature or the physical state.

2. If the resulting motion of the molecules is not zero, the initial diamagnetic modification is followed by an orientation of the molecules under the action of the external field, which cause a paramagnetism to appear that masks the underlying diamagnetism, the new phenomenon being considerable compared to the first, when the symmetry permits it to appear.

In slightly paramagnetic bodies, such as gases, the heat agitation is opposed to the complete orientation of the molecular magnets, to saturation, and one finds, in seeking what permanent condition is established, the law of Curie, that the variation of paramagnetic constants is in inverse ratio to the absolute temperature.

3. Finally, the change of period of revolution in consequence of the diamagnetic modification corresponds to the Zeeman effect, as general as diamagnetism itself; iron, certain rays of which show the Zeeman effect, is diamagnetic before the orientation of the molecular magnets under the action of the external field makes it appear paramagnetic.

The orbits considered, which represent the molecular currents of Ampère, are also the circuits of zero resistance of the diamagnetism of Weber, with this remarkable peculiarity that the flux which passes through them is not constant, as Weber supposed, if the inertia of the electrons is entirely of electromagnetic origin.

I have shown, on the other hand, that the orbits of the electrons supposed circular, and described under the action of central forces, experience no deformation during the diamagnetic modification, this latter consisting only in a change of velocity of the electrons in their orbits. We can thus form an exact and simple conception of the facts of magnetism and diamagnetism by considering the molecular currents as non-deformable but movable currents, of zero resistance and of enormous self-induction, to which all the ordinary laws of induction are applicable.

XI. *Conclusion*

The rapid perspective which I have just sketched is full of promises, and I believe that rarely in the history of physics has one had the opportunity of looking either so far into the past or so far into the future. The relative importance of parts of this immense and scarcely explored domain appears different to-day from what it did in the preceding century: from the new point of view the various plans arrange themselves in a new order. The electrical idea, the last discovered, appears to-day to dominate the whole, as the place of choice where the explorer feels that he can found a city before advancing into new territories.

The mechanical facts, the most evident of all those of which matter is possessed, from the first attracted the attention of our ancestors, and led them to conceive of the notions of mass and force which appeared a long while the most fundamental, those from which all the others ought to raminate. As the means of investigation have increased, as the more hidden facts have been discovered, we have thought for a long while to be able to reduce them to the old laws, to be able in fact to find an explanation of mechanical origin.

The actual tendency, of making the electromagnetic ideas to occupy the preponderating place, is justified, as I have sought to show, by the solidity of the double base on which rests the idea of the electron; on the one hand by the exact knowledge of the electromagnetic ether which we owe to Faraday, Maxwell, and Hertz, and on the other hand by the experimental evidence brought forward by the recent investigations into the granular structure of electricity. Moreover, this assurance which we express when considering the past is increased, if it is possible, when we consider the future.

Already all views, not only of the ether, but of matter, source and receiver of luminous waves, obtain an immediate interpretation which mechanics is powerless to give, and this mechanics itself appears to-day as a first approximation, largely sufficing in all cases of motion of matter taken in mass, but for which a more complete expression must be sought in the dynamics of the electron.

Although still very recent, the conceptions of which I have sought to give a collected idea are about to penetrate to the very heart of the entire physics, and to act as a fertile germ in order to crystallize around it, in a new order, facts very far removed from one another.

Falling in ground well prepared to receive it, in the ether of Faraday, Maxwell, and Hertz, the idea of the electron, an electrified movable centre which experiment to-day allows us to lay hold of individually, constitutes the tie between the ether and matter formed of a group of electrons.

This idea has taken an immense development in the last few years, which causes it to break the framework of the old physics to pieces, and to overturn the established order of ideas and laws in order to branch out again in an organization which one foresees to be simple, harmonious, and fruitful.

PRESENT PROBLEMS OF RADIOACTIVITY

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I

Since the initial discovery by Becquerel of the spontaneous emission of new types of radiation from uranium, our knowledge of the phenomena exhibited by uranium and the other radioactive bodies has grown with great and ever increasing rapidity, and a very large mass of experimental facts has now been accumulated. It would be impossible within the limits of this article even to review briefly the more important experimental facts connected with the subject, and, in addition, such a review is rendered unnecessary by the recent publication of several treatises[1] in which the main facts of radioactivity have been dealt with in a fairly complete manner.

In the present article, an attempt will be made to discuss the more important problems that have arisen during the development of the subject and to indicate what, in the opinion of the writer, are the subjects which will call for further investigation in the immediate future.

II. *Nature of the radiations*

The characteristic radiations from the radioactive bodies are very complex, and a large amount of investigation has been necessary to isolate the different kinds of rays and to determine their specific character. The rays from the three most studied radio-elements, uranium, thorium, and radium, can be separated into three distinct types, known as the α, β, and γ rays.

The nature of the α and β rays has been deduced from observations of the deflection of the path of the rays by a magnetic and electric field. According to the electromagnetic theory, a radiation which is deflectable by a magnetic or electric field must consist of a flight of charged particles. If the amount of deflection of the rays from their path is measured when both a magnetic and an electric field of known strength is applied, the value V of the velocity of the particles and the ratio $\frac{e}{m}$ of the charge carried by the particle to its apparent mass m can be determined. From the direction of the deviation, the sign of the electric charge carried by the particle can be deduced.

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[1] Mme. Curie, *Thèses présentées à la Faculté des Sciences.* Paris, 1903.
H. Becquerel, *Recherches sur une propriété nouvelle de la Matière.* Typographie de Firmin, Didot et Cie. Paris, 1903.
E. Rutherford, *Radioactivity.* Cambridge, University Press, 1904.
F. Soddy, *Radioactivity.* Electrician Co., London, 1904.

Examined in this way, the β rays have been shown to consist of negatively charged particles projected with a velocity approaching that of light. The experiments of Becquerel and Kaufmann have shown that the β rays are identical with the cathode rays produced in a vacuum tube. This relationship has been established by showing that the value of $\frac{e}{m}$ is the same for the two kinds of rays. In both cases the value of $\frac{e}{m}$ has been found to be about 10^7 electromagnetic units, while the corresponding value of $\frac{e}{m}$ for hydrogen atoms set free in the electrolysis of water is 10^4. If the charge on the β particles — or electrons as they may be termed — is the same as that carried by the hydrogen atom, this result shows that the apparent mass of the electrons at slow speeds is about $\frac{1}{1000}$ of that of the hydrogen atom. The β particles from the radio-elements are expelled with a much greater speed than the cathode ray particles in a vacuum tube. The velocity of the β particles from radium is not the same for all particles, but varies between about 10^{10} and 3×10^{10} cms. per second. The swifter particles move with a velocity of at least 95 per cent of that of light. The emission by radium of electrons with high but different velocities has been utilized by Kaufmann to determine the variation of $\frac{e}{m}$ with speed. He found that the value of $\frac{e}{m}$ decreased with increase of velocity, showing that the apparent mass increased with the speed. By comparison of the experimental results with the mathematical theory of a moving charge, he deduced that the mass of the electrons was in all probability electromagnetic in origin, *i. e.*, the apparent mass could be explained purely in terms of electricity in motion without the necessity of a material nucleus on which the charge was distributed. J. J. Thomson, Heaviside, and others, have shown that a moving charged sphere increases in apparent mass with the speed, and that, for speeds small compared with the velocity of light, the increase of mass $m = \frac{2}{3}\frac{e^2}{a}$ where e is the charge carried by the body and a the radius of the conducting sphere over which the electricity is distributed. Kaufmann deduced that the value of $\frac{e}{m} = 1.86 \times 10^7$ for electrons of slow velocity. If the mass of the electrons is electrical in origin, it is seen that $a = 10^{-13}$ cms., since the value of $e = 3.4 \times 10^{-10}$ electrostatic units. The results of various methods of determination agree in fixing the diameter of an atom as about 10^{-8} cms. The apparent diameter of an electron is thus minute compared with that of the atom itself.

The highest velocity of the radium electrons measured by Kaufmann was 95 per cent of the velocity of light. The power of electrons of penetrating solid matter increases rapidly with the velocity, and some of those expelled from radium are able to penetrate through more than 3 mms. of lead. It is probable that a few of the electrons from radium move with a velocity still greater than the highest value observed by Kaufmann, and it is important to determine the value of $\frac{e}{m}$ and the velocity of such electrons. According to the mathematical theory, the mass of the electron increases rapidly as the speed of light is approached, and should be infinitely great when the velocity of light is reached. This leads to the conclusion that no charged body can be made to move with a velocity greater than that of light. This result is of great importance, and requires further experimental verification. A close study of the high speed electrons from radium may throw further light on this question.

Only a brief and imperfect statement of our knowledge of electrons has been given in this paper. A more complete and detailed account of both the theory and experiment will be given by my colleague, Dr. Langevin.

III. *The α rays*

The β rays are readily deflected by a magnetic field, but a very intense magnetic field is required to deflect appreciably the α rays. The writer showed by the electric method that the α rays of radium were deflected both by a magnetic and electric field, and deduced the velocity of projection of the particles and the ratio $\frac{e}{m}$ of the charge to the mass. The direction of deflection of the α rays is opposite in sense to the β rays. Since the β rays carry a negative charge, the α particles thus behave as if they carried a positive charge. The magnetic deflection of these rays was confirmed by Becquerel and Des Coudres, using the photographic method, while the latter, in addition, showed their deflection in an electric field and deduced the value of the velocity and $\frac{e}{m}$. The values obtained by Rutherford and Des Coudres were in very good agreement, considering the difficulty of obtaining a measurable deviation.

Observer	*Value of Velocity*	*Value of* $\frac{e}{m}$
Rutherford	2.5×10^9 cms. per sec.	6×10^3 electromagnetic units
Des Coudres	1.6×10^9 cms. per sec.	6×10^3 electromagnetic units

Since the value of $\frac{e}{m}$ for the hydrogen atom is 10^4, on the assumption that the α particle carries the same charge as the hydrogen atom, this result shows that the apparent mass of the α particle is about twice that of the hydrogen atom. If the α particle consists of any known kind of matter, this result indicates that it is either the atom of hydrogen or of helium. The α particles thus consist of heavy bodies projected with great velocity, whose mass is of the same order of magnitude as the helium atom and at least 2000 times as great as the apparent mass of the β particle.

If the α particles carry a positive charge, it is to be expected that the particles, falling on a body of sufficient thickness to absorb them, should under suitable conditions give it a positive charge, while the substance from which they are projected should acquire a negative charge. The corresponding effect has been observed for the β rays. The β particles from radium communicate a negative charge to the body on which they fall, while the radium from which they are emitted acquires a positive charge. This effect has been very strikingly shown by a simple experiment of Strutt. The radium compound, sealed in a small glass tube, the outer surface of which is made conducting, is insulated by a quartz rod. A simple gold-leaf electroscope is attached to the bottom of the glass tube, in order to indicate the presence of a charge. The whole apparatus is inclosed in a glass vessel, which is exhausted to a high vacuum, in order to reduce the loss of charge in consequence of the ionization of the gas by the rays. Using a few milligrams of radium bromide, the gold leaf diverges to its full extent in a few minutes and shows a positive charge. The explanation is simple. A large proportion of the negatively charged particles are projected through the glass tube containing the radium, and a positive charge is left behind. By allowing the gold leaf, when extended, to touch a conductor connected to earth, the gradual divergence of the leaves and their collapse becomes automatic, and will continue, if not indefinitely, at any rate for as long a time as the radium lasts.

When the radium is exposed under similar conditions, but unscreened in order to allow the α particles to escape, no such charging action is observed. This is not due to the equality between the number of positively and negatively charged particles expelled from the radium, for no effect is observed when the radium is temporarily freed from its power of emitting β rays by driving off the emanation by heat. The writer recently attempted to detect the charge carried by the α rays from radium by allowing them to fall on an insulated plate in a vacuum, but no appreciable charging was observed. The β rays were temporarily got rid of by heating the radium in order to drive off its emanation. There was found to be a strong surface ionization set up at the surface from which the rays emerged and the surface on which they impinged. The presence of this ionization causes the upper plate to lose rapidly a charge communicated to it. Although this action would mask to some extent the effect to be looked for, a measurable effect should have been obtained under the experimental conditions, if the α rays were expelled with a positive charge; but not the slightest evidence of a charge was observed. I understand that similar negative results have been obtained by other observers.

This apparent absence of charge carried by the α rays is very remarkable and difficult to account for. There is no doubt that the α particles *behave* as if they carried a positive charge, for several observers have shown that the α rays are deflected by a magnetic field. It is interesting, in this connection, that Wien was unable to detect that the "canal rays" carried a charge. These rays, discovered by Goldstein, are analogous in many respects to the α rays. They are slightly deflected by a magnetic and electric field, and behave like positively charged bodies atomic in size. The value of $\frac{e}{m}$ is not a constant, but depends upon the nature of the gas in the tube through which the discharge is passed. The apparent absence of charge on the α particles may possibly be explained on the supposition that a negatively charged particle (an electron) is always projected at the same time as the positively charged particle. Such electrons if they are present should be readily bent back to the surface from which they came by the action of a strong magnetic field. It will be of interest to examine whether the charge carried by the α rays can be detected under such conditions. Another hypothesis, which has some points in its favor, is that the α particles are uncharged at the moment of their expulsion, but, in consequence of their collision with the molecules of matter, lose a negative electron, and consequently acquire a positive charge. This point is at present under examination. The question is in a very unsatisfactory state, and requires further investigation.

It is remarkable that positive electricity is always associated with matter atomic in size, for no evidence has been obtained of the existence of a positive electron corresponding to the negative electron. This difference between positive and negative electricity is apparently fundamental, and no explanation of it has, as yet, been forthcoming.

The evidence that the α particles are atomic in size mainly rests on the deflection of the path of the rays in a strong magnetic and electric field. It has, however, been suggested by H. A. Wilson that the α particle may in reality be a "positive" electron, whose magnitude is minute compared with that of the negative. The electric mass of an electron for slow speeds is equal to $\frac{2}{3}\frac{e^2}{a}$. Since there is every reason to believe that the charge carried by the α particle and the electron is the same, in order that the mass of the positive electron should be about 2000 times that of the negative, it would be necessary to suppose that the radius of the sphere over which the charge is distributed is only $\frac{1}{2000}$ of that of the electron, *i. e.*, about 10^{-16} cms. The magnetic and electric deflection would be equally well explained on this view. This hypothesis, while interesting, is too far-reaching in its consequences to accept before some definite experimental evidence is forthcoming to support it. The evidence at present obtained strongly supports the view that the α particles are in reality projected matter, atomic in size. The probability that the α particle is an atom of helium is discussed later, in section VIII.

Becquerel showed that the α rays of polonium were deflected by a magnetic field to about the same extent as the α rays of radium. On account of the feeble activity of thorium and uranium, compared with radium and polonium, it has not been found possible to examine whether the α rays emitted by them are deflectable. There is little doubt, however, that the α particles of all the radio-elements are projected matter of the same kind (probably helium atoms). The α rays from the different radioactive products differ in their power of penetration of matter in the proportion of about three to one, being greatest for the α rays from the imparted or "induced" activity of radium and thorium, and least for uranium. This difference is probably mainly due to a variation of the velocity of projection of the α particles in the various cases. The interpretation of results is rendered difficult by our ignorance of the mechanism of absorption of the α rays by matter. Further experiment on this point is very much required.

It is of importance to settle whether the α particles of radium and polonium have the same ratio of $\frac{e}{m}$. Becquerel states that the amount of curvature of the α rays from polonium in a field of constant strength was the same as for the α rays from radium. This would show that the product of the mass and velocity is the same for the α particles from the two substances. The α rays of polonium, however, certainly have less penetrating power than those of radium, and presumably a smaller velocity of projection. This result would indicate that $\frac{e}{m}$ is different for the α particles of polonium and radium. It is of importance to determine accurately the ratio of $\frac{e}{m}$ and the velocity for the rays from these two substances in order to settle this important point.

IV. *The γ Rays*

In addition to the α and β rays, uranium, thorium, and radium all emit very penetrating rays known as γ rays. These rays are about 100 times as penetrating as the β rays, and their presence can be detected after passing through several centimeters of lead. Villard, who originally discovered these rays in radium, stated that they were not deflected in a magnetic field, and this result has been confirmed by other observers. Quite recently, Paschen has described some experiments which led him to believe that the γ rays are corpuscular in character, consisting of negatively charged particles (electrons) projected with a velocity very nearly equal to that of light. This conclusion is based on the following evidence. Some pure radium bromide was completely inclosed in a lead envelope 1 cm. thick, — a thickness sufficient to absorb completely the ordinary β rays emitted by radium, but which allows about half of the γ rays to escape. The lead envelope was insulated in an exhausted vessel, and was found to gain a positive charge. In another experiment, the rays escaping from the lead envelope fell on an insulated metal ring, surrounding the lead envelope. When the air was exhausted, this outer ring was found to gain a negative charge. These experiments, at first sight, indicate that the γ rays carry with them a negative charge like the β rays. In order to account for the absence of deflection of the path of the γ rays in very strong magnetic or electric fields, it is necessary to suppose that the particles have a very large apparent mass. Paschen supposes that the γ particles negative are electrons like the β particles, but are projected with a velocity so nearly equal to that of light that their apparent mass is very great.

Some experiments recently made by Mr. Eve, of McGill University, are of great interest in this connection. He found by the electric method that the γ rays set up secondary rays, in all directions, at the surface of which they emerge and also on the surface of which they impinge. These rays are of much less penetrating power than the primary rays, and are readily deflected by a magnetic field. The direction of deflection indicated that these secondary rays consisted, for the most part, of negatively charged particles (electrons) projected with sufficient velocity to penetrate through about 1 mm. of lead. In the light of these results, the experiments of Paschen receive a simple explanation without the necessity of assuming that the γ rays of radium themselves carry a negative charge. The lead envelope in his experiment acquired a positive charge in consequence of the emission of a secondary radiation consisting of negatively charged particles, projected with great velocity from the surface of the lead. The electric charge acquired by the metal ring was due to the absorption of these secondary rays by it, and the diminution of this charge in a magnetic field was due to the ease with which these secondary rays are deflected. It is thus to be expected that the envelope surrounding the radium, whether made of lead or other metal, would always acquire a positive charge, provided the metal is not of sufficient thickness to absorb all the γ rays in their passage through it.

No conclusive evidence has yet been brought forward to show that the γ rays can be deflected either in a magnetic or electric field. In this, as in other respects, the rays are very analogous to the Roentgen X rays.

According to the theory of Stokes, J. J. Thomson, and Weichert, of rays are transverse pulses set up in the ether by the sudden arrest X the motion of the cathode particles on striking an obstacle. The more sudden the stoppage, the shorter is the pulse, and the rays, in consequence, have greater power of penetrating matter. In some recent experiments Barkla found that the secondary rays set up by the X rays, on striking an obstacle, vary in intensity with the orientation of the X-ray tube, showing that the X rays exhibit the property of one-sidedness or polarization. This is the only evidence so far obtained in direct support of the wave-nature of the X rays.

If X rays are not set up when the cathode particles are stopped, conversely, it is to be expected that X rays should be set up when they are suddenly set in motion. Now this effect is not observable in an X-ray tube, since the cathode particles acquire most of their velocity, not at the cathode itself, but in passing through the electric field between the cathode and anti-cathode. It is, however, to be expected theoretically that a type of X rays should be set up at the sudden expulsion of the β particles from the radio-atoms. The rays, too, should be of a very penetrating kind, since not only is the charged particle projected with a speed approaching that of light, but the change of motion must occur in a distance comparable with the diameter of an atom.

On this view, the γ rays are a very penetrating type of X rays, having their origin at the moment of the expulsion of the β particle from the atom. If the β particle is the parent of the γ rays, the intensity of the β and γ rays should, under all conditions, be proportional to one another. I have found this to be the case, for the γ rays always accompany the β rays and, in whatever way the β-ray activity varies, the activity measured by the γ rays always varies in the same proportion. Active matter which does not emit β rays does not give rise to γ rays. For example, the radio-tellurium of Marckwald, which does not emit β rays, does not give off γ rays.

Certain differences are observed, however, in the ionizing action of γ and X rays. For example, gases and vapors like chlorine, sulphuretted hydrogen, methyl-iodine, and chloroform, when exposed to ordinary X rays, show a much greater ionization, compared with air, than is to be expected, according to the density law. On the other hand, the relative ionization of these substances by γ rays follows the density law very closely. It seemed likely that this apparent difference was due mainly to the greater penetrating power of the γ rays. This was confirmed by some recent experiments of Eve, who found that the relative conductivity of gases exposed to very penetrating X rays from a hard tube approximated in most cases closely to that observed for the γ rays. The vapor of methyl-iodine was an exception, but the difference in this case would probably disappear if

X rays could be generated of the same penetrating power as that of the γ rays.

The results so far obtained thus generally support the view that the γ rays are a type of penetrating X rays. This view is in agreement, too, with theory, for it is to be expected that very penetrating β rays should always appear with the β rays.

No evidence of the emission of a type of X rays is observed from active bodies which emit only α rays. If the α particles are initially projected with a positive charge, such rays are to be expected. Their absence supplies another piece of evidence in support of the view that the α particle is projected without a charge, but acquires a positive charge in its passage through matter.

V. *Emission of Energy by the Radioactive Bodies*

It was early recognized that a very active substance like radium emitted energy at a rapid rate, but the amount of this energy was very strikingly shown by the direct measurements of its heating effect made by Curie and Laborde. They found that one gram of radium in radioactive equilibrium emitted about 100 gram calories of heat per hour. A gram of radium would thus emit 896,000 gram calories per year, or over 200 times as much heat as is liberated by the explosion of hydrogen and oxygen to form one gram of water. They showed that the rate of heat emission was the same in solution as in the solid state, and remained constant when once the radium had reached a stage of radioactive equilibrium. Curie and Dewar showed that the rate of evolution of heat from radium was unaltered by plunging the radium into liquid air, or liquid hydrogen.

It seemed probable that the evolution of heat by radium was directly connected with its radioactivity, and the experiments of Rutherford and Barnes proved this to be the case. The heating effect of a quantity of radium bromide was first determined. The emanation was then completely driven off by heating the radium, and condensed in a small glass tube by means of liquid air. After removal of the emanation, the heat evolution of the radium in the course of about three hours fell to a minimum corresponding to one quarter of its original value, and then slowly increased again, reaching its original value after an interval of about one month. The heat emission from the emanation tube at first increased with the time, rising to a maximum value about three hours after its introduction. It then slowly decreased according to an exponential law with the time, falling to half value in about four days. If Q_{max} is the maximum heating effect of the emanation tube, the heat emission Q at any time t, after the maximum is reached, is given by

$$Q = Q_{max} e^{-\lambda t}$$

where λ is the radioactive constant of the emanation.

The curve expressing the recovery with time of the heating effect of radium from its minimum is complementary to the curve expressing the diminution of the heating effect of the emanation tube with time. The curves of decay and recovery agree within the limit of experimental error with the corresponding curves of decay and recovery of the activity of radium when measured by the a rays. Since the minimum, or non-separable activity of radium, measured by the a rays, after the emanation has been removed, is only one quarter of the maximum activity, these results indicate that the heating effect of radium is proportional to its activity measured by the a rays. It is not proportional to the activity measured by the β or γ rays, since the β or γ ray activity of radium almost completely disappears some hours after removal of the emanation.

These results have been confirmed by further observations of the distribution of the heat emission between the emanation and the successive products which arise from it. If the emanation is left for several hours in a closed tube, its activity measured by the electric method increases to about twice its initial value. This is due to the "excited activity," or in other words to the radiations from the active matter deposited on the walls of the tube by the emanation. The activity of this deposit has been very carefully analyzed, and the results show that the matter deposited by the emanation breaks up in three successive and well-marked stages. For convenience, these successive products of the emanation will be termed radium A, radium B, and radium C. The time T taken for each of these products to be half transformed, and the radiations from each product, are shown in the following table:

Product	*T*	*Radiations*
Radium		a rays
↓		
Emanation	4 days	a rays
↓		
Radium A	3 mins.	a rays
↓		
Radium B	21 mins.	no rays
↓		
Radium C	28 mins.	a, β, and γ rays

When the emanation has been left in a closed vessel for several hours, the emanation and its successive products reach a stage of approximate radioactive equilibrium, and the heating effect is then a maximum. If the emanation is suddenly removed from the tube by a current of air, the heating effect is then due to radium A, B, and C together. On account, however, of the rapidity of the change of radium A (half value in three minutes), it is experimentally very difficult to distinguish between the heating effect of the emanation and that of radium A. The curve of variation with time of the heating effect of the emanation tube after removal of the emanation is very nearly the same as the corresponding curve for the activity measured by the α rays. These results show that each of the products of radium supplies an amount of heat roughly proportional to its activity measured by the α rays. Each product loses its heating effect at the same rate as it loses its activity, showing that the heating effect is directly connected with the radioactive changes. The results indicated that the product, radium B, which does not emit rays does not supply an amount of heat comparable with the other products. This point is important, and requires more direct verification.

Since the heat emission is in all cases nearly proportional to the number of α particles expelled, the question arises whether the bombardment of these particles is sufficient to account for the heating effects observed. The kinetic energy of the α particle $\frac{1}{2}mv^2$ can be at once determined since $\frac{e}{m}$ and V are known. The following table shows the kinetic energy of the α particle deduced from the measurements of Rutherford and Des Coudres. The third column shows the number of α particles expelled from 1 gram of radium per second on the assumption that the heating effect of radium (100 gram-calories per gram per hour) is entirely due to the energy given out by the expelled α particles.

Observer	Kinetic energy	Number of α particles expelled per second from 1 gram of radium.
Rutherford	5.9×10^{-6} ergs.	2×10^{-11}
Des Coudres	2.5×10^{-6} ergs.	5×10^{-11}

This hypothesis that the heating effect of radium is due to bombardment of the α particle can be indirectly put to the test in the following way. It seems probable that each atom of radium in breaking up emits one α particle. On the disintegration theory, the residue of the atom, after the α particle is expelled, is the atom of the emanation, so that each atom of radium gives rise to one atom of the emanation. Let q be the number of atoms in each gram of radium breaking up per second. When a state of radioactive equilibrium is reached, the number N of emanation particles present is given by $N=\frac{q}{\lambda}$, where λ is the constant of change of the emanation. Now Ramsay and Soddy deduced from experiment that the volume of the emanation released from 1 gram of radium was about one cubic millimeter at atmospheric pressure and temperature. It has been experimentally deduced that there are 3.6×10^{19} molecules in one cubic centimeter of gas at ordinary pressure and temperature. The emanation obeys Boyle's law and behaves, in all respects, like a heavy gas, and we may in consequence deduce that $N = 3.6 \times 10^{16}$. Now $\lambda = 2.0 \times 10^{-6}$. Thus $q = 7.2 \times 10^{10}$. Now the particles expelled from radium in a state of radioactive equilibrium are about equally divided between four substances, viz., the radium itself, the emanation, radium *A* and *C*. We may thus conclude that the number of α particles expelled per second from 1 gram of radium in radioactive equilibrium is 2.9×10^{11}. The value deduced by this method is intermediate between the values previously obtained (see previous table) on the assumption that the heating effect is entirely due to the α particles.

I think we may conclude from the agreement of these two methods of calculation that the greater portion of the heating effect of radium is a direct result of the bombardment of the expelled α particles, and that, in all probability, about 5×10^{10} atoms of radium break up per second.

The energy carried off in the form of β and γ rays is small compared with that emitted in the form of α rays. By calculation it can be shown that the average kinetic energy of the β particle is small in comparison with that of the α particle. This result is confirmed by comparative measurements of the total ionization produced by the α and β rays, when the energy of the rays is all used up in ionizing the gas, for the total ionization produced by the β rays is small compared with that due to the α rays. The total ionization produced by the γ rays is about the same as that produced by the β rays, showing that, in all probability, the energy emitted in the form of these two types of radiation is about the same. From the point of view of the energy radiated, and of the changes which occur in the radioactive bodies, the α rays thus play a far more important rôle in radioactivity than the β or γ rays. Most of the products which arise from radium and thorium emit only α rays, while the β and γ rays appear only in the last of the series of rapid changes which take place in these bodies.

Since most of the heating effect of radium is due to the α rays, it is to be expected that all radioactive substances, which emit α rays, should also emit heat at a rate proportional to their α ray activity. On this view, both uranium and thorium should emit heat at about one millionth the rate of radium. It is of importance to determine directly the heating effect for these substances, and also for actinium radio-tellurium.

According to the disintegration theory, the α particle is expelled as a result of the disintegration of the atom of radioactive matter. While it is to be expected that a greater portion of the energy emitted should be carried off in the form of kinetic energy by the expelled particles, it is also to be expected that some energy would be radiated in consequence of the rearrangement of the components of the system after the violent ejection of one of its parts. No direct measurements have yet been made of the heating effect of the α particles, independently of the substance in which they are produced. Experiments of this character would be difficult, but would throw light on the important question of the division of the energy radiated between the expelled α ray particle and the system from which it arises.

The enormous evolution of energy by the radioactive substances is very well illustrated by the case of the radium emanation. The emanation released from 1 gram of radium in radioactive equilibrium emits during its changes an amount of energy corresponding to about 10,000 gram-calories. Now Ramsay and Soddy have shown that the volume of this emanation is about 1 cubic millimeter at standard pressure and temperature. One cubic millimeter of the emanation and its product thus emits about 10^7 gram-calories. Since 1 centimeter of hydrogen, in uniting with the proportion of oxygen required to form water, emits 3.1 gram-calories, it is seen that the emanation emits about 3 million times as much energy as an equal volume of hydrogen.

It can readily be calculated, on the assumption that the atom of the emanation has a mass 100 times that of hydrogen, that 1 pound of the emanation some time after removal could emit energy at the rate of about 8000 horse-power. This would fall off in a geometrical progression with the time, but, on an average, the amount of energy emitted during its life corresponds to 50,000 horse-power days. Since the radium is being continuously transformed into emanation, and three quarters of the total heat emission is due to the emanation and its products, a simple calculation shows that 1 gram of radium must emit during its life about 10^9 gram-calories. As we have seen, the heat emission of radium is about equally divided between the radium itself and the three other α ray products which come from it.

The heat emitted from each of the other radioactive substances while their activity lasts, should be of the same order of magnitude, but in the case of uranium and thorium the present rate of heat emission would probably continue, on an average, for a period of about 1000 million years.

VI. *Source of the Energy emitted by the Radioactive Bodies*

There has been considerable difference of opinion in regard to the fundamental question of the origin of the energy spontaneously emitted from the radioactive bodies. Some have considered that the atoms of the radio-elements act as transformers of borrowed energy. The atoms are supposed to be able, in some way, to abstract energy from the surrounding medium and to emit it again in the form of the characteristic radiations observed. Another theory, which has found favor with a number of physicists, supposes that the energy is derived from the radio-atoms themselves and is released in consequence of their disintegration. The latter theory involves the conception that the atoms of the radio-elements contain a great store of latent energy, which only manifests itself when the atom breaks up. There is no direct evidence in support of the view that the energy of the radio-elements is derived from external sources, while there is much indirect evidence against it. Some of this evidence will now be considered. There is now no doubt that the α and β rays consist of particles projected with great speed. In order for the α particle to acquire the velocity with which it is expelled, it can be calculated that it would be necessary for it to move freely between two points differing in potential by about five million volts. It is very difficult to imagine any mechanism which could suddenly impress such an enormous velocity on one of the parts of an atom. It seems much more reasonable to suppose that the α and β particles were originally in rapid motion in the atom, and, for some reason, escaped from the atomic system with the velocity they possessed at the instant of their release. There is now undeniable evidence that radioactivity is always accompanied by the production of new kinds of active matter. Some sort of chemical theory is thus required to explain the facts, whether the view is taken that the energy is derived from the atom itself or from external sources. The "external" theory of the origin of the energy was initially advanced to explain only the heat emission of radium. We have seen that this is undoubtedly connected with the expulsion of α particles from the different disintegration products of radium, and that the radium itself only supplies one quarter of the total heat emission, the rest being derived from the emanation and its further products. On such a theory it is necessary to suppose that in radium there are a number of different active substances, whose power of absorbing external energy dies away with the time, at different but definite rates. This still leaves the fundamental difficulty of the origin of these radioactive products unexplained. Unless there is some unknown source of energy in the medium which the radioactive bodies are capable of absorbing, it is difficult to imagine whence the energy demanded by the external theory can be derived. It certainly cannot be from the air itself, for radium gives out heat inside an ice calometer. It cannot be any type of rays such as the radioactive bodies emit, for the radioactivity of radium, and consequently its heating effect, are unaltered by hermetically sealing it in a vessel of lead several inches thick. The evidence, as a whole, is strongly against the theory that the energy is borrowed from external sources, and, unless a number of improbable assumptions are made, such a theory is quite inadequate to explain the experimental facts. On the other hand, the disintegration theory, advanced by Rutherford and Soddy, not only offers a satisfactory explanation of the origin of the energy emitted by the radio-elements, but also accounts for the succession of radioactive bodies. On this theory, a definite, small proportion of the atoms of radioactive matter every second becomes unstable and breaks up with explosive violence. In most cases, the explosion is accompanied by the expulsion of an α particle, in a few cases, by only a β particle, and in others by α and β particles together. On this view, there is at any time present in a radioactive body a proportion of the original matter which is unchanged and the products of the part which has undergone change. In the case of a slowly changing substance like radium, this point of view is in agreement with the observed fact that the spectrum of radium remains unchanged with its age.

The expulsion of an α or β particle or both from the atom leaves behind an atom which is lighter than before and which has different chemical and physical properties. This atom in turn becomes unstable and breaks up, and the process, once started, proceeds from stage to stage with a definite and measurable velocity in each case.

The energy radiated is, on this view, obtained at the expense of the internal energy of the radio-atoms themselves. It does not contradict the principle of the conservation of energy, for the internal energy of the products of the changes, when the process has come to an end, is supposed to be diminished by the amount of energy emitted during the changes. This theory supposes that there is a great store of internal energy in the radio-atoms themselves. This is not in disagreement with the modern views of the electronic constitution of matter, which have been so ably developed by J. J. Thomson, Larmor, and Lorentz. A simple calculation shows that the mere concentration of the electric charges, which on the electronic theory are supposed to be contained in an atom, implies a store of energy in the atom so enormous that, in comparison, the large evolution of energy from the radio-elements is quite insignificant.

Since the energy emitted from the radio-elements is for the most part kinetic in form, it is necessary to suppose that the α and β particles were originally in rapid motion in the atoms from which they are projected. The disintegration theory supposes that it is the atoms and not the molecules which break up. Such a view is necessary to explain the independence of the rate of disintegration of radioactive matter, of wide variations of temperature, and of the action of chemical and physical agents at our command. This must be conceded if the term atom is used in the ordinary chemical sense. It is, however, probable that the atoms of the radio-elements are in reality complex aggregates of known or unknown kinds of matter, which break up spontaneously. This aggregate behaves like an atom and cannot be resolved into simpler forms by external chemical or physical agencies. It breaks up, however, spontaneously with an evolution of energy enormous compared with that released in ordinary chemical changes. This question is further considered in section VIII of this paper.

The disintegration theory assumes that a small fraction of the atoms break up in unit time, but no definite explanation is, as yet, forthcoming of the causes which lead to this explosive disruption of the atom. The experimental results are equally in agreement with the view that each atom contains within itself the potentiality of its final disruption, or with the view that the disintegration is precipitated by the action of some external cause that may lead to the disintegration of the atom in the same way that a detonator is necessary to start certain explosions. The energy set free is, however, not derived from the detonator, but from the substance on which it acts. There is another general view which may possibly lead to an explanation of atomic disruption. If the atom is supposed to consist of electrons or charged bodies in rapid motion, it tends to radiate energy in the form of electromagnetic waves. If an atom is to be permanently stable, the parts of the atom must be so arranged that there is no loss of energy by electromagnetic radiation. J. J. Thomson has investigated certain possible arrangements of electrons in an atom which radiate energy extremely slowly, but which ultimately must break up in consequence of the loss of internal energy. According to present views, it is not such a matter of surprise that atoms do break up as that atoms are so stable as they appear to be. This question of the causes of disintegration is fundamental, and no adequate explanation has yet been put forward.

VII. *Radioactive Products*

Rutherford and Soddy showed that the radioactivity was always accompanied by the appearance of new types of active matter which possessed physical and chemical properties distinct from the parent radio-element. The radioactivity of these products is not permanent, but decays according to an exponential law with the time. The activity I_t and at any time t is given by $I_t=I_o e^{-\lambda t}$ where I_o is the initial activity and λ a constant. Each radioactive product has a definite change-constant which distinguishes it from all other products. These products do not arise simultaneously, but in consequence of a succession of changes in the radio-elements; for example, thorium in breaking up gives rise to *Thorium X*, which behaves as a solid substance soluble in ammonia. This in turn breaks up and gives rise to a gaseous product, the thorium emanation. The emanation is again unstable and gives rise to another type of matter which behaves as a solid and is deposited on the surface of the vessel containing the emanation. It was found that the results would be quantitatively explained on the assumption that the activity of any product at any time is the measure of the rate of production of the next product. This is to be expected, since the activity of any substance is proportional to the number of atoms which break up per second; and since each atom in breaking up gives rise to one atom of the next product together with a or β particles or both, the activity of the parent is a measure of the rate of production of the succeeding product.

Of these radioactive products, the radium emanation has been very closely studied on account of its existence in the gaseous state. It has been shown to be produced by radium at a constant rate. The amount of emanation stored up in a given mass of radium reaches a maximum value when the rate of supply of fresh emanation balances the rate of change of the emanation present.

If q be the number of atoms of emanation produced per second by the radium and N the maximum number present when radioactive equilibrium is reached, then $N = \frac{q}{\lambda}$, where λ is the constant of change of the emanation. This relation has been verified experimentally. The emanation is found to diffuse through air like a gas of heavy molecular weight. It is unattacked by chemical reagents, and in that respect resembles the inert gases of the argon family. It condenses at a definite temperature −150°C. Its constant of change is unaffected between the limits of temperature of 450°C and −180°C. Since the emanation changes into a non-volatile type of matter which is deposited on the surface of vessels, it was to be expected that the volume of the emanation should decrease according to the same law, as it lost its activity. These deductions, based on the theory, have been confirmed in a striking manner by the experiments of Ramsay and Soddy. The radium emanation was chemically isolated and found to be a gas which obeyed Boyle's law. The volume of the emanation observed was of the same order as had been predicted before its separation. The volume was found to decrease with the time according to the same law as the emanation lost its activity. Ramsay and Collie found that the emanation had a new and definite spectrum similar in some respects to that of the argon group of gases.

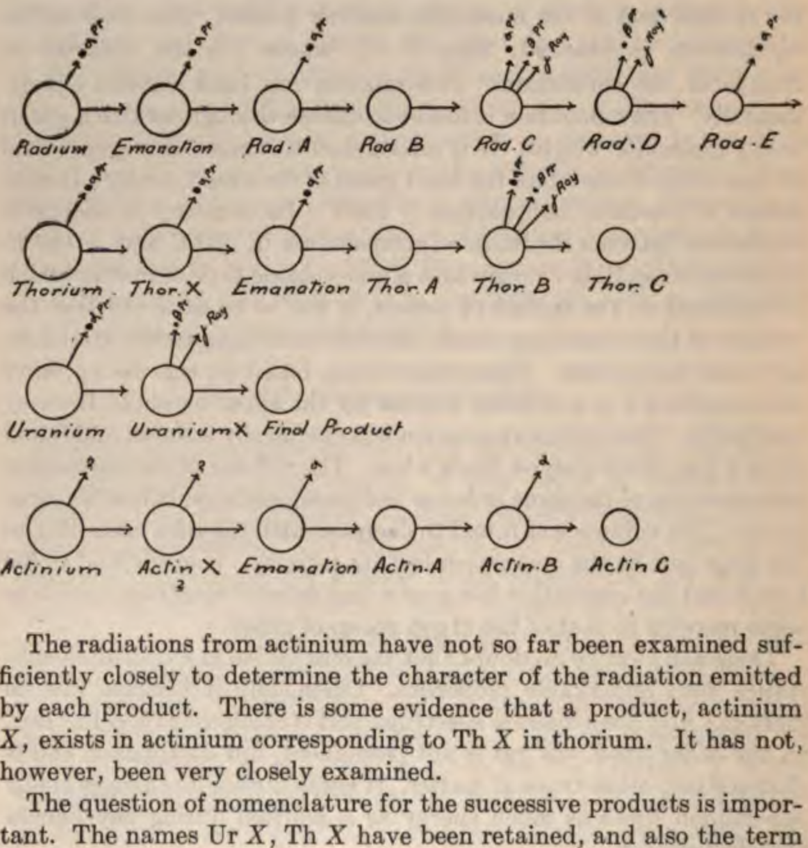
There can thus be no doubt that the emanation is a transition substance with remarkable properties. Chemically it behaves like an inert gas, and has a definite spectrum, and is condensed by cold. But, on the other hand, the gas is not permanent, but disappears, and is changed into other types of matter. It emits during its changes about one million times as much energy as is emitted during any known chemical change.

From the similarity of the behavior of the emanation of thorium and actinium to that of radium, we may safely conclude that these also are new gases which have only a limited life and change into other substances.

The non-volatile products of the radioactive bodies can be dissolved in strong acids and show definite chemical behavior in solution.

They can be partially separated by electrolysis and by suitable chemical methods. They can be volatilized by the action of high temperature, and their differences in this respect can be utilized to effect in many cases a partial separation of successive products. There can be little doubt that each of these radioactive products is a transition substance, possessing, while it lasts, some definite chemical and physical properties which serve to distinguish it from other products and from the parent element.

The radioactive products derived from each radio-element, together with the type of radiation emitted during their disintegration, are shown graphically in Fig. 1.



The radiations from actinium have not so far been examined sufficiently closely to determine the character of the radiation emitted by each product. There is some evidence that a product, actinium X, exists in actinium corresponding to Th X in thorium. It has not, however, been very closely examined.

The question of nomenclature for the successive products is important. The names Ur X, Th X have been retained, and also the term emanation. The emanation from the three radio-elements in each case gives rise to a non-volatile type of matter which is deposited on the surface of the bodies. The matter initially deposited from the radium emanation is called radium A, radium A changes into B, and B into C, and so on. A similar nomenclature is applied to the further products of the emanation of thorium and actinium. This notation is simple and elastic, and is very useful in mathematical discussion of the theory of successive changes. In the following table a list of the products is given, together with the nature of the radiation and the most marked chemical and physical properties of each product. The time T for each of the products to be half transformed is also added.

Radioactive products.	T	*Rays.*	*Some chemical and physical properties.*
URANIUM	2.5 × 10^8 years.	α	Soluble in excess of ammonium carbonate.
↓ Uranium *X*	22 days.	β, γ	Insoluble in excess of ammonium carbonate.
↓ Final product.			
THORIUM	10^9 years.	α	Insoluble in ammonia.
↓ Thorium *X*	4 days.	α	Soluble in ammonia.
↓ Emanation	1 min.	α	Inert gas condenses about −120°C.
↓ Thorium *A*	11 hours.	no rays.	Attaches itself to negative electrode, soluble in strong acids.
↓ Thorium *B*	55 mins.	α, β, γ	Separable from *A* by electrolysis.
↓ Final product.			
ACTINIUM			
↓ Actinium *X* ?			
↓ Emanation	3.9 secs.	α	Gaseous product.
↓ Actinium *A*	41 mins.	no rays.	Attaches itself to negative electrode, soluble in strong acids.
↓ Actinium *B*	1.5 mins.	α	Separable from *A* by electrolysis.
↓ Final product.			
RADIUM	1000 years.	α	
↓ Emanation	4 days.	α	Inert gas, condenses −150° C.
↓ Radium *A*	3 mins.	α	Attaches itself to negative electrode soluble in strong acids.
↓ Radium *B*	21 mins.	no rays.	Volatile at 500°C.
↓ Radium *C*	28 mins.	α, β, γ	Volatile about 1100°C.
↓ Radium *D*	about 40 years.	β, γ	Soluble in sulphuric acid.
↓ Radium *E* ↓	about 1 year.	α	Attaches itself to bismuth plate in solution, volatilizes at 1000°C.

The changes which occur in the active deposits from the emanation of radium, thorium, and actinium have been difficult to determine on account of their complexity. For example, in the case of radium, the active deposit, obtained as a result of a long exposure to the emanation, contains quantities of radium A, B, and C. The changes occurring in the active deposit of radium have been determined by P. Curie, Danne, and the writer. The value of T for the three successive changes is 3, 21, and 28 minutes respectively. Radium *A* gives only α rays, *B* gives out no rays at all, while *C* gives out α, β, and γ rays. These results have been deduced by the comparison of the change of activity with time, with the mathematical theory of successive changes. The variation of the activity with time depends upon whether the activity is measured by the α, β, or γ rays. The complicated curves are very completely explained on the hypothesis of three successive changes of the character already mentioned.

The activity of a vessel in which the radium emanation has been stored for some time rapidly falls to a very small fraction after the emanation is withdrawn. There, however, always remains a slight residual activity. The writer has recently examined the activity in detail. The residual activity at first mainly consists of β rays, and the activity measured by them does not change appreciably during the period of one year. The α ray activity is at first small, but increases uniformly with the time for the first few months that the activity has been examined. These results receive an explanation on the hypothesis that radium *C* changes into a product *D* which emits only β rays. *D* changes into a product *E*, which emits only α rays. This view has been confirmed by separating the α ray product by dipping a bismuth plate into the solution containing radium *D* and *E*. The probable period of these changes can be deduced from observations of the magnitude of the α and β ray activity at any time. It has been deduced that radium *D* is probably half transformed in forty years, and radium *E* is half transformed in about one year. The evidence at present obtained points to the conclusion that radium *E* is the active constituent present in Marckwald's radio-tellurium, and probably also in the polonium of Mme. Curie.

The changes in the active deposit of thorium have been analyzed by the writer, and the corresponding changes in actinium by Miss Brooks.

The occurrence of a "rayless change" in the active deposits from the emanation of radium, thorium, and actinium is of great interest and importance. As these products do not emit either α or β or γ rays, their presence can only be detected by their effect on the amount of the succeeding products. The action of the rayless change is most clearly brought out in the examination of the variation of activity with time of a body exposed for a very short interval in the presence of the emanations of thorium and actinium. Let us consider, for simplicity, the variation of activity with time for thorium. The activity (measured by the α rays) observed at first is very small, but gradually increases with the time, passes through a maximum, and finally decays according to an exponential law with the time falling to half value in 11 hours. The shape of this curve can be completely explained on the assumption of the two successive changes, the second of which alone gives out rays. The matter deposited on the body during the short exposure consists almost entirely of thorium A. Thorium A changes into B and the breaking up of B gives rise to the activity measured.

Let n_o = number of particles of thorium A deposited on the body during the time of exposure to the emanation.

Let P and Q be the number of particles of thorium A and B respectively at any time after removal.

Let λ_1, λ_2 be the constants of the two changes.

The number of particles of P existing at any time t is given by $P = n_o e^{-\lambda_1 t}$. If each atom of A in breaking up gives rise to one atom of B, the increase dQ in the number of Q in the time dt is given by the difference between the number of atoms of B supplied by the change in A and the number of B which break up.

Thus,
$$\frac{dQ}{dt} = \lambda_1 P - \lambda_2 Q = \lambda_1 n_0 e^{-\lambda_1 t} - \lambda_2 Q.$$

The solution of this equation is of the form $Q = ae^{-\lambda_1 t} + be^{-\lambda_2 t}$. Since for a very short exposure $Q = 0$

$$a = -b = \frac{\lambda_1}{\lambda_1 - \lambda_2}$$

and
$$Q = \frac{n_0}{\lambda_1 - \lambda_2}(c^{-\lambda_2 t} - e^{-\lambda_1 t}).$$

If thorium A does not give out rays, the activity of the body at any time after removal is proportional to Q. Thus the activity at any time t is proportional to $e^{-\lambda_2 t} - e^{-\lambda_1 t}$. Now the experimental curve of variation of activity is found to be accurately expressed by an equation of this form. A very interesting point arises in settling the values of λ_1 and λ_2 corresponding to the two changes. It is seen that the equation is symmetrical in λ_1 and λ_2 and in consequence is unaltered if the values of λ_1 and λ_2 are interchanged. Now the constant of the change is determined by the observation that the activity finally decays to half value in 11 hours. The theoretical and experimental curves are found to coincide if one of the two products is half transformed in 11 hours and the other in 55 minutes. The comparison of the theoretical and experimental curves does not, however, allow us to settle whether the period of change of thorium A is 55 minutes or 11 hours. In order to settle the point, it is necessary to find some means of separating the products thorium A and B from each other. In the case of thorium, this is done by electrolysing a solution of thorium. Pegram obtained an active product which decayed according to an exponential law with the time falling to half value in a little less than 1 hour. This result shows that the radiating product thorium B has the shorter period. In a similar way, by recourse to electrolysis, it has been found that the change of actinium B has a period of 1.5 minutes.

In the case of radium, P. Curie and Danne utilized the difference in volatility of radium *B* and *C* in order to fix the period of the changes.

It is very remarkable that the third successive product of radium, thorium, and actinium should not give out rays. It seems probable that these rayless changes are not of so violent a character as the other changes, and consist either of a rearrangement of the components of the atom or of an expulsion of an α or β particle with so slow a velocity that it fails to ionize the gas. The appearance of such changes in radioactive matter suggests the possibility that ordinary matter may also be undergoing slow "rayless changes," for such changes could not have been detected in the radio-elements, unless its succeeding products emitted rays.

It is seen that the changes occurring in radium, thorium, and actinium are of a very analogous character and indicate that each of these bodies has a very similar atomic constitution.

While there can be no doubt that numerous kinds of radioactive matter with distinct chemical and physical properties are produced in the radio-elements, it is very difficult to obtain direct evidence in some cases that the products are successive and not simultaneous. This is the case for products which have either a very slow or very rapid rate of change compared with the other product. For example, it is difficult to show directly that radium *B* is the product of radium *A* and not the direct product of the emanation. In the same way, there is no direct evidence that radium *C* is the parent of radium *D*. At the same time the successive nature of these products is indicated by indirect evidence.

There can be little doubt that each of the radioactive products is a distinct chemical substance and possesses some distinguishing physical or chemical properties. There still remains a large amount of chemical work to be done to compare and arrange the chemical properties of these products and to see if the successive products follow any definite law of variation. The electrolytic method can in many cases be used to find the position of the product in the electrochemical series. The products which change most rapidly are present in the least quantity in radium and pitchblende. Only the slower changing products like the radium emanation and radium *D* and *E* exist in sufficient quantities to be examined by the balance. It is possible that the products radium *A*, *B*, and *C* may be obtained in sufficient quantity to obtain their spectrum.

VIII. *Connection between the α Particles and Helium*

The discovery of Ramsay and Soddy that helium was produced by the radium emanation was one of the greatest interest and importance, and confirmed in a striking manner the disintegration theory of radioactivity, for the possible production of helium from radioactive matter had been predicted on this theory before the experimental evidence was forthcoming. Ramsay and Soddy found that the presence of helium could not be detected in a tube immediately after the introduction of the emanation, but was observed some time afterwards, showing that the helium arose in consequence of a slow change in the emanation itself or in its further products.

The question of the origin of the helium produced by the radium emanation and its connection with the radioactive changes occurring in the emanation is one of the greatest importance. The experimental evidence so far obtained does not suffice to give a definite answer to this question, but suggests the probable explanation. There has been a tendency to assume that the helium is the final disintegration product of the radium emanation, *i. e.*, it is the inactive substance which remains when the succession of radioactive changes in the emanation have come to an end. There is no evidence in support of such a conclusion, while there is much indirect evidence against it. It has been shown that the emanation which breaks up undergoes three fairly rapid transformations; but after these changes have occurred, the residual matter — radium *D* — is still radioactive, and breaks up slowly, being half transformed in probably about forty years. There then occurs a still further change. Taking into account the minute quantity of the radium emanation initially present in the emanation tube, the amount of the final inactive product would be insignificant after the lapse of a few days or even months. It thus does not seem probable that the helium can be the final product of the radioactive changes. In addition, it has been shown that the a particle behaves like a body of about the same mass as the helium atom. The expulsion of a few a particles from each of the heavy atoms of radium would not diminish the atomic weight of the residue very greatly. The atomic weight of the atoms of radium *D* and *E* is in all probability of the order of two hundred, since the evidence supports the conclusion that each atom expels one a particle at each transformation. In order to explain the presence of helium, it is necessary to look to the other inactive products produced during the radioactive changes. The a particles expelled from the radioactive product are themselves non-radioactive. The measurement of the ratio $\frac{e}{m}$ shows that they have an apparent mass intermediate between that of the hydrogen and helium atoms. If the a particles consist of any known kind of matter they must be either atoms of hydrogen or of helium. The actual value of $\frac{e}{m}$ has not yet been determined with an accuracy sufficient to give a definite answer to the question. On account of the very slight curvature of the path of the a particles in a strong magnetic or electric field, an accurate determination of $\frac{e}{m}$ is beset with great difficulties.

The experimental problem is still further complicated by the fact that the a particles escaping from a mass of radium have not all the same velocity, and in consequence it is difficult to draw a definite conclusion from the observed deviation of the complex pencil of rays.

The results so far obtained are not inconsistent with the view that the a particles are helium atoms, and indeed it is difficult to escape from such a conclusion. On such a view, the helium, which is gradually produced in the emanation tube, is due to the collection of a particles expelled during the disintegration of the emanation and its further products. This conclusion is supported by evidence of another character. It is known that thorium minerals like monazite sand contain a large quantity of helium. In this respect, they do not differ from uranium minerals which are rich in radium. The only common product of the different radioactive substances is the a particle, and the occurrence of helium in all radioactive minerals is most simply explained on the supposition that the a particle is a projected helium atom. This conclusion could be indirectly tested by examining whether helium is produced in other substances besides radium, for example, in actinium and polonium.

The experimental determination of the origin of helium is beset with difficulty on all sides. If the a particle is a helium atom, the total volume of helium produced in an emanation tube should be three times the initial volume of the emanation present, since the emanation in its rapid changes gives rise to three products each of which emits a particles. This is based on the assumption, which seems to be borne out by the experiments, that each atom of each product in breaking up expels one a particle. This at first sight offers a simple experimental means of settling the question, but a difficulty arises in accurately determining the volume of helium produced by a known quantity of the radium emanation. It would be expected that, if the emanation were isolated in a tube and left to stand, the volume of gas in the tube should increase with time in consequence of the liberation of helium. In one case, however, Ramsay and Soddy observed an exactly opposite result. The volume diminished with time to a small fraction of its original value. This diminution of volume was due to the decomposition of the emanation into a non-gaseous type of matter deposited on the walls of the tube, and followed the law of decrease to be expected in such a case, namely, the volume decreased according to an exponential law with the time, falling to half value in four days. The helium produced by the emanation must have been absorbed by the walls of the tube. Such a result is to be expected if the particle is a helium atom, for the a particle is projected with a velocity sufficient to bury itself in the glass to a depth of about $\frac{1}{100}$ mm. This buried helium would probably be in part released by the heating of the tube, such as occurs with the strong electric discharge employed in the spectroscopic detection of helium. Ramsay and Soddy have examined the glass tubes in which the emanation had been confined for some time, to see if the buried helium was released by heat. In some cases, traces of helium were observed.

Accurate measurements of the value of $\frac{e}{m}$ for the a particle, and also an accurate determination of the relative volume of the emanation and the helium produced by it, would probably definitely settle this fundamental question.

Certain very important consequences follow on the assumption that the a particle is, in all cases, an atom of helium. It has already been shown that the radio-elements are transformed into a succession of new substances, most of which in breaking up emit an a particle. On such a view, the atom of radium, thorium, uranium, and actinium must be supposed to be built up in part of helium atoms. In radium, at least five products of the change emit a particles, so that the radium atom must contain at least five atoms of helium. In a similar way, the atoms of actinium and thorium (or if thorium itself be not radioactive, the atom of the active substance present in it) must be compounds of helium. These compounds of helium are not stable, but spontaneously break up into a succession of substances, with an evolution of helium, the disintegration taking place at a definite but different rate at each stage. Such compounds are sharply distinguished in their behavior from the molecular compounds known to chemistry. In the first place, the radioactive compounds disintegrate spontaneously and at a rate that is independent of the physical or chemical forces at our control. Changes of temperature, which exert such a marked influence in altering the rate of molecular reactions are here almost entirely without influence. But the most striking feature of the disintegration is the expulsion, in most cases, of a product of the change with very great velocity — a result never observed in ordinary chemical reactions. This entails an enormous liberation of energy during the change, the amount, in most cases, being about one million times as great as that observed in any known chemical reaction. In order to account for the expulsion of an a or β particle with the observed velocities, it is necessary to suppose that their particles exist in a state of rapid motion in the system from which they escape. Variation of temperature, in most cases, does not seem to affect the stability of the system.

It is well established that the property of radioactivity is inherent in the radio-atoms, since the activity of any radioactive compound depends only on the amount of the element present and is not affected by chemical treatment. As far as observation has gone, both uranium and radium behave as elements in the usual accepted chemical sense.

They spontaneously break up, but the rate of their disintegration seems to be, in most cases, quite independent of chemical control. In this respect, the radioactive bodies occupy a unique position. It seems reasonable to suppose that while the radioactive substances behave chemically as elements, they are, in reality, compounds of simpler kinds of matter, held together by much stronger forces than those which exist between the components of ordinary molecular compounds. Apart from the property of radioactivity, the radio-elements do not show any chemical properties to distinguish them from the non-radioactive elements, except their very high atomic weight. The above considerations thus evidently suggest that the heavier inactive elements may also prove to be composite.

IX. *Origin of the Radio-Elements*

We have seen that the radio-elements are continuously breaking up and giving rise to a succession of new substances. In the case of uranium and thorium, the disintegration proceeds at such a slow rate that in all probability a period of about 1000 million years would be required before half the matter present is transformed. In the case of radium, however, where the process of disintegration proceeds at over one million times the rate of uranium and thorium, it is to be expected that a measurable proportion of the radium should be transformed in a single year. A quantity of radium left to itself must gradually disappear as such in consequence of its gradual transformation into other substances. This conclusion necessarily follows from the known experimental facts. The radium is continuously being transformed into the emanation which in turn is changed into other types of matter. Since there is no evidence that the process is reversible, all the radium present must, in the course of time, be transformed into emanation. The rate at which radium is being transformed can be approximately calculated either from the number of a particles expelled per second or from the observed volume of the emanation produced per second. Both methods of calculation agree in fixing that in a gram of radium about one milligram is transformed per year. From analogy with other radioactive changes, it is to be expected that the rate of change of radium would be always proportional to the amount present. The amount of radium would thus decrease exponentially with the time, falling to half value in about 1000 years. On this point of view, radium behaves in a similar way to the other known products, the only difference being that its rate of change is slower. We have already seen that, in all probability, the product radium D is half transformed in about 40 years and radium E in about one year. In regard to their rate of change, the two substances radium D and E, which are half transformed in about 40 years and 1 year respectively, occupy an intermediate position between the rapidly changing substances like radium *A*, *B*, and *C* and the slowly changing parent substance radium.

If the earth were supposed to have been initially composed of pure radium, the activity 20,000 years later would not be greater than the activity observed in pitchblende to-day. Since there is no doubt that the earth is much older than this, in order to account for the existence of radium at all in the earth, it is necessary to suppose that radium is continuously produced from some other substance or substances. On this view, the present supply of radium represents a condition of approximate equilibrium where the rate of production of fresh radium balances the rate of transformation of the radium already present. In looking for a possible source of radium, it is natural to look to the substances which are always found associated with radium in pitchblende. Uranium and thorium both fulfill the conditions necessary to be a source of radium, for both are found associated with radium and both have a rate of change slow compared with radium. At the present time, uranium seems the most probable source of radium. The activity observed in a good specimen of pitchblende is about what is to be expected, if uranium breaks up into radium. If uranium is the parent of radium, it is to be expected that the amount of radium present in different varieties of pitchblende obtained from different sources should always be proportional to the amount of uranium contained in the minerals. The recent experiments of Boltwood, Strutt, and McCoy indicate that this is very approximately the case. It is not to be expected that the relation should, in all cases, be very exact, since it is not improbable, in some cases, that a portion of the active material may be removed from the mineral, by the action of percolating water or other chemical agencies. The results so far obtained strongly support the view that radium is a product of the disintegration of uranium. It should be possible to obtain direct evidence on this question by examining whether radium appears in uranium compounds which have been initially freed from radium. On account of the delicacy of the electric test of radium by means of its emanation, the question can very readily be put to experimental trial. This has been done for uranium by Soddy and for thorium by the writer, but the results, so far obtained, are negative in character, although, if radium were produced at the rate to be expected from theory, it should very readily have been detected. Such experiments, however, taken over a period of a few months, are not decisive, for it is by no means improbable that the parent element may pass through several slow changes, possibly of a "rayless" character, before it is transformed into radium. In such a case, if these intermediate products are removed by the same chemical process from the parent element, there may be a long period of apparent retardation before the radium appears. The considerations advanced to account for radium apply equally well to actinium, which in all probability, when isolated, will prove to be an element of the same order of activity as radium. The most important problem at present in the study of radioactive minerals is not the attempt to discover and isolate new radioactive substances, but to correlate those already discovered. Some progress has already been made in reducing the number of different radioactive substances and in indicating the origin of some of them. For example, there is no doubt that the "emanating substance" of Giesel contains the same radioactive substance as the actinium of Debierne. In a similar way, there is very strong evidence that the active constituent in the polonium of Mme. Curie is identical with that in the radio-tellurium of Marckwald. The writer has recently shown that the active constituent in radio-tellurium or polonium is, in all probability, a disintegration product of radium (radium E). The same considerations apply to the radio-lead of Hofmann, which is probably identical with the product radium D. It still remains to be shown whether or no there is any direct family connection between the radioactive substances uranium, thorium, radium, and actinium. It seems probable that some at least of these substances will prove to be lineal descendants of a single parent element, in the same way that the radium products are lineal descendants of radium. The subject is capable of direct attack by a combination of physical and chemical methods, and there is every probability that a fairly definite answer will soon be forthcoming.

X. *Radioactivity of the Earth and Atmosphere*

It is now well established, notably by the work of Elster and Geitel, that radioactive matter is widely distributed both in the earth's crust and atmosphere. There is undoubted evidence of the presence of the radium emanation in the atmosphere, in spring water, and in air sucked up through the soil. It still remains to be settled whether the observed radioactivity of the earth's crust is due entirely to slight traces of the known radioactive elements or to new kinds of radioactive matter. It is not improbable that a close examination of the radioactivity of the different soils may lead to the discovery of radioactive substances which are not found in pitchblende or other radioactive minerals. The extraordinary delicacy of the electroscopic test of radioactivity renders it possible not only to detect the presence in inactive matter of extremely minute traces of a radioactive substance, but also in many cases to settle quickly whether the radioactivity is due to one of the known radio-elements.

The observations of Elster and Geitel render it probable that the radioactivity observed in the atmosphere is due to the presence of radioactive emanations or gases, which are carried to the surface by the escape of underground water and by diffusion through the soil. Indeed, it is difficult to avoid such a conclusion, since there is no evidence that any of the known constituents of the atmosphere are radioactive. Concurrently with observations of the radioactivity of the atmosphere, experiments have been made on the amount of ionization in the atmosphere itself. It is important to settle what part of this ionization is due to the presence of radioactive matter in the atmosphere. Comparisons of the relative amount of active matter and of the ionization in the atmosphere over land and sea will probably throw light on this important problem.

The wide distribution of radioactive matter in the soils which have so far been examined has raised the question whether the presence of radium and other radioactive matter in the earth may not, in part at least, be responsible for the internal heat of the earth. It can readily be calculated that the presence of radium (or equivalent amounts of other kinds of radioactive matter) to the extent of about five parts in one hundred million million by mass would supply as much heat to the earth as is lost at present by conduction to its surface. It is certainly significant that, as far as observation has gone, the amount of radioactive matter present in the soil is of this order of magnitude.

The production of helium from radium indirectly suggests a method of calculation of the age of the deposits of radioactive minerals. It seems reasonable to suppose that the helium always found associated with radioactive minerals is a product of the decomposition of the radioactive matter present. About half of the helium is removed by heating the mineral and the other half by solution. It thus does not seem likely that much of the helium found in the mineral escapes from it, so that the amount present represents the quantity produced since its formation. If the rate of the production of helium by radium (or other radioactive substance) is known, the age of the mineral can at once be estimated from the observed volume of helium stored in the mineral and the amount of radium present. All these factors have, however, not yet been determined with sufficient accuracy to make at present more than a rough estimate of the age of any particular mineral. An estimate of the rate of production of helium by radium has been made by Ramsay and Soddy by an indirect method. It can be deduced from their result that 1 gram of radium produces per year a volume of helium of about 25 cubic mms. at standard pressure and temperature. They, however, consider this to be an underestimate. On the other hand, if the α particle is a helium atom, it can readily be calculated that 1 gram of radium produces per year about 200 cubic mms. of helium.

Let us consider for example the mineral fergusonite. Ramsay and

Travers have shown that it yields about 1.8 cc. of helium per gram and contains about 7 per cent of uranium. It can readily be deduced from known data that each gram of the mineral contains about one four-millionth of a gram of radium. Supposing that one gram of radium produces $\frac{1}{5}$ cc. of helium per year, the age of the mineral is readily seen to be about 40 million years. If the above rate of production of helium by radium is an overestimate, the time will be correspondingly longer. I think there is little doubt that when the data required are accurately known this method can be applied, with considerable confidence, to determine the age of the radioactive minerals.

XI. *Radioactivity of Ordinary Matter*

The property of radioactivity is exhibited to the most marked extent by the radioactive substances found in pitchblende, but it is natural to ask the question whether ordinary matter possesses this property to an appreciable degree. The experiments that have so far been made show conclusively that ordinary matter, if it possesses this property at all, does so to a minute extent compared with uranium. It has been found that all the matter that has so far been examined shows undoubted traces of radioactivity, but it is very difficult to show that the radioactivity observed is not due to a minute trace of known radioactive matter. Even with the extraordinarily delicate methods of detection of radioactivity, the effects observed are so minute that a definite settlement of the question is experimentally very difficult. J. J. Thomson has recently given an account at the Meeting of the British Association of the work done on this subject in the Cavendish Laboratory, and has brought forward experimental evidence that strongly supports the view that ordinary matter does show specific radioactivity. Different substances were found to give out radiations that differed in quality as well as in quantity. A promising beginning has already been made, but a great deal of work still remains to be done before such an important conclusion can be considered to be definitely established.

SHORT PAPER

Professor R. A. Millikan, of the University of Chicago, presented a paper to this Section on "The Relation between the Radioactivity and the Uranium Content of Certain Minerals," of which the following is an abstract:

In March, 1904, the author, assisted by Mr. H. A. Nichols, Assistant Curator of Geology at the Field Columbian Museum (Chicago), began an investigation of the relation between the radioactivity and the uranium content of uranium-bearing minerals with a view to ascertaining whether the radioactive substances found in pitchblende are not all decomposition products of uranium. If such be the case the ratio between the uranium content and the radioactivity of uranium ought obviously to be constant, in case the assumption may be made that the active products of the decomposition are not washed out of the mineral by percolating water or other agencies.

Since the beginning of this investigation some preliminary results have been published in *Nature* by Boltwood which indicate a constancy in this ratio in the case of a few American ores which he has examined. McCoy (*cf. Ber. d. Chem. Ges.* 36, 3043) has also found a similar indication of constancy in the case of the six different kinds of uranium minerals which he has studied.

The present investigation is not yet complete, but so far as it has gone it furnishes additional evidence in support of the view that uranium is the parent of radium, for it extends somewhat the number of minerals for which the ratio between the activity and the uranium content is approximately constant. The following table gives the results thus far obtained.

Name of mineral	*Locality*	*Per cent of uranium contained*	*Activity in terms of uranium oxide*	*% uranium divided by activity*	*% of departure from mean*
Pitchblende	Colorado	59.1	3.24	18.2	4.2
Clevite	Norway	69.3	4.03	17.2	9.4
Gummite	North Carolina	55.4	2.56	21.6	13.6
Pitchblende	Cornwall, Eng.	9.23	.55	16.9	11.6
Autunite	Cornwall, Eng.	6.9	.33	20.8	9.5
Autunite	Saxony	4.0	.205	19.5	3.6
			Mean	19.0	

It will be seen that the departures from the mean value of the ratio amount in some cases to as much as 13 %, but this was found to be no more than the differences which might be obtained by "resurfacing" the same specimen of a given substance.

The measurements on activity were all made as follows: three hundred mg. of the very carefully powdered mineral were spread as uniformly as possible over three square inches of a metal sheet. This sheet was then placed upon the lower plate of an air-condenser which was connected with one pair of quadrants of an electrometer, the other pair being earthed. The condenser-plates were ten cm. on a side and 3 cm. apart. A potential of one hundred and thirty volts was applied to the upper condenser-plate, and the rate of charging of the electrometer noted. The potential to which the needle of the electrometer was charged was one hundred and twenty-five volts. The chemical analyses were all made by Mr. Nichols.

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(Prepared for the Department by the courtesy of Professor Henry Crew)

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DEPARTMENT X — CHEMISTRY

DEPARTMENT X — CHEMISTRY

(*Hall* 5, *September* 20, 4.15 *p. m.*)

Chairman: Professor James M. Crafts, Massachusetts Institute of Technology.
Speakers: Professor John U. Nef, University of Chicago.
Professor Frank W. Clarke, Chief Chemist, U. S. Geological Survey.

The Chairman of the Department of Chemistry was Professor James M. Crafts, of the Massachusetts Institute of Technology, who in opening the work of the Department spoke of the great stimulus which American chemists owed to European laboratories and the lively remembrance of the freedom of these laboratories and priceless instruction given. The application which Americans make of the scientific methods acquired abroad are characteristic of our nationality, but at the same time strongly reminiscent of other sources.

The decade within which this Congress meets has been one of extraordinary interest in the history of chemistry. I say a decade, although perhaps I should say a half-decade, since we are told by the British Premier, addressing the meeting for the advancement of science at Cambridge, that "until five years ago our race has without exception lived and died in a world of illusions." His admirably turned periods appear to signalize our old conceptions of the constitution of matter as the chief among illusions, and he seems to look forward to the immediate replacement of the false doctrine by a more idealistic conception of the universe. The atomic theory is naturally dismissed with censure, and thus we have taken away from us those blocks with which we built so happily our toy houses in the days of our innocent, childish faith. The last Faraday lecturer has been less cruel, for although he has no faith in the indivisibility of atoms, from which we can knock off electrons, nor in the individuality of the elements, his criticism is not merely negative, but, like a truly scientific engineer, he offers us a new model for our constructions. Professor Ostwald invites us to enter a beautiful stalactite cavern, groping, indeed, in some obscurity, but with the vision of a brighter light beyond.

The observations of the Röntgen and Becquerel rays have led in Germany, France, England, and Canada to a study of emanations, which has been distinguished by extreme skill in the invention of new methods and by the minute study of phenomena, which seemed even a year ago beyond the reach of human ingenuity.

The simplest statement of facts is sufficiently wonderful and mysterious. Although not more than two or three grams of radium have been gathered from the earth's crust, its natural history is already well developed, and at latest news we are told that one gram of radium bromide will evolve 0.0022 milligrams of helium in one year; that the life of a radium atom is 1050 years, or, in another experiment, 1250 years.

It may be said that within this decade the knowledge of the structure of carbon compounds has become so complete that the way to the production of the most useful bodies has become evident in theory, and I need not remind you of the consequent achievements by that happy combination of pure and industrial science in Germany.

Also within this decade, the somewhat neglected study of mineral chemistry has acquired unexpected interest by the discovery in France of metallic carbides and nitrides, formed at temperatures comparable with those of the sun, and these discoveries, besides giving rise to most unexpected industrial applications, show entirely new possibilities for the geology of the primitive rocks.

The active pursuit of physical chemistry has extended over some thirty years. Great dates are the publication, just two decades ago, of van 't Hoff's *Etudes de dynamique chimique,* and one year afterwards of Ostwald's *Allgemeine Chemie;* and, again, ten years ago *Dix Années d'une Théorie.*

Suffice it to say that the title, General Chemistry, has been amply justified. The attractive presentation of bold theories, their rapid confirmation by experiment, and the completeness of treatment by the founders of the new science have led to the immediate acceptance of their views, until the mathematical analysis of chemical phenomena has become the dominating feature of our science, and has transformed our methods of thought, as Kepler and Newton's theories transformed the study of astronomy.

ON THE FUNDAMENTAL CONCEPTIONS UNDERLYING THE CHEMISTRY OF THE ELEMENT CARBON

BY JOHN ULRIC NEF

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Two fundamental conceptions underlie our present system of carbon chemistry. First, the idea of the constant quadrivalence of carbon, which explains most adequately the existence of the vast array of carbon compounds. Second, the conception of substitution or metalepsis, which gives us a basis for interpreting many of the reactions shown by these substances.

These ideas are, however, in the light of investigations of the past twenty years, inadequate; they must be replaced by the conception of a variable valence of carbon and by the conception of dissociation in its broadest sense. A rigid application of the latter conception gives a far simpler basis for interpreting all the reactions of carbon chemistry; they are naturally also applicable to the chemistry of all the other elements.

I. *On the Valence of the Carbon Atom*

The progress of organic chemistry since 1858 is due chiefly to the development of a few very simple ideas concerning the valence of the elements, ideas which were first clearly and fully presented at that time by Kekulé.

Hydrogen, oxygen, and nitrogen are the elements which most frequently combine with carbon to form the so-called organic compounds. Since the compounds of one atom of oxygen, nitrogen, or carbon with hydrogen possess the empyrical formulæ, $O{=}H_2$, $N{\equiv}H_3$, $C{\equiv}H_4$, the conception naturally presents itself that the capacity of the various elements for holding hydrogen atoms varies. Oxygen is capable of holding two such atoms, nitrogen holds three, and carbon four atoms of hydrogen.

Therefore we assume, taking hydrogen as our unit, that the valence of the element oxygen is two, $-O-$, of nitrogen, three, $-\overset{|}{N}-$, and of carbon, four, $-\underset{|}{\overset{|}{C}}-$.

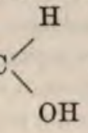
Without going into much detail concerning the nature of valence, or, what is the same thing, concerning the nature of the forces inherent in our atoms, we assume briefly that every atom of an element possesses one, two, three, four, or more such units of force, and we call the element univalent, bivalent, trivalent, quadrivalent, etc., according to the number of such units it possesses. It is by virtue of the existence of these units of force that the compounds made up of the same or of various elementary atoms exist. We assume that in such a molecular compound the atoms are bound one to another in a definite way by means of their affinity units.

Since the development of these ideas concerning the valence of the elements there has been a great deal of work carried on with the object of determining whether the valence of an element is constant or whether it may vary; the majority of chemists are now convinced that it may vary. The valence of nitrogen may be three or five. The valences of hydrogen, oxygen, and carbon, on the other hand, have, until recently, been assumed always to remain constant, *i. e.*, one, two, and four, respectively.

Since the complexity, the very great variety and number of existing compounds containing carbon are unquestionably to be attributed to the peculiar nature of the forces inherent in the carbon atom, let us consider a little more in detail what hypotheses we make in our present system of carbon chemistry concerning this element. We assume, first, that the valence of the carbon atom is always four; second, that the four valences or affinity units of the carbon atom are equivalent; third, that they are distributed in space in three dimensions and act in tetrahedral directions; fourth, that the carbon atoms can unite with one another by means of one, two, or three affinity units to form what we usually call chains.

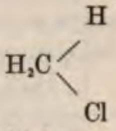
These chains may be open, or closed rings or cycles. The number of carbon atoms thus bound to one another may be exceedingly large. The closed chains usually contain three, four, five, six, or seven carbon atoms in the ring. We may have in these chains, whether open or closed, some of the carbon atoms replaced by oxygen, nitrogen, sulphur, or other elements. If now we unite the extra valences of each carbon or other atom — *i. e.*, those affinity units which are not necessary for binding the atoms together in chains — with other atoms or radicals, it is at once evident that we can represent theoretically, by so-called graphical formulæ, molecules of great complexity. It is also at once obvious that with a small number of atoms it must be possible to construct a relatively large number of aggregates which differ from one another simply in the way the atoms are bound together. In 1884, for instance, fifty-five totally different substances of the empyrical-formula $C_9H_{10}O_3$ were actually known. We call them isomers. One of the chief problems of organic chemistry since 1858 has been to determine on the basis of these ideas of valence the "constitution" of the carbon compounds; we determine by methods which are called synthetic, as well as by an exhaustive study of the reactions of a given compound, what may be called the "architecture" of its molecule, *i. e.*, we determine how the various atoms of carbon, nitrogen, oxygen, and hydrogen, etc., of which the substance may be composed are joined together by virtue of their affinity units. How much has been accomplished on the basis of these ideas during the past forty-six years, and how beautifully and simply all the facts known with regard to the almost countless carbon compounds are thus explained, only those can fully appreciate who have a detailed knowledge of the subject. Notwithstanding the large number of workers in the field, it has often required more than a decade of work to determine the molecular architecture of one single carbon compound, and the question at times seriously presents itself whether we must not reach our limitations in this respect. In any case one point is deserving of especial emphasis: this idea of structure which has been applied chiefly to molecules containing the element carbon attributes to them a rigidity which is improbable from a purely dynamic standpoint.

The present system of organic chemistry is thus founded upon the assumption that the valence of all the atoms of carbon, wherever found, remains invariably four. In the earlier part of the last century many attempts were made to isolate the hydrocarbon methylene, $C=H_2$, which must contain bivalent carbon. Dumas and Peligot tried to obtain this substance from methylalcohol,



$H_2C\left\langle\begin{array}{l}H\\OH\end{array}\right.$

by loss of water. Perrot tried to isolate it from methylchloride,



$H_2C\left\langle\begin{array}{l}H\\Cl\end{array}\right.$

by dissociation into methylene and hydrogen chloride at high temperature. Berthelot, Butlerow, Wurtz, and Kolbe also made many fruitless attempts in this direction. As a final result of these repeated and negative efforts, chemists finally became convinced that compounds containing bivalent carbon could not be isolated, and the conclusion, therefore, that carbon was one of the few elements possessing a constant valence became very general.

There has, however, long existed one very simple compound of carbon which does not adjust itself to this system, — namely, the inactive and poisonous carbon monoxide. If we assume the valence of oxygen as two, then we have here simply a derivative of methylene in which the two hydrogen atoms are substituted by oxygen, $C=O$.

To be sure there were many chemists who preferred to consider the valence of carbon in carbon monoxide as four, thus making the valence of oxygen four, $C \equiv O$; and when we bear in mind that the other members of the oxygen group, sulphur selenium and tellurium, exist as di-, tetra-, and hexavalent atoms, there is some justification for this interpretation. To me personally, however, it seems in the highest degree improbable that two atoms should be thus bound to each other by four affinity units.

About fourteen years ago a series of systematic experiments was undertaken with the object of ascertaining whether carbon can exist in a bivalent condition. The experiments have established this point in a most decisive manner; we have now quite an array of substances which contain bivalent carbon. Furthermore it has been possible to prove, from the experience gained in their study, that methylene chemistry plays an important rôle in many of the simplest reactions of organic chemistry, reactions which have hitherto been explained on the basis of substitution. At the time when these experiments were undertaken there existed besides carbon monoxide several substances which might contain bivalent carbon — namely, prussic acid and its salts the cyanides, $HN=C$ and $MN=C$. Also the so-called carbylamines, $RN=C$, discovered in 1866 by Gautier.

These substances were, therefore, exhaustively studied in order to establish rigidly by experiment whether bivalent carbon was present or absent. The presence of dyad carbon having been established and its properties thus being known, the problem then presenting itself was the isolation of methylene and its homologues.

You are probably all aware that Gay Lussac established in 1815 the existence of a radical, composed of one atom of carbon and one of nitrogen, in prussic acid and the cyanides. This radical, cyanogen, plays in its compounds a rôle similar to that of the elements of the halogen group.

In 1832 Pelouse discovered the alkylcyanides, $R-C \equiv N$, by treating cyanide of potash with alkyliodides or with alkylpotassic sulphates, $KCN + RI$ or $ROSO_2OK \rightarrow R-C \equiv N + KI$ or $KOSO_2OK$, an apparent double decomposition reaction by which we obtain a compound in which the radical $R(=Cn\ H_{2n+1})$ is joined to the cyanogen group by means of carbon. The alkylcyanides thus obtained are neutral, pleasant-smelling, harmless liquids, resembling ether, chloroform, and the alkylhalides, RCl, RBr, and RI.

In 1866 Gautier discovered by treating cyanide of silver with alkyliodides, $RI + AgNC \rightarrow RN=C + AgI$, also an apparent double decomposition reaction, a new class of organic compounds; they are isomeric, not identical, with the alkylcyanides of Pelouse. He called them the carbylamines or isonitriles, and proved that the alkyl group is bound to the cyanogen radical by means of nitrogen $RN=C$ or

$RN \equiv C$. It thus became evident that we must distinguish between two cyanogen radicals, viz., one which in its compounds is bound to alkyl, hydrogen, or metal by means of carbon, $R-C\equiv N$, $H-C\equiv N$, $MC\equiv N$, and another which is joined to these elements or groups by means of nitrogen, $RN=C$, $HN=C$, $MN=C$. We may call the former radical cyanogen, $-C\equiv N$, and the latter isocyanogen,

$$-N=C \text{ or } -N\equiv C;$$

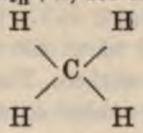
these radicals may obviously combine with each other to form three isomers of the empyrical formula C_2N_2. The substances discovered by Gautier, the alkylisocyanides, $R-N=C$ or $R-N\equiv C$, have properties strikingly different from those of their isomers, the alkylcyanides, $R-C\equiv N$, of Pelouse. They are poisonous, nauseating compounds which affect the throat like prussic acid and color the blood intensely red; they produce violent headaches and vomiting. Their odor is most pronounced and persistent. Hofmann, who, in 1868, discovered another method for making them from primary amines, chloroform, and caustic potash,

$$RNH_2 + 3KOH + CHCl_3 \rightarrow RN=C + 3KCl + 3H_2O,$$

found it impossible to work with them except for very short periods.

An exhaustive study of the reactions of these alkylisocyanides, carried out in 1891–92, led to the definite conclusion that they contain a dyad carbon atom, *i.e.*, they possess the constitution represented by the formula $RN : C$; the other possible formula with quadrivalent carbon and quinquivalent nitrogen, $RN\equiv C$, is excluded by the facts.

The alkylisocyanides belong to the vast category of unsaturated compounds whose chemistry will be briefly discussed from a perfectly general standpoint below; they manifest especially their great chemical activity by absorbing other substances forming new molecules in which the valence of carbon has changed from two to four. Such reactions we call additive. Two molecules simply unite to form one new molecule — the addition product. A molecule containing an unsaturated carbon atom, *i.e.*, one with two of its valences latent or polarized, $RN=C$ or $RN=C]$, cannot *per se* show any chemical activity whatever. This is also true of a system containing a pair of doubly or triply bound carbon atoms, ethylene, $CH_2=CH_2$, and acetylene, $CH\equiv CH$; and finally of a saturated system which we may represent by a paraffine, $C_nH_{2n}+2$, for instance, marsh gas,

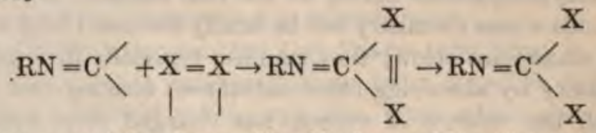


All these substances manifest chemical activity simply because they are to a greater or less degree in a dissociated or what may be called an active condition. A given quantity of alkylisocyanide contains an extremely small per cent of molecules with two free affinity units,

$RN{=}C\langle$; these are in dynamic equilibrium with the absolutely inert molecules $RN{=}C$ or better $RN{=}C\rangle$. That this percentage varies with the nature and mass of R is shown by the fact that various alkylated and arylated isocyanides manifest different degrees of chemical activity. Carbon monoxide possesses relatively a smaller number of such active particles, $O{=}C\langle$, and consequently is a comparatively inert substance, since the speed of addition reactions shown by unsaturated compounds must naturally be directly in proportion to the per cent of active molecules present. A similar conception obviously explains the relative differences in reactivity shown by the various members of the olefine and acetylene series. Marsh gas, a saturated system, reacts with other substances because it is partially dissociated as follows, $CH_4 \rightleftarrows CH_3- + H-$ and $\rangle CH_2 + 2H-$.
From this point of view chemical action depends entirely upon dissociation processes. The reactions often proceed with very great slowness because the percentage of dissociation is extremely low, possibly one tenth to one thousandth of one per cent, or even less.

Turning now to a consideration of the reactions of alkylisocyanides; the substances which are absorbed by the unsaturated carbon atom present in the isonitriles are the following.

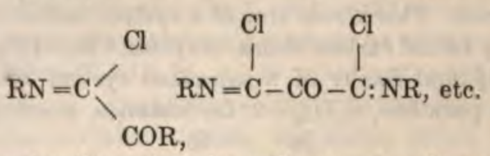
(1) *Halogens* [(chlorine, bromine, iodine); speed of reaction in the order named].



$$RN{=}C\langle + \underset{|}{X}{=}\underset{|}{X} \rightarrow RN{=}C\langle \begin{matrix} X \\ \| \\ X \end{matrix} \rightarrow RN{=}C\langle \begin{matrix} X \\ \\ X \end{matrix}$$

The reactions, especially those with chlorine and bromine, take place with great evolution of heat at −20°.

(2) *Acidchlorides*, such as $RCO-Cl$, $Cl-OC_2H_5$, $Cl-CO-Cl$, $Cl-CN$, $Cl-COOR$, to form the addition products:



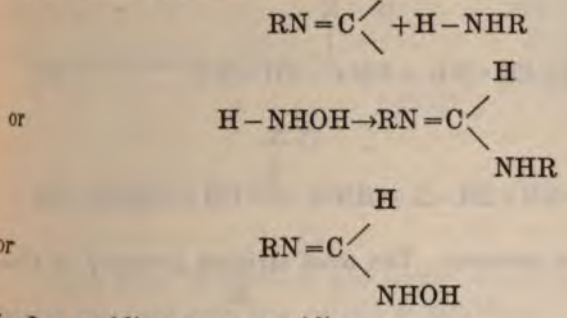
$$RN{=}C\langle \begin{matrix} Cl \\ \\ COR, \end{matrix} \qquad RN{=}\overset{\overset{Cl}{|}}{C}-CO-\overset{\overset{Cl}{|}}{C}{:}NR, \text{ etc.}$$

A hyphen denotes the point where the compounds are partially dissociated and consequently absorbed. These reactions, especially those with phosgene and ethylhypochlorite, take place with great violence at −20°.

(3) *Oxygen and sulphur*, to form isocyanates and mustard oils, $RN{=}C{=}O$ and $RN{=}C{=}S$. Methylisocyanide unites directly at its boiling-point, 58°, with the oxygen of the air. The dry oxides

of silver and mercury are reduced to metals with violence at 40°, alkylisocyanates being first formed. This shows the great affinity of bivalent carbon for oxygen.

(4) *Primary amines and hydroxylamine,*



$$\mathrm{RN{=}C\!<\ + H{-}NHR}$$

or

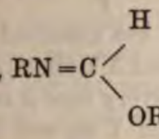
$$\mathrm{H{-}NHOH \rightarrow RN{=}C\!<}\begin{matrix}\mathrm{H}\\ \mathrm{NHR}\end{matrix}$$

or

$$\mathrm{RN{=}C\!<}\begin{matrix}\mathrm{H}\\ \mathrm{NHOH}\end{matrix}$$

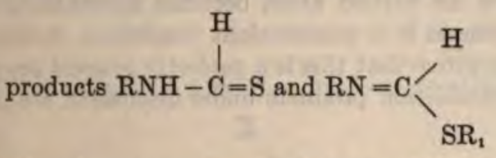
giving amidines or oxyamidines.

(5) *Alcohols* in the presence of an alkali are absorbed, giving imido ethers,



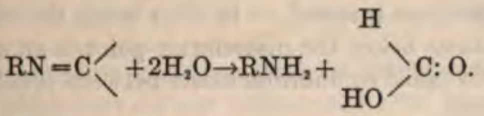
$$\mathrm{RN{=}C\!<}\begin{matrix}\mathrm{H}\\ \mathrm{OR_1}\end{matrix}$$

(6) *Hydrogen sulphide and mercaptans* give readily at 100° the addition products



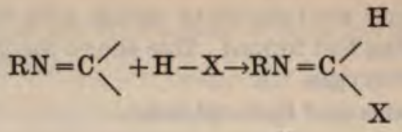
$$\mathrm{RNH{-}\overset{\overset{\displaystyle H}{|}}{C}{=}S} \text{ and } \mathrm{RN{=}C\!<}\begin{matrix}\mathrm{H}\\ \mathrm{SR_1}\end{matrix}$$

(7) *Acids.* Aqueous mineral acids act with great violence on the isonitriles giving primary amines and formic acid:

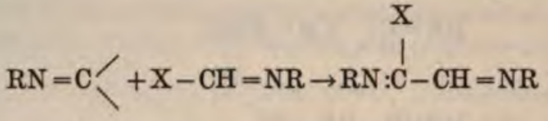


$$\mathrm{RN{=}C\!<\ + 2H_2O \rightarrow RNH_2 +} \begin{matrix}\mathrm{H}\\ \mathrm{HO}\end{matrix}\!\mathrm{>C{:}\,O.}$$

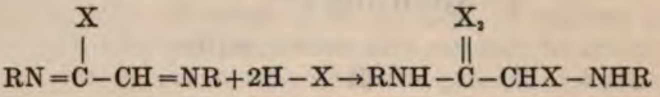
In the absence of water and on diluting the alkylisocyanides with absolute ether, perfectly dry halogen hydride causes the separation of white hygroscopic salt-like substances of the empyrical formula 2RNC, 3HX[X=Cl Br or I]. For this reason Gautier as well as Hofmann supposed the isonitriles to be basic compounds, *i. e.*, substances behaving like ammonia — hence the name carbylamine was given them by Gautier. Further study has shown, however, that this conclusion was erroneous. The isonitriles are entirely devoid of basic properties; the great violence with which they act with halogen hydrides is due to the presence of unsaturated carbon. The reaction probably takes place as follows:



$$\mathrm{RN{=}C}\Big\langle + \mathrm{H{-}X} \rightarrow \mathrm{RN{=}C}\begin{matrix}\diagup \mathrm{H}\\ \diagdown \mathrm{X}\end{matrix} \qquad (1)$$



$$\mathrm{RN{=}C}\Big\langle + \mathrm{X{-}CH{=}NR} \rightarrow \mathrm{RN{:}}\overset{\overset{\mathrm{X}}{|}}{\mathrm{C}}\mathrm{{-}CH{=}NR} \qquad (2)$$

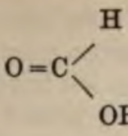


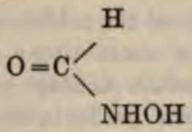
$$\mathrm{RN{=}}\overset{\overset{\mathrm{X}}{|}}{\mathrm{C}}\mathrm{{-}CH{=}NR{+}2H{-}X \rightarrow RNH{-}}\overset{\overset{\mathrm{X_2}}{\|}}{\mathrm{C}}\mathrm{{-}CHX{-}NHR} \qquad (3)$$

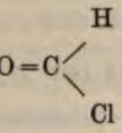
Reversibility of the reactions. The most striking property of the addition products of the isonitriles, $\mathrm{RN{=}C}\begin{matrix}\diagup \mathrm{X}\\ \diagdown \mathrm{Y}\end{matrix}$ is their low point of dissociation, *i. e.*, the carbon atom which has absorbed the $X-Y$, thus becoming quadrivalent, is unable to hold XY above certain temperature limits. There is consequently in every case a temperature, varying with the nature and mass of X and Y as well as with the nature and mass of the groups bound to the other two affinity units of carbon, at which the carbon atom becomes spontaneously dyad and is unable to remain in a quadrivalent condition; it was subsequently possible to prove that this is a perfectly general property of this atom. All the addition products under discussion are partially dissociated, the dissociation $\mathrm{RN{=}C}\begin{matrix}\diagup \mathrm{X}\\ \diagdown \mathrm{Y}\end{matrix} \rightleftarrows \mathrm{RN{:}C}{<}+\mathrm{XY}$, increasing as the temperature is raised, — in other words the valence of carbon at temperatures below the dissociation-point is an equilibrium phenomenon; dynamic equilibrium exists between bivalent and quadrivalent carbon.

The point of complete dissociation of the various addition products of the isonitriles has not been accurately determined in every case. The following data with reference to the dissociation-points of carbon monoxide addition products are of interest and therefore used for illustration in this connection:

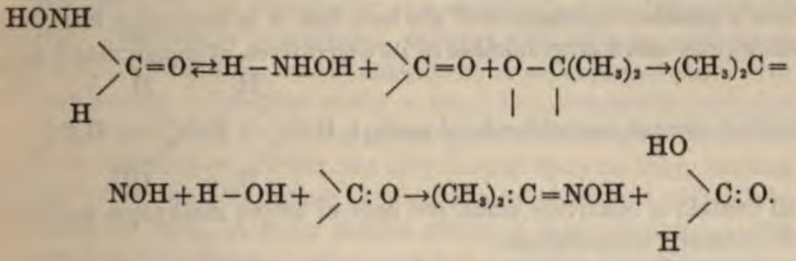
	Dissociation-Point
Formaldehyde, $\mathrm{O{:}\,C{=}H_2}$	600°
Formamide, $\mathrm{O{=}C}\begin{matrix}\diagup \mathrm{H}\\ \diagdown \mathrm{NH_2}\end{matrix}$	about 250°

Formic acid,  $O=C\left\langle\begin{matrix}H\\OH\end{matrix}\right.$ 169°

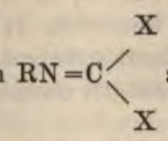
Formhydroxamic acid,  $O=C\left\langle\begin{matrix}H\\NHOH\end{matrix}\right.$ 85°

Formylchloride,  $O=C\left\langle\begin{matrix}H\\Cl\end{matrix}\right.$ below —20°

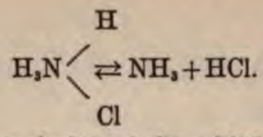
Since these substances containing quadrivalent carbon decompose spontaneously into carbon monoxide, *i. e.*, cannot exist in the quadrivalent condition at temperatures above those indicated, it is self-evident that at lower temperatures the addition products must be partially dissociated and that in the future we must be able to determine in each case with absolute accuracy the per cent of dissociation at any temperature. A striking experiment with formhydroxamic acid, dissociation-point 85°, proves the correctness of this conclusion; on allowing this crystalline substance to stand at 20° in acetone solution the following reaction takes place quantitatively:



$$\left.\begin{matrix}HONH\\H\end{matrix}\right\rangle C=O \rightleftarrows H-NHOH+\rangle C=O+O-C(CH_3)_2\rightarrow(CH_3)_2C=NOH+H-OH+\rangle C:O\rightarrow(CH_3)_2:C=NOH+\left.\begin{matrix}HO\\H\end{matrix}\right\rangle C:O.$$

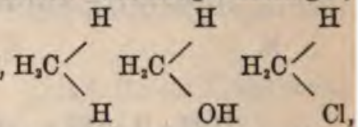
In a similar manner we can prove that the isonitrile addition products, many of which have definite boiling-points and are quite stable, are partially dissociated at ordinary temperatures. Thus the addition products with halogen  $RN=C\left\langle\begin{matrix}X\\X\end{matrix}\right.$ are all converted back quantitatively into the alkylisocyanides by treatment with finely divided metals, zinc-dust or sodium, which simply abstract the free halogen.

Many of the acylhalide addition products dissociate spontaneously into the components on distillation; these phenomena are perfectly analogous to the dissociation of dry ammonium chloride:



$$H_3N\langle^{H}_{Cl} \rightleftarrows NH_3+HCl.$$

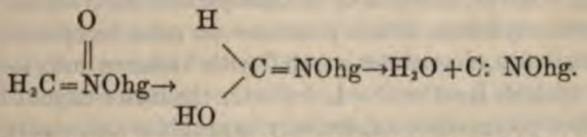
For this reason the majority of the addition products of the isonitriles can be kept only for a short time; this property rendered futile many attempts to isolate definite addition products. The continual dissociation of such products sets free active or dissociated alkylisocyanide particles, and these slowly condense with one another, $xRN=C\langle \rightarrow (RN=C)x$, giving rise to the so-called alkylisocyanide resins (non-reversible), — products whose molecular weight has not yet been determined and which are perfectly analogous to azulmic or polymerized prussic acid. Consequently in carrying out an addition reaction with an isonitrile, especially if it requires much time or a temperature above 20°, large quantities of these resinous polymers are formed from which it is possible to isolate the addition product only with great difficulty.

Many of the isonitriles themselves even when perfectly pure undergo rapid polymerization to resins so that they can be kept only for a very short time. Phenylisocyanide, $C_6H_5N=C\rangle$, is the most striking instance, as it changes in a few minutes from a colorless to a dark blue liquid, and in a few days condenses to a dark brown resin. Have we not here a possible explanation of the fact that it is impossible to isolate methylene and a large number of its derivatives, although marsh gas, methyl alcohol, and chloride of methyl,  $H_2C\langle^{H}_{H}\quad H_2C\langle^{H}_{OH}\quad H_2C\langle^{H}_{Cl}$, all contain a relatively small per cent of active methylene particles at ordinary temperatures?

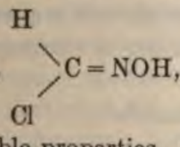
The presence of bivalent carbon in the alkylisocyanides having been established, the next question presenting itself was whether prussic acid and its salts contain the cyanogen or the isocyanogen radical. In the latter case, $H-N=C$, $M-N=C$, these substances must be analogous to Gautier's isonitriles. It had hitherto been considered as established, but without sufficient evidence, that prussic acid and the cyanides were cyanogen compounds analogous to the nitriles of Pelouse.

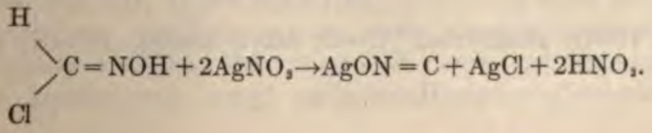
When one considers the physical and physiological properties of prussic acid [boiling-point 25°, specific gravity 0.7, a violent poison] and contrasts these with the corresponding properties of methylcyanide [boiling-point 81°, specific gravity 0.81, sweet-smelling harmless oil] and of methylisocyanide [boiling-point 58°, specific gravity 0.75, a poison], one at once comes to the conclusion that prussic acid as well as its salts must belong to the isocyanogen compounds and consequently must contain bivalent carbon. An exhaustive study of prussic acid and the cyanides establishes this sharply, especially in the case of the salts, from a chemical standpoint. The relation of fulminic acid to prussic acid corroborates the evidence.

You are all familiar with fulminate of mercury — a substance which is made on a commercial scale and used for explosives. It was discovered in 1800 by Howard, and analyzed in 1824 by Liebig in Gay Lussac's laboratory. We obtain it by dissolving mercury in concentrated nitric acid and adding the resulting solution to ordinary alcohol. It has the empyrical formula $HgC_2N_2O_2$, and being obtained from ethylalcohol, CH_3CH_2-OH, fulminic acid was supposed to have two carbon atoms in its molecule, $H_2C_2N_2O_2$. The constitution of this substance was for a long time a great puzzle to chemists. That we have here a substance very closely related to prussic acid was discovered by accident. In working with the mercury salt of isonitromethane it was found that this compound is spontaneously converted at 0° into fulminate of mercury according to the equation,



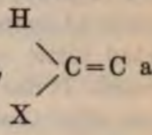
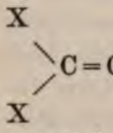
$$H_2C=\overset{\overset{O}{\|}}{N}Ohg \rightarrow \begin{matrix}H\\HO\end{matrix}\!\!>C=NOhg \rightarrow H_2O+C\colon NOhg.$$

This synthesis led directly to the conclusion that fulminate of mercury possesses a constitution entirely analogous to cyanide of mercury, $C=Nhg$, *i. e.*, that it contains the isocyanogen radical with bivalent carbon. A further study of the fulminates established this point with precision. Especially striking is the behavior of fulminates towards dilute acids. Liebig and Gay Lussac state in 1824, judging from the odor, that fulminate of silver gives prussic acid with dilute hydrochloric acid. A more careful study of this reaction in 1894 proved that not a trace of prussic acid but a substance formylchloride oxime,  $\begin{matrix}H\\Cl\end{matrix}\!\!>C=NOH$, is formed which possesses the following remarkable properties. Long needles, clear as glass, which decompose and explode at 20°; extremely volatile even at 0° and having an odor similar to prussic acid which is obviously due to a partial dissociation into fulminic acid. Aqueous silver nitrate converts it quantitatively into chloride and fulminate of silver,



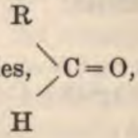
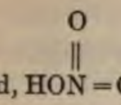
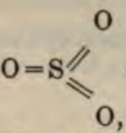
$$\begin{matrix}H\\Cl\end{matrix}\!\!>C=NOH+2AgNO_3\rightarrow AgON=C+AgCl+2HNO_3.$$

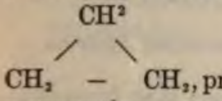
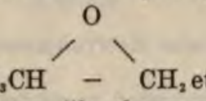
Up to 1897 the presence of bivalent carbon had been established in the following compounds, (1) carbon monoxide, $O=C$; (2) the alkyl and aryl isocyanides, $RN=C$; (3) prussic acid and the cyanides, $HN=C$, $MN=C$; (4) fulminic acid and the fulminates, $HO-N=C$, $MO-N=C$. (2), (3) and (4) are all compounds containing the isocyanogen radical.

In 1897 the presence of bivalent carbon was established in a series of nitrogen free carbon compounds obtained from acetylene. They are the mono-and dihalogen substituted acetylidenes,  $\begin{matrix}H\\ X\end{matrix}\!\!>C=C$ and  $\begin{matrix}X\\ X\end{matrix}\!\!>C=C$ [X = Cl Br or I]. The corresponding members of the acetylene series, $XC\equiv CH$ and $XC\equiv CX$, do not exist, although we have substances like $CH_3C\equiv CI$, $C_6H_5C\equiv C-X$, whose properties are in marked contrast to those of the acetylidene derivatives.

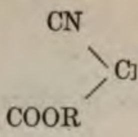
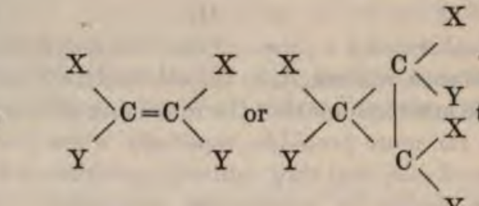
Diiodacetylidene, which possesses an odor deceptively like that of the isonitriles, dissociates at 100° with violence into iodine and diatomic carbon, $I_2=C=C\rightarrow I_2+C=C$; the latter cannot be isolated as such, but polymerizes explosively to graphite and amorphous carbon. The mono- and dihalogen substituted acetylidenes are all poisonous and spontaneously combustible compounds, possessing, therefore, like methylisocyanide a marked affinity for oxygen Up to the present time it has not been possible to isolate compounds containing bivalent carbon other than those mentioned above. We are, however, now in a position to explain clearly why we cannot hope by methods now known to isolate methylene and its homologues as such, although these substances play a great rôle in many of the fundamental reactions of organic chemistry. In order to approach this point more intelligently, let us consider briefly the properties of unsaturated compounds in general, their possibility of existence, etc.

II. *On the Unsaturated Compounds*

The unsaturated compounds may, first of all, be divided into three categories, namely; (1) those in which two atoms, which may be the same or different, are bound doubly or triply to each other by two or three affinity units, such as olefines, acetylenes, chlorine, $Cl\equiv Cl$, oxygen, $O=O$, aldelhydes,  $\begin{matrix}R\\ H\end{matrix}\!\!>C=O$, alkylcyanides, $RC\equiv N$, nitric acid,  $\mathrm{HO\overset{\overset{O}{\|}}{N}=O}$, sulphur trioxide,  etc.; (2), those in which an atom itself is unsaturated, *i. e.*, does not exert its maximum valence capacity, as, for instance, amines, R_3N, thioethers, $R_2=S$], methylene derivatives, etc. We must assume that the remaining affinity units are latent, or, what is far more probable, especially where two or four affinity units are available, that they mutually polarize each other in a manner entirely similar to unsaturated compounds containing doubly or triply linked atoms.

Finally we have a third class of unsaturated compounds, (3) those containing closed atomic chains such as trimethylene, , propyleneoxide,  etc., which show apparently a saturated molecular system like the paraffines, and yet react in a manner perfectly analogous to olefines and methylene derivatives. Fundamentally considered, these three classes of unsaturated compounds manifest their chemical activity in the same way; they absorb a great variety of other molecules and thus form combinations, called addition products. How does this union take place? An unsaturated compound with its affinities polarized represents in reality a saturated system; it cannot *per se* show chemical activity. This is also true of molecular systems in which the atoms are bound to one another by single affinity units. The sole basis for reactivity in either case is the presence of a relatively greater or smaller number of dissociated particles. The reactivity of any unsaturated, as well as of a saturated compound, must in fact be directly proportional to the ratio of such active particles present. If that ratio is very small, the substance may be entirely inert; if it is greater, absorption of reagents proceeds with regularly increasing speed.

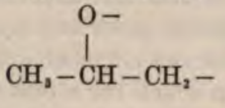
Experience has shown, furthermore, that many unsaturated compounds cannot be isolated, but polymerize spontaneously. It is clear that when the per cent of active particles present in an unsaturated compound becomes relatively great, the possibility of their uniting with each other to form condensed molecules increases — in fact, we may imagine a condition in which the active molecules simply cannot be prevented from combining with each other. This shows us why we cannot isolate and keep substances like formaldehyde, $H_2C=O$, or alkylcyanates, $R-O-C\equiv N$, in the monomolecular form. Similarly in many cases where attempts were made to isolate methylene derivatives like mono- and diphenyl methylene, benzoyl and acetyl methylene, cyanmethylene-carboxylate,


, a spontaneous polymerization to the di- or tri-molecular systems,  took place.

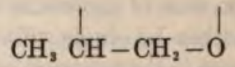
One further point with reference to unsaturated compounds must now be presented.

Intramolecular Rearrangements shown by Unsaturated Systems

From the discussion presented above it is obvious that trimethylene and propyleneoxide, belonging to class 3, must contain a small percentage of active particles; the dissociation of the triatomic ring in the former case can lead to only one form of active molecule, namely, $-CH_2-CH_2-CH_2-$; whereas propyleneoxide may give the following three active molecules:

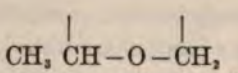


$$CH_3-\overset{\overset{O-}{|}}{C}H-CH_2- \qquad (A);$$



$$CH_3\,\overset{|}{C}H-CH_2-\overset{|}{O} \qquad (B);$$

and



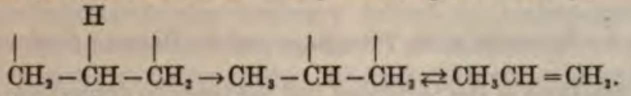
$$CH_3\,\overset{|}{C}H-O-\overset{|}{C}H_2 \qquad (C).$$

Since propyleneoxide absorbs dry ammonia or hydrogen chloride, as was proved by especially careful and exhaustive experiments, giving addition products of the general formula

$$CH_3\,CHOH-CH_2\,X\ [X=Cl \text{ or } NH_2],$$

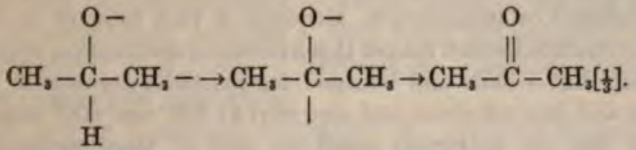
the only possible conclusion that can be reached is that propyleneoxide contains relatively more active A than active B or C molecules; consequently the absorption reactions proceed by preference in *only one* of three theoretically possible directions.

When trimethylene or propyleneoxide is heated or placed in contact with various catalytic agents, the per cent of active particles must naturally increase, and when a definite limit has been reached a spontaneous transformation of trimethylene into propylene and of propyleneoxide into propylaldehyde ($\frac{2}{3}$) and acetone ($\frac{1}{3}$) takes place; both reactions are non-reversible. These results can only be explained in the following manner: aside from the increase in active particles dissociation in other parts of the molecule and especially of hydrogen from carbon must also take place. Consequently the following intramolecular addition reactions finally occur spontaneously:



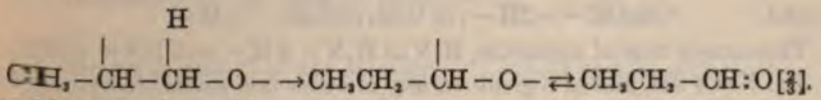
$$\overset{|}{C}H_2-\overset{\overset{H}{|}}{C}H-\overset{|}{C}H_2\rightarrow CH_3-\overset{|}{C}H-\overset{|}{C}H_2\rightleftarrows CH_3CH=CH_2.$$

active trimethylene particles — propylene



$$CH_3-\underset{\underset{H}{|}}{\overset{\overset{O-}{|}}{C}}-CH_2-\rightarrow CH_3-\underset{|}{\overset{\overset{O-}{|}}{C}}-CH_3\rightarrow CH_3-\overset{\overset{O}{\|}}{C}-CH_3[\tfrac{1}{3}].$$

active propyleneoxide particles [*A*] — acetone

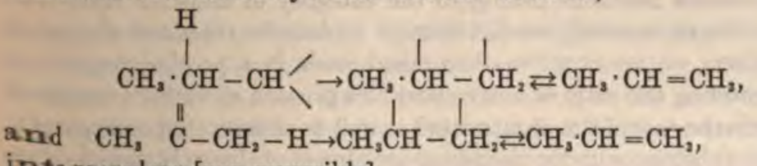


$$CH_3-\overset{|}{C}H-\overset{\overset{H}{|}}{C}H-O-\rightarrow CH_3CH_2-\overset{|}{C}H-O-\rightleftarrows CH_3CH_2-CH:O[\tfrac{2}{3}].$$

active propyleneoxide particles [*B*] — propionaldehyde.

It is interesting to note that the active *B* propyleneoxide molecules which are present in smaller ratio suffer rearrangement more readily than the active *A* molecules. The active *C* molecules, on the other hand, must be present in far smaller amount and certainly no transformation of propyleneoxide to vinylmethyloxide, $CH_2=CH-O-CH_3$, takes place. It is important to realize that propyleneoxide, acetone and propionaldehyde are isomers but do not stand in a tautomeric relation to one another. This is also true of trimethylene and propylene as well as of *a* and *β* amylene and isoamylene, etc.

Similarly it can be rigidly shown by experiment that *a* and *β* propylidene, $CH_3CH_2-CH=$ and $(CH_3)_2=C<$, which are spontaneously combustible substances not capable of isolation as such, transform themselves by intramolecular addition,



$$CH_3\cdot\overset{\overset{H}{|}}{C}H-CH<\rightarrow CH_3\cdot\overset{|}{C}H-\overset{|}{C}H_2\rightleftarrows CH_3\cdot CH=CH_2,$$

and $$CH_3\ \overset{\|}{C}-CH_2-H\rightarrow CH_3\overset{|}{C}H-\overset{|}{C}H_2\rightleftarrows CH_3\cdot CH=CH_2,$$

into propylene [non-reversible].

There is not the slightest doubt that such intramolecular addition reactions are the basis of the majority of our synthetic methods for making cyclic compounds. The cycloparaffines in Russian petroleum are probably formed from ordinary paraffines by dissociation into hydrogen and methylene derivatives, and the latter then spontaneously transform themselves, by intramolecular addition, into penta- and hexamethylene rings.

On the Reactions of the Paraffines and the Benzene Derivatives

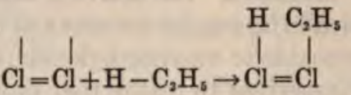
The reactions of the paraffines and the benzene derivatives towards halogens, nitric and sulphuric acids, whereby substitution products are formed, are still interpreted in the text-books from the standpoint of metalepsis or substitution, although a vast amount of evidence has accumulated which makes this axiomatic assumption improbable. The fact that ethane and benzene, for instance, decompose into hydrogen and into ethylene and diphenyl at 800° and 600° respectively proves that an extremely small per cent of these molecules must exist at ordinary temperatures in an active or dissociated condition,

$$CH_3CH_3 \rightleftarrows CH_3CH_2- + H-$$

and $$CH_3CH= + 2H- ;\ \text{or}\ C_6H_6 \rightleftarrows C_6H_5- + H-.$$

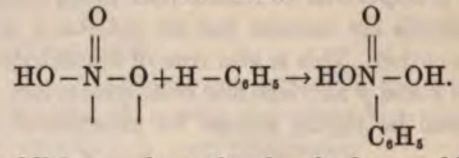
The same is true of ammonia, $H_3N \rightleftarrows H_2N- + H-$ and $HN= + 2H-$ and $N\equiv + 3H$, and of a great variety of other non-ionizable hydrogen compounds.

Consequently when chlorine or nitric acid acts with benzene or ethane to give the monochlor or mononitro substitution products, we have these reagents, in the *active molecular* condition, simply uniting by addition with the dissociated ethane or benzene particles,



$$\overset{|}{Cl}=\overset{|}{Cl}+H-C_2H_5 \rightarrow \overset{\overset{H}{|}}{Cl}=\overset{\overset{C_2H_5}{|}}{Cl}$$

or



$$HO-\underset{|}{\overset{\overset{O}{\|}}{N}}-\underset{|}{O}+H-C_6H_5 \rightarrow HO\underset{\underset{C_6H_5}{|}}{\overset{\overset{O}{\|}}{N}}-OH.$$

The resulting addition products then lose hydrogen chloride and water respectively and thus give the monochlor or nitro substitution product of the mother substance. From this point of view all so-called substitution reactions belong to the category of addition reactions. What is now especially needed in order to place the reactions of organic chemistry on an exact mathematical basis is a precise method of determining the ratio of active particles present at various temperatures in the case of the unsaturated as well as of saturated compounds. As the substances under discussion are almost exclusively non-electrolytes, the sole methods that suggest themselves for this purpose are determinations of the speed of decomposition as well as of addition reactions.

The above discussion makes it evident that all unsaturated compounds belonging to classes 1 and 3 contain a small and relatively varying per cent of active particles with one or more carbon atoms temporarily in an active or *trivalent* condition; the same is true of compounds containing hydrogen bound to carbon-paraffines, $C_nH_{2n+1}-H$, benzene derivatives, etc. The isolation of compounds containing trivalent carbon as such, I believe, however, to be an impossibility. Gomberg's triphenylmethyl, for instance, has recently been proved by him and others to be a bimolecular aggregate $C_{38}H_{30}$, — identical with hexaphenylethane — which, however, like the above-mentioned compounds, contains a very small percentage of active triphenylmethyl, $(C_6H_5)_3\equiv C-$, particles in dynamic equilibrium with the bimolecular aggregate.

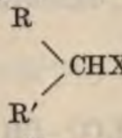
We are now in a position to consider the evidence showing that methylene and its homologues play a great rôle in many of the fundamental reactions of organic chemistry which have hitherto been explained on the basis of substitution.

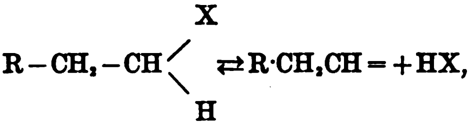
III. *On the Reactions of the Monatomic Alcohols and the Alkylhaloids*

The experiments which first suggest themselves as a means of isolating methylene and its homologues are, (1) dissociation of olefines as ethylene:

$$CH_2=CH_2 \rightleftarrows 2H_2C=, \text{ } \beta \text{ butylene,}$$

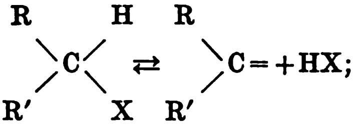
$$CH_3CH\colon CH\cdot CH_3 \rightleftarrows 2CH_3CH{<}, \text{ etc.}$$

Since ethylene gives hydrogen and acetylene by heat and the higher olefines also decompose with evolution of hydrogen, there was little prospect of success by experiments in this direction. (2) Dehydration of the monatomic alcohols, $C_nH_{2n+1}OH$, or removal of halogen hydride from the alkylhalides, $C_nH_{2n+1}X$; naturally only primary and secondary derivatives, RCH_2X and  $\genfrac{}{}{0pt}{}{R}{R'}{>}CHX$ [$X=OH$ Cl Br or I], and not tertiary compounds, $R_3\equiv C-X$, can yield methylene and its homologues. Furthermore since many of the alcohols and alkylhalides containing more than one carbon atom in the molecule are known to give olefines by dissociation, dehydration, or treatment with alcoholic potash respectively, the conclusion might naturally at first be drawn that only a direct olefine dissociation existed in these cases. From a purely theoretical standpoint, however, it is clear that a primary or secondary alkylhalide or a corresponding alcohol with more than one carbon atom in the molecule may dissociate with loss of halogen hydride or water in two possible ways: it may undergo (1) methylene dissociation, as



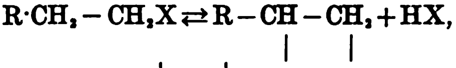
$$R-CH_2-CH\begin{matrix}X\\ H\end{matrix} \rightleftarrows R\cdot CH_2CH= + HX,$$

and

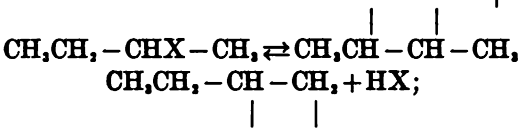


$$\begin{matrix}R\\ R'\end{matrix}C\begin{matrix}H\\ X\end{matrix} \rightleftarrows \begin{matrix}R\\ R'\end{matrix}C= + HX;$$

or (2) olefine dissociation, as



$$R\cdot CH_2-CH_2X \rightleftarrows R-\underset{|}{CH}-\underset{|}{CH_2}+HX,$$

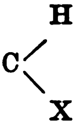


and $$CH_3CH_2-CHX-CH_3 \rightleftarrows CH_3\overset{|}{CH}-\overset{|}{CH}-CH_3$$

and $$CH_3CH_2-\underset{|}{CH}-\underset{|}{CH_2}+HX;$$

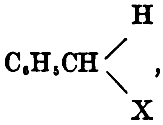
or both kinds of dissociation may take place simultaneously. A third kind of dissociation, where the hydrogen atom does not come from the atom containing the X or from a carbon atom adjacent to it, is also possible, and at times important, but it need not be considered in this connection.

An exhaustive study of the primary and secondary alcohols and alkylhalides covering a period of nine years has proved very conclusively that these substances undergo methylene dissociation only. Preliminary experiments with alcohols and alkylhalides where no olefine dissociation is possible, *i. e.*, in the methane,

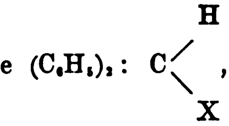


$H_3C\begin{matrix}H\\ X\end{matrix}$

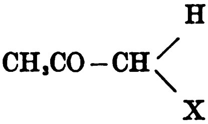
toluene,



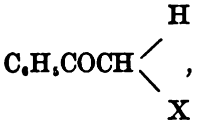
$C_6H_5CH\begin{matrix}H\\ X\end{matrix}$, diphenylmethane



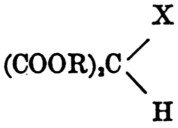
$(C_6H_5)_2:C\begin{matrix}H\\ X\end{matrix}$, acetone and acetophenone,



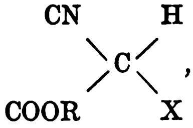
$CH_3CO-CH\begin{matrix}H\\ X\end{matrix}$ and

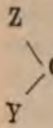
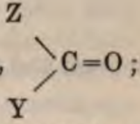


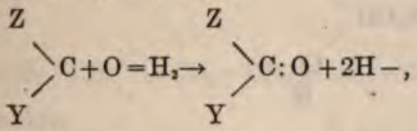
$C_6H_5COCH\begin{matrix}H\\ X\end{matrix}$, malonic and cyanacetic ether series



$(COOR)_2C\begin{matrix}X\\ H\end{matrix}$ and

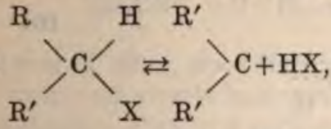


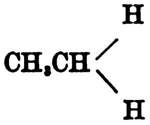
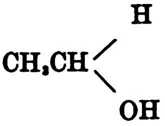
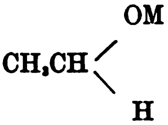
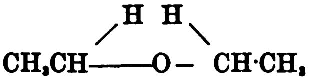
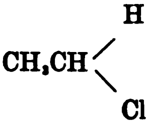
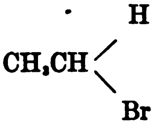
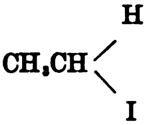
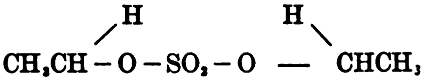
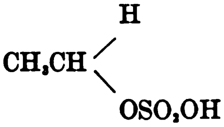
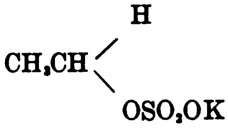
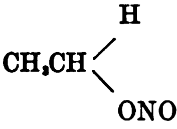
$\begin{matrix}CN\\ COOR\end{matrix}C\begin{matrix}H\\ X\end{matrix}$, have proved that all these compounds have very low dissociation-points — never over 300° in the aromatic nor with few exceptions in the aliphatic series. Nevertheless, it was found impossible to isolate in any case the methylene derivative as such; there was either a spontaneous conversion to a di- or trimolecular polymer, an olefine or a trimethylene derivative, or a conversion to resinous polymers analogous to azulmic acid and the alkylisocyanide resins. Most important was the discovery that these nascent or active methylene residues,  $\overset{Z}{\underset{Y}{>}}C$, are always spontaneously combustible, burning often with marvelous evolution of heat to the corresponding oxides,  $\overset{Z}{\underset{Y}{>}}C=O$; this was not surprising in view of the properties of the methylene derivatives described above. Furthermore, the affinity of unsaturated carbon for oxygen is strikingly shown by the fact that these residues have the power of decomposing water,



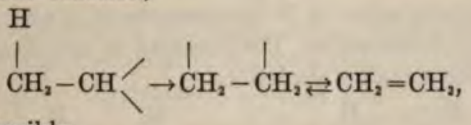
$$\overset{Z}{\underset{Y}{>}}C+O=H_2\rightarrow\overset{Z}{\underset{Y}{>}}C:O+2H-,$$

with evolution of hydrogen.

A subsequent investigation of the primary and secondary alcohols and alkylhalides containing more than one carbon atom proved, first of all, that all these substances have comparatively low points of dissociation. In no case was the decomposition-point found to be higher than 700°; it was often as low as 160° to 300°. The products of dissociation are water or halogen hydride and C_nH_{2n} respectively; and the latter, as emphasized above, is invariably methylene or a homologue and never an olefine. This naturally means that all these compounds are partially dissociated in this way at ordinary temperatures,  $\overset{R}{\underset{R'}{>}}C\overset{H}{\underset{X}{<}}\rightleftarrows\overset{R'}{\underset{R'}{>}}C+HX$, — relatively more the lower the actual decomposition-point. It is, therefore, possible that in all the interactions of the primary and secondary alkylhalides with other substances, such as salts, ammonia, metals, benzene, etc., they do not act as such, but by virtue of being partially dissociated. An enormous amount of evidence has accumulated in favor of this conclusion. Let us consider chiefly the results obtained in the ethyl series including ethyl alcohol and its derivatives. The dissociation or decomposition-point of the following compounds containing ethyl has been determined with a fair degree of accuracy.

		Decomposition-Point
Ethane,		800°
Ethylalcohol,		650°
Sodium and potassium ethylate,		250°
Ethylether,		550°
Ethylchloride,		600°
Ethylbromide,		500°
Ethyliodide,		400°(?)
Diethylsulphate,		200°
Monoethylsulphate,		160°
Ethylpotassium sulphate,		250°
Ethylnitrate,		200°(?)

Ethane, ethylchloride and bromide, when heated to the temperatures named, give ethylene and hydrogen or halogen hydride respectively, and on cooling these products do not again recombine. We can therefore obtain ethylene quantitatively from chloride or bromide of ethyl by simply passing their vapors through tubes heated to the decomposition-point. Nevertheless, it is impossible to obtain more than very small amounts of ethylene from the ethylhalides by means of alcoholic potash, caustic potash, or quicklime; in these cases ethylether or ethylalcohol is the chief product even when the ethylhalide is passed over quicklime in tubes heated from 300° to 500°. Furthermore, the per cent of ethylene obtained varies remarkably with the temperature, the concentration, and with the nature of the halogen in the alkylhalide used. The conclusions finally reached from these data and also from an exhaustive study of the behavior of the various alkylhalides, nitrates, sulphates, alkylpotassium-sulphates towards heat, sodium ethylate, caustic potash, quicklime, and other salts, are that ethylene cannot possibly be a primary product of dissociation of the ethylhalides, sulphates and nitrates and of free ethylalcohol. The ethylene, when obtained, is formed from ethylidene by an intramolecular addition reaction,



$$\overset{\overset{\textstyle H}{|}}{CH_2}-CH\Big\langle \rightarrow \overset{|}{C}H_2-\overset{|}{C}H_2 \rightleftarrows CH_2=CH_2,$$

which is not reversible.

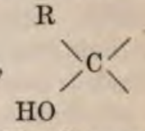
A similar intramolecular change always, in fact, takes place whenever an olefine is formed, whether from a primary or secondary alcohol or from a corresponding alkylhalide sulphate or nitrate. This transformation is perfectly analogous to the conversion, discussed above, of trimethylene and of propylene oxide into propylene, propionaldehyde and acetone.

When ethylalcohol or ethylether is heated to its dissociation-point the ethylidene interacts at once in great part with the other dissociation-product, water, to give hydrogen and acetaldehyde,

$$CH_3CH\Big\langle + O=H_2 \rightarrow CH_3CH:O+2H.$$

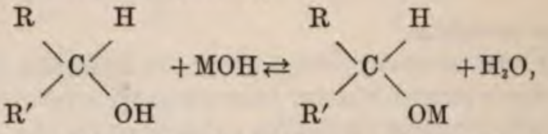
In the case of ether, since there are two ethylidene molecules to one of water, the atomic hydrogen is in part absorbed by ethylidene to give ethane. Finally, a portion of the ethylidene, 20 and 37 per cent respectively, is transformed, by intramolecular addition, into ethylene. The most striking proof that ether is dissociated into water and two C_2H_4 particles is the following: on passing ether vapor over phosphorous pentoxide at temperatures varying from 200° to 400° ethylene is formed quantitatively.

The primary and secondary alcohols and their corresponding ethers being in a state of very slight dissociation at ordinary temperatures, we are able to understand perfectly their behavior towards oxidizing agents. The alkylidenes are all spontaneously combustible substances possessing a great affinity for oxygen. Absolutely pure, dry ethylether, dissociation-point 550°, contains a sufficient per cent of ethylidene particles at ordinary temperatures to burn very slowly in dry oxygen; sodium ethylate, dissociation-point 250°, on the other hand, being dissociated to a far greater extent, burns with great violence in dry air. Ethylalcohol, dissociation-point 650°, is not capable of burning in the air; if, however, we increase the per cent of ethylidene particles by means of catalytic agents, enzymes, platinum sponge, etc., it, too, oxidizes readily, with incandescence with platinum sponge, giving acetic acid. The aldehydes, $RCH=O$, as has long been known, reduce Fehling's solution and silver solutions with great ease. This is due to the presence of oxyalkylidene particles,



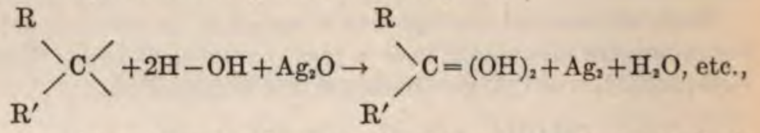
$\overset{R}{\underset{HO}{>}}C<$

which burn at the expense of the oxygen of the water. The discovery that all primary and secondary alcohols reduce silver oxide to metallic silver in aqueous solution in the presence of caustic alkalies has only very recently been made. The function of the alkali is obviously to form first the metallic alcoholate,



$$\overset{R}{\underset{R'}{>}}C\overset{H}{\underset{OH}{<}} + MOH \rightleftarrows \overset{R}{\underset{R'}{>}}C\overset{H}{\underset{OM}{<}} + H_2O,$$

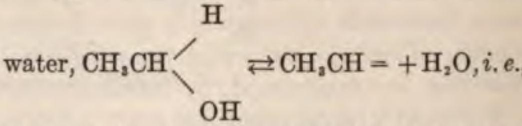
which, having a far lower dissociation-point than the free alcohol, causes a great increase in the per cent of alkylidene particles present; consequently the following reaction takes place,



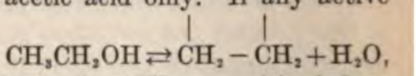
$$\overset{R}{\underset{R'}{>}}C< + 2H-OH + Ag_2O \rightarrow \overset{R}{\underset{R'}{>}}C=(OH)_2 + Ag_2 + H_2O, \text{ etc.,}$$

giving as the end result a fatty acid in the case of primary alcohols.

The most striking proof that ethylalcohol is dissociated only into ethylidene and water,



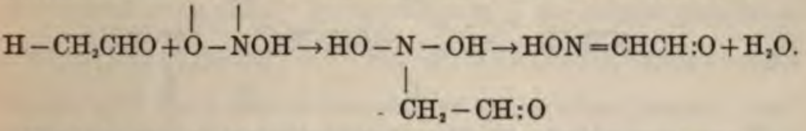
$CH_3CH\overset{H}{\underset{OH}{<}} \rightleftarrows CH_3CH= + H_2O$, *i. e.*, contains no ethylene particles, is the following. Ethylalcohol, containing one molecule of aqueous sodic hydrate, gives in the cold with potassium permanganate solution practically acetic acid only. If any active ethylene particles were present,



$CH_3CH_2OH \rightleftarrows \overset{|}{C}H_2-\overset{|}{C}H_2 + H_2O$, these must necessarily, in view of the work of Wagner with olefines and permanganate, be first converted by oxidation to ethyleneglycol,

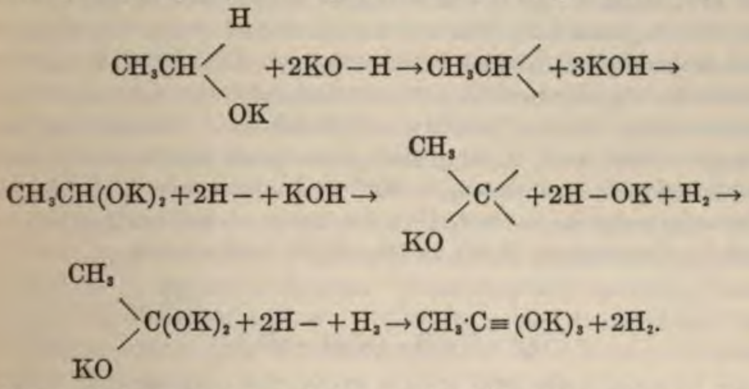
$$3\overset{|}{C}H_2 - \overset{|}{C}H_2 + 6H - OH + 2KMnO4 \rightarrow OHOH$$
$$3CH_2 - CH_2 + 2MnO_2 + 2KOH + 2H_2O.$$

Analogous results would naturally be expected in the case of all the homologous primary and secondary alcohols. Now a primary alcohol invariably first gives by oxidation with potassium permanganate or other oxidizing agents the corresponding fatty acid; glycols or their oxidation products have never been observed in such cases. The fact that ethylalcohol gives glyoxal, glyoxylic and oxalic acids with nitric acid, is no exception to this rule because these substances result from the hydrolysis and oxidation of isonitrosoacetaldehyde which is formed by the action of nitrous acid on acetaldehyde as follows:



$$H - CH_2CHO + \overset{|}{O} - \overset{|}{N}OH \rightarrow HO - \underset{\displaystyle CH_2 - CH{:}O}{\underset{|}{N}} - OH \rightarrow HON = CHCH{:}O + H_2O.$$

The behavior of aldehydes and of primary alcohols towards aqueous or solid caustic potash also leads to the conclusion that only alkylidene dissociation occurs. Ethylalcohol gives at 250°, with an excess of caustic potash, hydrogen and potassium acetate quantitatively,

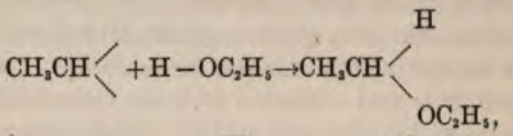


If any of the potassium ethylate, which is first formed, were dissociated into ethylene and caustic potash,

$$CH_3.CH_2OK \rightleftharpoons \overset{|}{C}H_2 - \overset{|}{C}H_2 + HOK,$$

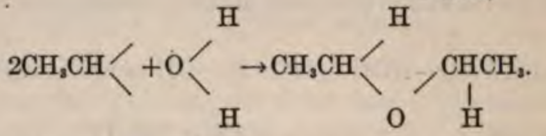
the olefine must naturally give, besides hydrogen, ethylene glycol, $\overset{|}{C}H_2.\overset{|}{C}H_2 + 2HOK \rightarrow KOCH_2 - CH_2OK + H_2$, or its decomposition products; these are, however, not formed. The reaction with potash lime and primary alcohols is so delicate and accurate that it has been suggested by Hell as a means of determining the molecular weight of an unknown primary alcohol.

As mentioned above, ethylether is the chief product when ethylhalides are treated with alcoholic potash or with dry sodium ethylate; this is also true when dry silver oxide and ethylhalides are used. These reactions, which have been interpreted by Williamson and others on the basis of double decomposition or of minute ionization, must obviously be attributed to the absorption by the ethylidene of alcohol or of water which is set free by the action of the halogen hydride particles on the sodium ethylate or silver oxide respectively,



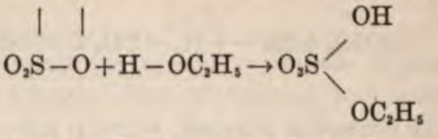
$$CH_3CH< + H-OC_2H_5 \rightarrow CH_3CH<^{H}_{OC_2H_5},$$

or



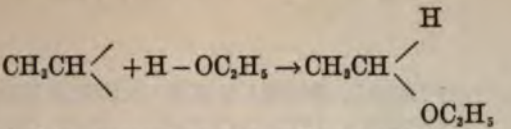
$$2CH_3CH< + O<^{H}_{H} \rightarrow CH_3CH<^{H}_{\underset{O}{}}>\underset{\overset{|}{H}}{C}HCH_3.$$

We are now able to consider an entirely new explanation of the function of sulphuric acid, or of phenylsulphonic acid, in converting ethylalcohol into ether. Sulphuric acid acts first of all with alcohol at ordinary temperatures to give both mono- and diethylsulphate; the first stage in the reaction cannot be ascribed to the union of ethylidene, formed by dissociation of alcohol, with free sulphuric acid, since ethylether, which is relatively more dissociated than alcohol, acts *only very* slowly with concentrated sulphuric acid at ordinary temperatures to give primary ethylsulphate. Furthermore since sulphuric acid itself is completely dissociated into its components, sulphurtrioxide and water, at 400° it is extremely probable that monoethylsulphate is formed by the union of sulphurtrioxide, present by dissociation, $H_2SO_4 \rightleftarrows SO_3 + H_2O$, with alcohol,



$$\overset{|}{O_2S}-\overset{|}{O}+H-OC_2H_5 \rightarrow O_2S<^{OH}_{OC_2H_5}$$

Now it is well known that ether formation in a mixture of sulphuric acid and alcohol begins perceptibly only at 95° and proceeds *very* slowly at that temperature. The favorable temperature for ether manufacture is 140°. This is self-evident in view of the following considerations. Primary and secondary ethylsulphate possess the dissociation-points 160° and 200°, respectively; consequently these substances must be dissociated at 140° to a very great extent into sulphuric acid and one or two molecules of ethylidene respectively. Addition of alcohol at 140°, therefore, simply necessitates a combination with the ethylidene particles,



$$CH_3CH\Big< + H-OC_2H_5 \rightarrow CH_3CH\Big<^{H}_{OC_2H_5}$$

to give ether, and this process can naturally go on indefinitely.

When ethyl alcohol is mixed with an excess of concentrated sulphuric acid and heated to 160° no ether but some ethylene is formed; in fact this method is still suggested and used as the best means of preparing ethylene. The yield of olefine, however, can never be raised above 20 per cent of the theory, and the operation is extremely tedious because carbonization and formation of sulphurdioxide takes place to a very marked extent. These results are now easily understood. The ethylidene molecules, formed by dissociation of ethylated sulphuric acid, burn chiefly at the expense of the oxygen present in sulphuric acid, $CH_3CH\Big< + O=SO_2 \rightarrow CH_3CH:O + SO_2$ and the resulting acetaldehyde is then at once charred by the vitriol present. Only twenty per cent at the utmost of the ethylidene particles escape this oxidation by intramolecular conversion to ethylene.

Finally we may summarize the conclusions reached in the above discussion as follows. The valence of carbon is not a constant. At definite temperatures, which vary remarkably with the nature of the groups bound to it, a carbon atom becomes spontaneously dyad. Below these limits there is dynamic equilibrium between bivalent and quadrivalent carbon. The existence of carbon compounds containing bivalent carbon has been definitely established; methylene chemistry plays a great rôle in many of the fundamental reactions of organic chemistry. The conception of substitution or metalepsis, which has been our guide in interpreting the reactions of carbon chemistry since 1833 is no longer tenable. It must be replaced by the conception of dissociation in its broadest sense. Fundamentally speaking, there are but two classes of carbon compounds,—the saturated and the unsaturated compounds. Excluding reactions called ionic, a chemical reaction between two substances always first takes place by their union to form an addition product. The one molecule being unsaturated and partially in an active molecular condition absorbs the second molecule because it is partially split or dissociated into two active portions. The resulting addition product then often dissociates spontaneously, giving two new molecules.

The similarity of such reactions to those called ionic is at once apparent, but their relationship cannot in the present state of our knowledge be clearly understood.

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Vol. 270, pp. 267–335; Vol. 280, pp. 263–342;
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Vol. 310, pp. 316–335; Vol. 318, pp. 1–57 and 137–230;
Vol. 335, pp. 191–333.

THE PROGRESS AND DEVELOPMENT OF CHEMISTRY DURING THE NINETEENTH CENTURY

BY FRANK WIGGLESWORTH CLARKE

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THE history of any science is a record of progress from empiricism to philosophy; from isolated details to systematic knowledge. At the outset, certain facts impress themselves upon the minds of men, either because the observed phenomena are beneficial, or for the opposite reason. Between the facts, the simpler and more obvious relations of cause and effect are first noted, but only in the most superficial way and without deliberate intention. By degrees, after many wanderings along paths that lead nowhere, and in spite of countless misinterpretations, mankind slowly accumulates a mass of data in which something like unity begins to appear, and through which it is seen that the universe is not a creature of caprice, but an existence organized and orderly. This conception lies at the foundation of all science; it is the one article of faith which the student dares not doubt; for rational investigation would be impossible without it. The belief in order, and the hope that we may discover its laws, inspire all scientific researches.

Speaking broadly, the development of science takes place in three stages, which merge one into another and often overlap. First, there is the collection of data; classification follows; and attempts at interpretation come last of all. This is the logical course, which, however, is not always followed. Premature speculations, efforts to determine what the universe should be, are not unknown in the history of human thought, nor have they been altogether futile. Hypotheses, framed in advance of positive knowledge, help to stimulate investigation, and so, despite their errors, lead us ultimately to the truth. In reality, the three stages of growth coexist; experiment and speculation go on side by side; and each one reinforces the others.

At the beginning of the nineteenth century, chemistry was in a transitional, I might almost say a formative, period of its existence. It was just emerging from the morasses of a philosophy unchecked by experiment, and from the vagaries of the alchemists, and was assuming something like its present form. A goodly mass of data had been gathered; they were partly classified, and the work of interpretation was successfully begun. The analyses of air and water, the discrimination between elements and compounds, and a recognition of the constancy of mass, had laid the foundations of the new science. This word "new" I use advisedly. In its earlier days chemistry was only an empirical art, in which discoveries were made by chance, and remembered because of their utility. Chemical facts were secrets in the hands of artisans, or held by initiated priesthoods; and when they were recorded at all it was only in the form of useful recipes or as medical prescriptions. As a science, as an organized body of knowledge with a philosophy of its own, chemistry hardly existed before the time of Boyle. Alchemy, groping in the darkness, had made useful discoveries; but their successful correlation was an affair of a much later period. To Lavoisier, more than to any other one man, the transformation of chemistry from an art into a science must be ascribed. There were greater discoverers than Lavoisier, perhaps, but he was the organizing spirit, and his proof that matter was indestructible made quantitative chemistry a possibility. Without such a basis a rational science would be almost inconceivable. It is a necessary complement to the older philosophical maxim that from nothing nothing can be made. Creation and destruction are equally beyond our powers — a truism which the ancients may have apprehended, but which before the time of Lavoisier rested on speculation alone. Indeed, the conception was defective until the middle of the nineteenth century, when the doctrine of the conservation of energy raised it to completion.

Let us now return to the opening of the century and see how matters stood. The simpler gases, acids, and bases, and the commoner metals were known, and many compounds had been more or less completely examined. Richter and Fischer had shown that reactions took place in proportions which exhibited simple relations to one another; the doctrine of phlogiston had ceased to dominate chemical opinion, and the law of definite proportions, despite the opposition of Berthollet, was generally received. That chemical changes should be governed by fixed quantitative laws was a natural condition to expect, but it needed both proof and explanation. So many reasonable theories had already broken down that a healthy skepticism prevailed, and chemists demanded concrete evidence in favor of every proposition that philosophy might offer for their edification. Rubbish had been cleared away, — what structure should rise in its place? An answer to this question was speedily forthcoming.

It was in October, 1803, that John Dalton published the beginning of his famous atomic theory, but it was not until five years later that he gave it completely to the world. Merely as a speculation, the idea of atoms was as old as philosophy; but in its scientific form it was something entirely new. Under it, the law of definite proportions became necessary and significant; the law of multiple proportions, which had been partially anticipated by others, was made complete; and these considerations alone would have justified the provisional acceptance of the doctrine. It unified the known or suspected laws of chemical combination and gave them philosophic validity. It incited chemists to verify the evidence in its favor, and so led to new discoveries; in short, it fulfilled all the conditions of a good scientific theory. Its chief peculiarity, however, its prime difference from all preceding atomism, remains to be stated. Dalton discovered that to every element a single definite number could be assigned, and that these numbers or their multiples governed the formation of all compounds. Oxygen, for instance, unites with other elements in the proportion of eight parts by weight or some multiple thereof; never in other ratios. These combining numbers, under Dalton's theory, became the relative weights of the atoms; and atomism, hitherto a qualitative notion only, received a quantitative expression. With the help of these atomic weights, or combining numbers, as some anti-theorists preferred to call them, the composition of any substance could be represented by a simple formula; and chemical calculations, which had been empirical and arbitrary, became systematic and easy. In short, Dalton had discovered a new class of constants, the fundamental numbers of quantitative chemistry, whose significance has steadily increased and is probably not even yet completely appreciated. To this point I shall recur later.

The decade following Dalton's unique discovery was chiefly characterized by two lines of research, the study of inorganic compounds, and the investigation of their physical relations. Davy, by decomposing the alkalies and earths, gave precision and definiteness to the conception of a chemical element, while Gay Lussac and Avogadro discovered the laws which connected the volume relations of gases with their chemical composition. To Avogadro we owe the discrimination between atoms and molecules — a distinction which physics, unaided by chemical evidence, could probably not have reached, and which even now is often overlooked by physicists. Maxwell, for example, in his article upon atoms in the Encyclopædia Britannica, deals with molecules throughout, and fails to mention Dalton's work at all. To Maxwell the physical arguments were clear, the chemical relations were not adequately appreciated.

In 1819 Dulong and Petit discovered the law connecting the specific heat of a solid element with its atomic weight, but apart from that investigation chemical research became for thirty years largely a matter of detail. Discoveries were many, successful generalizations were few. During this epoch, Wöhler, by his synthesis of urea, broke down the barrier between organic and inorganic compounds; Liebig and others proved that groups of atoms, the so-called compound radicles, could play the part of pseudo-elements; Dumas established the principle of substitution, and Faraday connected the phenomena of electrolysis with the atomic constants. Inorganic chemistry, however, received the lion's share of attention, and the commanding figure of the period was that of Berzelius. To him we owe the development of chemical formulæ and equations, the thorough determination of many atomic weights, the discovery of new elements, and the investigation of innumerable compounds. And yet his gigantic labors were performed in a laboratory which a modern high school would despise, in which the chemist of to-day would be able to accomplish next to nothing. It was, in fact, a kitchen, wherein cookery and research were carried on almost side by side. Had Berzelius possessed our wealth of resources, could he have achieved a greater success? Perhaps not, for we must remember that he had a virgin field to cultivate, and the implements of the pioneer are less elaborate than those which his successors require. A great part of the work done by Berzelius was necessarily crude, and much of it is still awaiting revision, for the man who clears the ground is not the one to give it the highest cultivation. As knowledge grows, the demands for facilities increase, and we could not return to primitive methods even if we wished to do so. Imagine a modern astronomer with Galileo's telescope, and no more mathematics than Kepler could command! Berzelius labored in the days of small things, and being great he overcame the obstacles that confronted him; we to-day are the slaves of a complexity such as the earlier chemists could never have imagined. I refer now to the material side of science; in its theoretical aspects simplicity has been gained and our range of vision has widened correspondingly. We work in clearer air and with much more powerful appliances than the investigators of earlier times, but to say that we do better would be rash indeed. There are giants in all days, and no age has a monopoly of greatness.

During the Berzelian period, as I have said, inorganic chemistry was the main subject of chemical research. But it was not the only theme, for chemical physics also received a good deal of attention, and organic compounds were by no means neglected. Inorganic substances were apparently simple, the organic were complex; and so the former were naturally considered first, the more obvious problems taking precedence over the less evident. By degrees, however, opinion changed, and the great discoveries of Wöhler, of Liebig, and of Dumas, the theoretical discussions of Laurent and Gerhardt, and perhaps also the physical regularities pointed out by Kopp, turned the current of research into a new channel. Substances that could be arranged in series, with progressive differences in composition and properties, were evidently worth examining; a compound radicle was, in its way, as fascinating an object of study as a new element; the possibilities of substitution and the marvelous chemical plasticity of organic matter were noted, and all of these considerations worked together in effecting a transformation of chemical thought. Organic chemistry became the fashion, and for nearly fifty years it was the central subject of research.

Before entering upon this new period, let us go back and examine the conditions under which progress had previously been made. How was the work done, and what impulses urged it forward? What purposes, what demands, what encouragement, led chemists to pursue their labors? At first, chemistry was a branch of the older natural philosophy, and the discovery of natural laws, the reaching after truth for its own sake, was the chief aim of investigators. These, as a rule, were individuals, working independently, each on his own resources, and without thought of practical results. Science and industry were as yet unallied; chemistry had but a small part in schemes of education; institutions for the aid of research were few, and those which did exist were scantily endowed. Davy, to be sure, had the Royal Institution behind him, and in it he discovered Faraday; Berzelius was secretary of the Academy at Stockholm; but these were exceptional cases, and not by any means the rule. Personal initiative and voluntary effort were almost the sole agencies at work. The great discoveries were made by amateurs, by men who among other labors found some leisure in which to study; and only the occasional man like Cavendish, with ample means, could give his whole time to research. Priestley was a clergyman; Scheele an apothecary; Lavoisier a public official, and these are typical examples. The impulse to investigate came from within, uninfluenced by thought of profit or by any manner of external compulsion. An inspiration, not the pressure of a duty, drove our predecessors forward.

By degrees, however, chemistry was found to be useful, and the commercial demand for chemical services began. Manufacturers discovered that processes and products could be improved, and that waste material had value; metallurgy developed along chemical lines, medicine gained new remedies, and agriculture was turned from its traditional empiricism into scientific courses. A new set of impulses was given to chemistry, and many of its practitioners became professional in expectation of material profit and reward. The field of research was widened, and civilization was thereby advanced. Chemistry was not merely a philosophical amusement, but an agent for "the betterment of man's estate;" and so a double motive existed for its further development. This combination of intellectual interest with utility gave the science a higher place in educational affairs; and when Liebig opened the first university laboratory for students at Giessen, a new era for chemistry began. Before that time the chemist was either self-taught or trained in private laboratories; now he could aspire to scholastic honors and assume his proper position as a learned man. As a discoverer, Liebig was great, but his chief services to chemistry were in his educational work and in the application of science to agriculture. To those achievements his wide reputation is mainly due.

For chemistry, then, the second half of the century opened auspiciously. Chemists were needed for technical purposes and as teachers, and resources were placed at their disposal almost without stint. Discovery was stimulated, investigation became more systematic, theory and practice developed side by side. Practical applications followed the most abstract researches; new industries sprang into existence, and in education mere bookishness gave way to experimental methods. A great but silent revolution had taken place, whose magnitude will be better appreciated by posterity than by ourselves. Had science done no more than to replace supposition by experiment, and chance discovery by orderly research, the revolution would still have been one of the greatest in the history of mankind. Chemistry was not the sole agent in effecting the transformation, but it surely played one of the leading parts.

All of the agencies which I have mentioned helped to encourage the study of organic chemistry. It was systematic, and therefore easily taught, and it was full of suggestiveness both for teacher and pupil. Its practical applications were many, and gave the investigator hope of material rewards; the revelations of coal-tar alone were enough to stimulate chemists to the greatest activity. So it happened that inorganic chemistry fell into neglect, and the majority of chemists followed the leaders into the new field. The conceptions of chemical structure, which had been slowly evolving during many years, were given definiteness by the discovery of valence, and of this the benzene theory was perhaps the most brilliant application. Frankland, Williamson, and Perkin in England; Dumas and Wurtz in France, Kekulé and Hofmann in Germany, and the Russian Butlerow, are the conspicuous names connected with the modern movement. Organic chemistry became an imposing structure, and yet it rested upon the foundations which the older chemists had laid. The constitutional formulæ were built upon atomic conceptions, valence itself was a property of the atom, and complete acceptance of the new ideas was impossible until after Cannizzaro had revised the atomic weights and brought them into harmony with Avogadro's law. Up to that point there was a chaos of rival doctrines, afterward order reigned. The full significance of valence could not appear until the old system of chemical equivalents had been set aside.

Naturally, as the mass of chemical data increased, specialism became necessary. No man could expect to know the whole of chemistry; a small part of it was all that any one could handle, and the inevitable results followed. A specialist may be broad, but the direct tendency of specialism is to narrow one's field of view, and to concentrate the attention upon details rather than generalities. The theories which fit immediate conditions then become satisfactory, and the chance that they may be only partial glimpses of greater laws is disregarded. Only the stronger and more philosophical minds can escape these limitations and see things in their larger aspects.

To the organic specialist, at least in most cases, the doctrine of valence was adequate; for it explained the combinations with which he had to deal. Relatively few of the chemical elements were seriously considered by him, and they offered no insuperable difficulties. Carbon was the typical element, the key to all organic matter; its quadrivalency in terms of the hydrogen unit was assured; its ability to unite with itself in chains or rings was established; with these data constitutional formulæ became truly significant, and useful for the correlation of existing knowledge. Even more can be said in their favor, for they had a certain prophetic ability which guided research and foretold discovery. But, after all, carbon was only one among many elements, and nobody was justified in assuming that its modes of combination represented general laws, or that ideas drawn from the study of organic matter alone were applicable elsewhere. The theory of valence must be tested with regard to all the elements before its full validity could be recognized, and that test implied a renewal of interest in inorganic problems. It was necessary to discriminate between special cases and fundamental principles, and so a much larger field than organic chemistry could offer had to be surveyed. Clues had been found in the study of carbon compounds, but where were they to lead?

So far as actual knowledge went, the chemical elements were distinct entities, and speculation as to their nature had been looked upon generally with disfavor. And yet they had points in common which rendered their classification possible, and it was perfectly evident that they could be arranged in a small number of natural groups. Certain elements were obviously types of others; some were isomorphous, as shown by Mitscherlich, and some exhibited serial relations as in Döbereiner's triads; but no one scheme of classification covered the entire ground. Analogies were numerous enough, but their meaning was not clear. A process of evolution was at work, however, and in due time it culminated in Mendelejeff's development of the periodic system. All partial classifications, all the dim foreshadowings of law, now fell into place together, and one simple generalization occupied the field. The atomic weights became more than ever the fundamental constants of chemistry, and all the properties of the elements were seen to be periodic functions of these quantities. In Mendelejeff's table stress was laid upon valence and the form of compounds which each element could yield; in Lothar Meyer's curves the physical relations were emphasized, and so each statement reinforced the other. Newlands, it is true, had partially anticipated Mendelejeff, but his law of octaves fell just short of completeness.

At first, the periodic classification attracted comparatively little attention, and its general acceptance might have been slow had it not been for certain prophecies. In Mendelejeff's table there were many gaps; these were attributed to the existence of elements as yet unknown, and for three of them the author ventured upon predictions. Each element must have a certain atomic weight, a prescribed density and melting-point, and should form compounds of a stated character. In due time the three unknown elements were actually found, and gallium, scandium, and germanium confirmed all of Mendelejeff's anticipations. The importance of the classification was thus established, and the periodic law became one of the foundation stones of modern chemistry. The conception of valence as a property of the atom acquired a broader significance; in cases that had been doubtful its magnitude could be determined, and with its aid the chaos of inorganic chemistry began to exhibit signs of something like order. The deficiencies of the periodic system I need not mention here, for this is no time for details; neither shall I discuss the obvious difficulties which arise when we seek to apply the doctrine of valence to inorganic compounds; only the larger verities concern us now. In the broadest sense the periodic classification is sound; the principle of valence is general, and the obstacles which now appear will doubtless be overcome by future investigation. That the greatest generalization has been reached, we cannot assume; but so far as we have gone we stand on solid ground, and can continue our explorations in safety.

Up to a certain point organic compounds had been successfully interpreted in terms of valence. Isomerism was explained, and the existence of unknown isomers could be predicted; different atoms were assignable to different positions within the molecule, as in the case of the four hydrogen atoms of acetic acid, one fixed and three replaceable; but after all this had been done there were still some difficulties outstanding. Isomers existed whose chemical structure seemed to be the same, and for their interpretation an extension of chemical theory was needed. This want was supplied by van't Hoff and Lebel, who almost simultaneously pointed out the consequences of assigning a tetrahedral form to the atom of carbon. From the properties of such an atom a new class of structural formulæ could be deduced, by means of which the so-called cases of "physical isomerism" were simply interpreted. The molecules of tartaric and racemic acids, for example, resemble each other as an object resembles its reflection in a mirror, the one being a reversal of the other. Our science acquired a new province, that of stereochemistry, which in less than thirty years has grown to impressive dimensions. The theory of van't Hoff and Lebel did more than to interpret the troublesome known phenomena, it encouraged additional research and led to many discoveries. At first, the asymmetric carbon atom alone was considered, but its peculiar properties are now shared by other elements, and physical or stereochemical isomers are found even among inorganic compounds. When one atom is combined with four other atoms or groups of as many different kinds, optical asymmetry appears, and physical isomerism becomes possible.

During the ninth decade of the century the dominating interest in organic chemistry began to wane, for the reason that other subjects were demanding their share of attention. I do not mean by this that the activity of organic chemists diminished, for their output of discovery was never greater than now; but the centre of the stage was slowly being filled by other groups of actors. Inorganic chemistry was reviving from its long neglect, and physical chemistry loomed large upon the horizon. In each of these branches journals were started, and no difficulty was found in filling their pages with the records of successful investigations. In theory, physical chemistry has made the greatest advances, inorganic research has been more a matter of detail. Let us briefly consider the two themes separately.

To the inorganic chemist several duties were apparent. Old work needed revision, the compounds of many elements were almost undescribed, there was a lack of system to remedy, and the theories derived from organic chemistry were to be tested and applied. A very large part of the work was necessarily descriptive, a preparation for the future, but back of it all lay a fundamental question with which all physical science is connected, for the nature of matter itself was to be determined. In its broadest sense this question demands the coöperation of all science and all philosophy, but to inorganic chemistry one phase of it may be assigned. What is the nature of the chemical elements? Are they one or many? And how shall an element be defined? To these questions there is as yet no final answer, but clues to follow are many, and some of them are offered by the periodic law. To remedy its imperfections is an obvious duty for inorganic chemists to perform.

Near the middle of the Mendelejeff table and of the Lothar Meyer curve there is an area which is partly blank and partly filled with the symbols of uncertain elements. That some of them were tri- and others quadrivalent was well established; but their number was undetermined, and the places which they should occupy were even more doubtful. Some of the uncertainties still remain, and some have been cleared away; but the main problem is as yet unanswered, and therefore the metals of the rare earths are still actively studied. Supposedly definite earths have proved to be mixtures; others, like cerium, lanthanum, yttrium, and scandium, seem to be definitely placed; but what shall we say of the rest? Didymium was thought to be a distinct element, and yet it has been split in two; samarium, gadolinium, erbium, and ytterbium are probably definite; but several other metals are claiming recognition; and so, notwithstanding the progress which has been made, a large part of the field is still obscure. Through the study of the rare earths, one side of our problem, the nature of the elements, is open to attack; but only the outworks have been carried so far.

According to modern ideas, the integrity of an element is determined by two conditions; it must have a distinct spectrum and a definite atomic weight. In the study of the rare earths these criteria have been systematically applied, and to great advantage; but what has been done elsewhere? To answer this question we must go back more than forty years in time and make a new beginning.

It was near the middle of the century that August Comte, seeking to find some limits to positive knowledge, argued that it would be impossible for us ever to determine the nature of the heavenly bodies. Are they composed of matter like that which forms the earth, or are they different in kind? — on that theme we might speculate, but we could never know. The prophecy was futile; Kirchhoff and Bunsen, with the spectroscope, swept the limitations away, and all the universe, as far as eye could reach, was found to contain familiar elements, but under conditions not always like our own. Astronomy, physics, and chemistry had gained a new weapon, and discovery followed discovery along widely different lines.

In the chemical laboratories the value of the new instrument was immediately proved. Two metals, caesium and rubidium, were presently discovered by its aid, thallium and indium were found a little later, and their analogies to other elements made them comparatively easy to classify. The periodic system, which was developed later still, gave them their proper positions among the metals, and they in turn made the classification more complete, and therefore easier to establish. In each case the double criterion was applicable, and definite spectrum was connected with definite atomic weight. I speak now of emission spectra; but they are not the only kind. Certain solutions give absorption spectra, and they have been of great assistance in the study of the rare earths. In the identification of the elements, then, the spectroscope has rendered service of inestimable value, and discovery would have been very slow without it. Quite recently, at the very end of the last century and during the few years of the new, relations have appeared between the wave-lengths of the spectral lines and the atomic weights of the elements; but the general expression which shall connect them all is yet to be revealed.

Another discovery in the realm of inorganic chemistry is deserving of mention now on account of its peculiar significance. The atmosphere was thought to be well known, and yet in 1895 a new element, argon, was discovered in it. This find was quickly followed by others of like kind, and now five gases previously unknown have been extracted from the air. Each gas is identifiable by its spectrum and its density, and from the latter datum the atomic weight can be inferred.

Now the interesting fact concerning these atmospheric gases — helium, neon, argon, krypton, and xenon — is that they represent matter of a new kind. So far as evidence goes, they are monatomic, absolutely inert, and incapable of union with other elements. Their valence is zero, and when the periodicity of the elements is represented by a vibratory curve, they occupy the points of rest, — the nodes. They are matter having physical, but no chemical, properties, and therefore they can be investigated only upon the physical side. This conclusion, perhaps, should be stated provisionally, for it may be reversed by future discovery; but of this possibility we have only one suggestion. Helium was first extracted from the mineral uraninite in which it is firmly held, and we cannot say with certainty that it is not chemically combined. Altered or massive uraninite contains little or no helium; the crystallized varieties yield more, and the most brilliant and perfect crystals are the richest of all. The gas may be merely occluded, but the bare chance of combination should not be overlooked. Either supposition is legitimate; but there is still one more possibility, namely, that helium may be generated by the decay of another substance, and not be an original constituent of uraninite at all. Here we touch the mystery of radium — a body which challenges our former conceptions of an element, for seemingly it can be decomposed.

The discovery of radium by Mme. Curie belongs to the nineteenth century, and therefore it falls within the scope of this essay. How it was found, how laboriously the phenomena of radioactivity were observed in order to isolate traces of the new metal, we all know, and the details need no repetition here. At last pure salts of radium were obtained, and the two criteria of spectrum and atomic weight were satisfied. Radium is clearly a metal of the barium group, it fills a definite place in the periodic table, its claims to elementary rank are on a level with those of other elements, and yet it exhibits an apparent instability which is difficult to explain. Radium gives off material emanations that are different from itself; they are gaseous and inert; in them the spectrum of helium has been observed. From one element another seems to be derived, and all our notions of what an element should be are thrown into temporary confusion. I say "temporary," for I believe that order will be restored, and that a deeper insight into the constitution of matter is close at hand.

Pardon me now if I seem to wander from one part of my subject to another. Between the various departments of knowledge there are no sharp boundaries, and the solution of a problem often depends upon the convergence of testimony from many different directions. The nature of the elements is primarily a question for the inorganic chemist; but physics has much to say upon the subject, and even the serial relations of organic compounds offer suggestive analogies which are entitled to some consideration. The periodic system, with its fulfilled prophecies, tells us that the elements are related one to another by some distinct law; the spectroscope gives us evidence of a different order; electrical phenomena have their share in the story, and the modern phenomena of radioactivity offer the latest testimony of all. What conclusions seem to be foreshadowed by the data now in hand?

One of the earliest achievements of the spectroscope was the rehabilitation of the nebular hypothesis. The resolution of some nebulæ into clusters of stars had shaken faith in Laplace's speculation; but when it was proved that others were really clouds of incandescent gas, belief in the hypothesis was restored. One point, however, was of peculiar interest: in the nebulæ only one or two elements, low in the scale of atomic weights, could be seen; in the whiter and hotter stars a few more substances appeared; colored stars were of still greater complexity, and so on progressively from the simplest constitution to the material heterogeneity of our globe. If suns and planets were evolved from nebulæ, it seemed as if the chemical elements had been successively generated at the same time — a supposition which was certainly legitimate, although it was at first denied by some chemists as unworthy to be heard. At all events, here was testimony bearing upon the problem of the elements, although its full significance was not so clear. It could be pigeonholed, but not thrown away.

Recently, and in great part through the researches of J. J. Thomson, evidence has been obtained of the reverse order. On one side an evolution of the elements is apparently indicated; Thomson's experiments suggest a breaking-down. By studying the ionization of gases, phenomena were observed which point to the existence of particles smaller than the Daltonian atoms, and a beginning has been made toward the identification of matter with electricity. The negative particles, corpuscles or electrons, have been split off from ordinary matter, and they are always the same, regardless of the element from which they separate. Even their mass can be estimated, and it appears to be about the thousandth part of that which represents an atom of hydrogen. These conclusions are perhaps not final, but they are emphasized by the results obtained in the study of radioactivity. The investigations of Rutherford and Soddy, of Ramsay, Dewar, and others, all tend in the same direction, and lead to the suspicion that the atoms are complex and subject to decay. The three most radioactive elements are radium, thorium, and uranium, and these have the highest atomic weights of any substances known. If the elements are complex, these are the most so, and therefore presumably the least stable. If we take this testimony in connection with that given by Thomson, the evidence offered by the spectra of the heavenly bodies, and the regularities of the periodic law, we have a strong argument in favor of the supposition that the so-called elements are not the simplest forms of matter, and that they may be ultimately one. The doctrines of unity of matter and the unity of force are thus philosophically allied, and only negative evidence can be adduced to support a belief in the actual diversity of the elements.

Speaking broadly, organic and inorganic chemistry, at least as they are commonly studied, are essentially descriptive in their character, and they deal with statical phenomena. Physical chemistry, on the other hand, is more concerned with dynamics, and seeks to determine the conditions of chemical equilibrium, and the nature of chemical change. What substances are and what substances do are of course only two phases of the same fundamental problem, which are separable ideally, but not otherwise. Descriptive chemistry lays stress upon one side of the science, physical chemistry emphasizes the other; but they blend together by imperceptible degrees, and no clear line of demarcation can be drawn between them.

Every science, when viewed historically, is seen to have a central line of growth, to which its various branches are naturally related. In chemistry this line is marked by physical phenomena, and from their study the greater generalizations have been derived. Avogadro's law, the law of Dulong and Petit, Faraday's theory of electrolysis, and the periodic classification of the elements are good illustrations of this principle. The atomic theory itself, which connects all of the other relations, is fully as much physical as chemical; valence is best explained in electrical terms, and stereochemistry arose from optical and crystallographic considerations. Physical chemistry is the main stem of our science, and statical conditions are merely the results of dynamical equilibrium. The description of a product is incomplete unless we have noted the physical phenomena, the transformations of energy, which took place during its formation, and to studies of this kind the chemists of the future must devote a large part of their time.

During the last twenty years the importance of physical chemistry, or rather the recognition of its importance, has steadily increased, and to-day it seems to dominate the entire field of chemical research. Laboratories are equipped for its purposes alone, journals are devoted to it, and the activity of investigators has become so great that subdivision has already begun, and men are known as thermo-chemists, electro-chemists, and so on. Electro-chemical societies have been formed and are prosperous; specialism is passing into subspecialism; in short, chemistry is swiftly assuming an entirely new form.

In the evolution of any science successes and disappointments are almost equally influential; the former stimulating, the latter tending to arrest research. The fruitful line is followed, and attracts workers; the barren field is deserted or nearly so. Barrenness, however, may be due not to lack of fertility, but to premature effort; and the truth which is beyond our reach to-day may drop into our hands to-morrow. Thermo-chemistry, for example, has so far failed to repay the labor spent upon it, and has fallen into disfavor; but the future may tell a different story. Its importance is obvious, and its general laws cannot elude discovery forever. The thermal changes which accompany all chemical reactions must sometime be interpreted.

On the other hand, success has followed the physical study of solutions, and thereby chemical theory has been enriched. First, it was found that substances in solution exerted pressure — a phenomenon attended by depression of the melting-point and increased temperature for boiling. This pressure resembled that observed in gases, and a relation between the two was apparent. It was van 't Hoff's privilege to trace the connection, and to develop a kinetic theory of solutions. Avogadro's law was completely paralleled, and equal volumes of solutions at equal osmotic pressures were shown to contain equal numbers of molecules. For both laws, the liquid and the gaseous, however, there were certain apparent exceptions, which, for gases, were easily explained as the result of dissociation. Arrhenius applied this explanation to the exceptional solutions, taking into consideration also the ionic conceptions developed in the study of electrolysis, and the abnormalities vanished. A salt in dilute solution is electrically dissociated into its ions, which remain in equilibrium although separate. From these generalizations several important consequences followed. First, it became a simple matter to determine the molecular weights of soluble substances — a class of measurements that had previously been possible for gases alone. Secondly, much light was thrown upon the subject of reactions between dissolved salts, especially such as involve precipitation or double decomposition. In most cases, although not invariably, the phenomena are ionic, and the molecules are first broken down. In the third place, the uniform heat of neutralization between acids and bases was explained by showing that in all cases it represented one and the same change, namely, the union of hydrogen and hydroxyl ions to form water — a conclusion which gave a significant datum to thermo-chemistry. In brief, many distinct lines of physico-chemical research converge in the kinetic theory of solutions — a theory whose development has hardly more than begun. Like most successful theories, its importance may at first be exaggerated; we have not yet the perspective which shall enable us to judge it truly; in all probability, it is but one phase of some larger law; but, notwithstanding all difficulties and all objections, it is a stride forward, and will bring us to new truth.

We now reach a point where it is difficult to disentangle the many threads of investigation, and to determine their relations to one another and to the past. Current work is more or less confusing, for it is too near our eyes, and its ultimate significance is not easily apprehended. The theory of solutions, the law of mass-action enunciated by Guldberg and Waage, and the phase rule of Willard Gibbs interact in so many ways, and are so rapidly developing, that I for one dare not attempt to predict what the outcome shall be. Chemistry is becoming more and more a mathematical science, and so is gaining in precision; but mathematical reasoning leads to correct conclusions only when its premises are secure. The data must be verified and reverified before we can certainly determine their meaning, and in the enthusiasm of new investigation this necessary duty is often deferred. The pioneer leaves much undone behind him, and patient laborers are needed to follow in his lead. The first glimpse of truth is rarely the whole truth, for that is best gained by what we may call the method of successive approximations.

If prophecy is difficult, retrospection is easy; we may therefore retrace our steps and see what road we have followed. Boyle, Priestley, Scheele, and Lavoisier prepared the way for Dalton, and his atomic theory, the first quantitative theory of its kind, has been for a century the key to all chemistry. All of the great advances in our science have hinged directly upon Dalton's conception, and his atomic weights, as developed by Berzelius and Cannizzaro, are now seen to be fundamental constants, with whose aid the physical relations of different substances are easiest interpreted. The periodic law is based upon the atomic weights, valence is an atomic function, in stereochemistry we have a hint of atomic form, isomerism is intelligible only upon the assumption of variable atomic position, and the structure of a molecule depends upon atomic groupings. The ions of physical chemistry and the molecules of thermodynamics are either atoms or groups of atoms; and, in short, whichever way we turn in physical science we find ourselves, consciously or unconsciously, thinking in atomic terms. And yet we are sometimes told that the atomic theory is outworn, and that some other conception should replace it. We may well ask, therefore, whether atomism has any basis in reality. Is it the truth or only an illusion — a concrete fact, or misinterpretation of testimony?

That the atomic theory has rendered great service to chemistry, and that it correlates our positive data, is clear; but after all it is hypothetical, for no atom has been isolated and seen. The molecule and the atom are inferred from the properties of matter in mass; and if we need a theory at all, there is none other at hand. The attempts to evade it are agnostic in character, and are based upon the tacit assumption that it is unscientific to speculate upon ultimate questions, which, in the nature of things, can never be absolutely solved. We can observe and classify relations, but it is useless to ask what they mean. The phase rule has been suggested as a basis for our classification, and under it the different kinds of matter become different phases of something which we may or may not be able to comprehend. Perhaps I misrepresent the position of the anti-atomists; but if so it is because their statements are to my mind far from clear. If we object to the atom, we must object to the ether, for that is equally unknowable; we cannot divorce matter and motion, for they are never observed apart; in short, we must reconstruct all physical science and keep within the limits of things known. But is the agnostic position sound? Is not the imagination as truly an instrument of science as is the reason? May we never look forward and anticipate what is to come, shall we always observe and experiment without the help of ideals? To do so we must assume limitations where no limits can be seen, and the human mind refuses to work in that way. Speculation is the guide of science; an indispensable assistant in our exploration of the unknown; a good servant, but the worst of masters. Scientific methods differ from unscientific methods partly by their use of system, and partly in their employment of disciplined as against unrestrained speculation.

That the atomic theory has been a useful tool no one can deny; but can we, in the light of present knowledge, imagine a universe without it? We see that matter differs in its properties from point to point, and all of our experiments end in records of these differences. But is not difference a proof of discontinuity? How could a plenum vary? Even the ether itself, that mysterious medium which is thought to pervade all space, is now believed to have a granular structure, or, in other words, to be atomic. Several mathematicians have worked upon this phase of the problem with curious results; but their conclusions lie outside of my theme. The chemical atom alone concerns us now, regardless of its ultimate or physical nature, which may be exceedingly complex. The conception is so bound up with all modern chemical ideas that we cannot abandon it if we would, so long as nothing better is offered us in its place.

The chemist, then, may legitimately claim that matter, as we know it, is made up of small, distinct particles, which, so far as they have been chemically defined, are of few kinds. These particles gather into clusters, through some form of attraction whose nature is still unknown, and in which differences of position probably represent differences of chemical structure. Allotropy and isomerism are thus explainable, two phenomena that are perhaps the same, and for which the atomic theory alone has offered any reasonable interpretation. But this is not all. Certain numerical constants, commonly known as the atomic weights, have been discovered, one for each element, which are fundamental for all quantitative chemistry and for an important part of physics. These constants are real; they represent definite, measurable relations; and in one form or another they will remain in use, apart from all changes in theory. Whether they are independent of one another is yet to be determined; there are indications that they may be connected by some mathematical law; and should such an expression, a quantitative periodicity, be discovered, it would go far towards enlightening us as to the real nature of the elements themselves. The exact determination of the atomic weights is therefore a matter of supreme importance and one bearing directly upon the profounder problems of chemistry. If the atoms are separable into electrons, the masses of the latter should bear some relation to the atomic weights and give us clues to their mathematical interpretation. Future investigations along this line are certain to be made, and we may fairly hope that they will prove successful.

The nineteenth century is often called the age of steam, and its latter half the age of electricity. May we not, with equal propriety, name it the age of chemistry? During the passage of its years chemistry has developed from an art into a science, with a clear philosophy of its own, and with useful applications which affect all other sciences and many industries. A great university may now employ twenty chemists as teachers where fifty years ago there was barely work for one. Training in chemical research has become a recognized feature in higher education; the student is taught to think and investigate; the production of new knowledge is seen to be a distinct function of the teacher. Scholarship is now rated according to its fertility; and the man who merely knows, no matter how thoroughly, the work of his forerunners, is given a low rank in the thinking world. In the industries, chemical thought is translated into action, and so becomes doubly creative, yielding at the same time new knowledge and material wealth. Governments maintain public laboratories; it may be in preparation for warfare, for sanitary purposes, as aids in the enforcement of revenue laws, or for their own protection as purchasers of supplies; and so the usefulness of chemistry is felt along innumerable lines. The science advances with ever-increasing rapidity and there are as yet no signs of slackening. What shall the future be? We can distinguish necessities and express our hopes, even if we cannot prophesy. An essay of this kind would have small value if it failed to offer any helpful suggestions for the work that is to be done.

In the realm of descriptive chemistry certain work is obviously needed and is therefore likely to be done. Part of this, and the least attractive part, is revisionary — a verification of the older data with the correction of venerable errors. On the inorganic side we may predict many advances, and some of the possible lines of research we have already considered. In order to complete the periodic table the rare earths must be exhaustively studied, and the irregularities shown by iodine and tellurium, or by potassium and argon, ought to be explained. The problems of chemical structure which are offered by complex bases and acids and by double salts require elucidation, and here physico-chemical methods are likely to be most applicable. The correlation of chemical structure with crystalline form is sure to receive much attention; but what direction researches of this kind may take is not easy to foresee.

For organic chemistry I am hardly qualified to speak, at least not with regard to the more immediate urgencies. It is plain, however, to every one that there are large and important groups of compounds which await constitutional interpretation, the alkaloids and albuminoids being among them. Organo-metallic bodies also deserve a good deal of attention, for in them the two departments of descriptive chemistry meet, and each one, organic or inorganic, can be made to shed light upon the other. Finally, the relations between physical properties and chemical composition are most easily investigated upon the organic side, and here are problems enough to keep men busy for a good part of the present century. All the properties of a substance should be calculable from its composition; but the adequate data and the conclusive theory are far beyond our reach. We have a few beginnings, nothing more.

In physical chemistry, it seems to me, we find the unifying principles which are to bind all the subdivisions of our science into one. Some of the problems mentioned under the heading of descriptive chemistry are almost wholly physical in their nature; only they are statical, and leave dynamics untouched. They deal with equilibria established by transformations of energy — a statement which holds true whether we connect it with the atomic theory or base it upon the phase rule. The laws of chemical equilibrium are fundamental, beyond question; but antecedent to their application there was an interplay of active forces whose statutes are more general still. What is the nature of chemical change, and what laws govern its transformations of energy? These, to my mind, are the most general questions of dynamical chemistry. They are raised by every reaction, and they involve the consideration of all the physical forces. The problems of thermo-chemistry, of electro-chemistry, of optical chemistry, are mere special cases arising under the more universal general laws, and they will cease to exist when the latter have been discovered. So ideal a condition may never be reached, but we can approach it.

How, now let me ask, shall the work of the future be done? Hitherto individual initiative has been the chief agency in effecting progress, and each man has handled his own problems in his own way. By individual geniuses the greatest discoveries are made, but they are tried and tested by the collective intelligence of many laborers, more humble, perhaps, but also more patient and thorough. The genius is fortunate, but science has use for plodders as well, who furnish the commonplace facts that are the raw material from which laws and generalizations are developed. The great thinker needs only opportunity and encouragement; the rank and file of investigators, it seems to me, require something more. We need not fear that personal effort will cease; and still we may fairly ask whether it is sufficient for the tasks which are now waiting to be done.

One result of individualism in scientific research is evident. Our knowledge increases irregularly, unsymmetrically — with one phase over-developed and another neglected. In every group of data there are gaps to be filled, side by side with needless duplications. One man finishes a research only to find himself anticipated by some more fortunate worker, and he feels that his labor has been thrown away. Competition is a good thing, but coöperation is better, for it insures that economy of effort which is as important in intellectual affairs as it is in the factory or in commerce. Can we, without stifling enthusiasm, without harming the individual, encourage the organization of research, and so give to science a swifter growth and a more perfect symmetry? That vague but potent agency, "the spirit of the time," has taken "organization" for one of its watchwords, and we cannot escape from its spell. Collectivism and individualism, however, are not necessarily antagonistic; they are two forces acting side by side, and each helping the other. A man best develops himself when he works in harmony with his fellows.

Chemical societies are an invention of the nineteenth century, and they stand for one step in the right direction. In their meetings, by conference and discussion, and in their publications, by making research effective, they have done much to encourage investigation, and to avert, in some measure, useless duplications of effort. Through committees, they sometimes direct the growth of science, not by the exercise of compulsion, but by classifying work that has been done and showing where work is needed. An extension of this process might easily be devised, in such manner that a definite field of study should be divided among a number of scholars, each doing his own share and earning whatever independent credit he deserved. In astronomy we already have an example to follow, for observatories have divided a part of their work in exactly this manner, each institution mapping a zone of stars assigned to it by mutual agreement. Coöperative research upon a well-considered plan ought to be possible among chemists. Some overlapping, some duplication, cannot be avoided, but the waste can at least be diminished.

There is one other step which needs to be taken, and one which I have repeatedly urged on other occasions. There should be laboratories organized, equipped, and manned for systematic chemical research upon those problems which are too large for individuals to handle. The exhaustive determination of constants, for example, must precede the development of laws, and few chemists laboring singly care to attempt work of so tedious a nature. Each one often feels the need of data which do not exist, wants that he is unable personally to supply, and such a laboratory as I have in mind could render invaluable service. Astronomy has its observatories, biology is provided with experimental stations, physics is represented by institutions like the Reichsanstalt, while chemistry is almost unaided. Chemistry, the creator of wealth, receives few endowments, and those which have fallen to its share have been in aid, not of research, but of teaching. Great things have been and will yet be achieved in the universities, but their laboratories can cover no more than a small portion of the field. A laboratory for research would not compete with them; it would, on the other hand, reinforce their efforts. When, a hundred years hence, the progress and development of chemistry during the twentieth century is summed up, investigations carried on under endowments will fill a conspicuous portion of the stage. I have faith in the future; I believe it will be better than the past; and to my mind the great advances in science which we celebrate are only a beginning.

SECTION A — INORGANIC CHEMISTRY

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(*Hall* 16, *September* 21, 10 *a. m.*)

CHAIRMAN: PROFESSOR JOHN W. MALLET, University of Virginia.
SPEAKERS: PROFESSOR HENRI MOISSAN, The Sorbonne; Member of the Institute of France.
SIR WILLIAM RAMSAY, K.C.B., Royal Institution, London.
SECRETARY: PROFESSOR WILLIAM L. DUDLEY, Vanderbilt University.

INORGANIC CHEMISTRY: ITS RELATIONS WITH THE OTHER SCIENCES

BY HENRI MOISSAN

(*Translated from the French by Professor R. S. Woodworth, Columbia University*)

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CHEMISTRY, though young as a science, traces its first applications back to the very cradle of the human race. As soon as man in his struggle with nature had come into possession of his individuality, his observing intelligence enabled him to take cognizance of some of the phenomena occurring about him, and to engage in the study of them. He saw the importance of fire, and soon recognized that certain metallic substances could take the place of flint in the manufacture of weapons. Thereupon he devoted himself to that primitive metallurgy of copper, of which we still find so many examples, more or less transformed, in the earliest foundations of Babylon; these remain as witnesses, not dumb, though far from explicit, of the most remote of our civilizations.

The importance of metal in the different stages of human development is so well recognized that a single name is used to cover all the centuries that have made use of the same metal.

To the age of copper succeeds the age of bronze. At the same time gold, being found in the native state, becomes known, and is wrought with the hammer. Iron, since its preparation is much more difficult, cannot be utilized till later.

In these distant times, the epoch most fertile in chemical applications was that of the Egyptian civilization. After many industrial experiments, this people succeeded in dyeing fabrics with purple, in working the rare metals, in making enamels by fusion, in producing and fashioning glass, and in preparing fermented liquors.

On the other hand, a little people, whose part in everything was to throw the most brilliant light on it without inventing new applications, sought to explain, philosophically, these transformations of matter. The Greek philosophers discussed this subject at length. Empedocles reduced all the bodies that nature can present to four elements: fire, air, water, and earth. For him, these elements were composed of a multitude of minute particles, indivisible and insecable. Such a theory leads easily to the atoms of Democritus. Whether these elements were considered as symbols or as a veritable classification of matter, the idea of Empedocles, adopted by Aristotle, and taught by all the schools, was destined to maintain for centuries the position of a doctrine that could not be disputed.

Later, Epicurus revives the theory of atoms, and Lucretius, in a poetic divination, can write:

> Principio, quoniam terra corpus, et humor
> Aurarumque leves animae, calidique vapores,
> E quibus haec rerum consistere summa videtur,
> Omnia nativo ac mortali corpore constant,
> Debet eodem omnis mundi natura putari.[1]

The idea of the four elements reappears unchanged among the Arabian chemists, and among the alchemists of the Middle Ages; though it undergoes various transformations at the hands of Paracelsus who recognized five elements: spirit; mercury; phlegm or water; salt, sulphur, or oil; and earth, — and later at the hands of Beecher who admits the existence of three essences in earth, — vitrifiable, inflammable, and mercurial earth.

The theory of four elements reigns without contest up to the moment when Stahl, professor at the University of Halle, develops his important conception of phlogiston. For Beecher, combustible bodies and metals contained his three sorts of earth combined. For Stahl, "inflammable earth" becomes phlogiston. Carbon, by its combustion, gives heat and light; it therefore contains phlogiston. When a calx, that is to say, a metallic oxide, is heated with carbon, it extracts and fixes the phlogiston, and becomes a metal. These were important conceptions, because they made it possible to unite in one body of doctrine the phenomena of oxidation and of reduction.

Such was the state of the science when Lavoisier followed up his memorable experiments by developing the notion of simple substances. This great savant showed that the same body could change its state, and he separated clearly, among the phenomena of chemistry, on the one side the weight of the reacting bodies, and on the other side the heat set free. By weighing the simple substances that were combined and also the compound produced, he definitely established the weight equation of the chemical reaction. By measuring with the calorimeter the amounts of heat set free, he separated ponderable matter from the imponderable agents. All these views were, in addition, logically bound up with each other, and it would have been impossible to study properly the phenomena of combustion if he had not been forming an exact idea of the passage of a body from the solid to the liquid and gaseous states.

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[1] "First, since earth, and water, and the light breath of air, and glowing fire — of which our world seems to be composed — all consist of matter that is subject to birth and to death, we must think that the whole universe is made up of this same sort of matter."

We need not here recall his memorable experiments on the composition of air and of water, on the increase of the weight of metals during their oxidation, on the phenomena of combustion, respiration, and the production of animal heat, and on fermentation, or finally his creation of the nomenclature. These new ideas overthrew the theory of phlogiston. They brought light into the midst of the laborious researches of the alchemists, they prepared the way for organic and physiological chemistry, they gave rigor and exactitude to chemical reactions. In a word, they established chemistry in the position of a science.

Starting from this epoch, we can divide into three great periods the numerous researches that were pursued in different countries. In the first period, the modern idea of elements takes shape; in the second, the chemical laws are established; and in the third, the atomic weights of the elements are determined.

The first period includes the studies of a great number of investigators, but among them four names emerge above all others, — Scheele, whose chemical genius enriched our science; Priestley, a mind at once original and conservative; Cavendish, whose analyses have never been surpassed; and finally Humphry Davy, who, by the discovery of the metals of the alkalies and alkaline earths, explained the composition of the earths and won definitive acceptance for the conception of elements.

The second period presents to us the legislators of our science. Wenzel, following up the work of Rouelle, gives precision to the knowledge of salts and of double decompositions. Richter publishes the first tables of neutralization for acids and bases. Proust formulates the law of the constancy of proportions (1806); and Dalton, at the same time, gives a complete exposition of the law of multiple proportions, a first sketch of which he had presented, in 1803, to the literary and scientific society of Manchester. As we shall see further on, the importance of Dalton's law was not appreciated at its full value till much later. Finally, in 1808, Gay Lussac indicated the laws, so simple, of the combination of gases. By their promulgation, Gay Lussac gave to the concept of combination a truly mathematical exactitude.

After that, the study of the weight of the different elements which enter into combination could be pursued with success, especially when Mitscherlich's law of isomorphism (1819) and Dulong and Petit's law of specific heat (1819) became known. In this third period, in which experimental precision was carried to its furthest limits, alongside of the researches of Victor Regnault, Faraday, Marignac, and many others, the most important works published on the subject that we are considering are those of Berzelius, Dumas, and Stas.

The magnificent effort of Berzelius provided a study, as complete as possible, of most of our simple substances. This line of experiment was taken up with the greatest care by Dumas, who first determined the composition, by weight, of water and of air; and then by means of simple and elegant experiments gave us a certain number of atomic weights, among them that of carbon, the pivot of all organic chemistry. Stas next took up the study of these questions, and, *à propos* of William Prout's hypothesis of the unity of matter, showed clearly that the atomic weights are not simple multiples of unity. Stas's experiments will remain in our science as models of precision.

During this splendid period, which requires about a century, the theories by which we bind together the innumerable details of our science had time to change more than once.

We have already seen how Lavoisier's ideas replaced the theory of phlogiston. Later, Humphry Davy, after his splendid discoveries, assigned a predominating rôle to electricity and created the electrochemical theory, which was adopted and modified by Berzelius. Then came the investigations of vapor density, and, after prolonged discussions, many chemists abandoned the numbers of Berzelius, and followed the so-called notation of equivalents, proposed by Wollaston and adopted by Gmelin, Liebig, and Dumas. But soon Gerhardt, considering as equivalents the quantities of hydrochloric acid, water, and ammonia, which correspond to equal volumes, proposed a system of atomic weights, which won as adherents: in France, Laurent, Wurtz, Friedel; in England, Williamson, Frankland; in Germany, Hofmann, Kekulé, Baeyer; in Italy, Cannizaro. The hypothesis of Avogadro and Ampère took on new life, and a sharp distinction between atoms and molecules made possible a reconstitution of the atomic theory on the basis of the great law of Dalton.

Considerably before this time chemistry was divided into two great chapters: inorganic and organic.

The study of organic chemistry had begun with the investigations of Lavoisier. During the succeeding century and more, chemists tried first of all to isolate the proximate principles of vegetables and animals. These studies, pursued on all sides with varying success, endowed chemistry with a great number of clearly defined compounds, some of which possessed important therapeutic properties.

The analysis of all these substances was a rather delicate task, and, as is the rule in the sciences, no definitive results could be established until the methods of analysis were carried to a sufficient point of exactness. Only after this preliminary work was it possible to classify these innumerable compounds. Various theories then followed, till at last synthesis came to complete the work that had been begun. We recall the great researches of Berthelot on this subject: synthesis of the proximate principles of the animal fats, of the alcohols, acids, carbides (acetylene in particular), camphor, different essences. The vital force accepted by Berzelius, Liebig, and Gerhardt no longer existed. Though man's power was limited in so many things, he could make by synthesis inert organic matter.

Soon appears Kekulé's schema giving a new orientation to organic chemistry; and the synthesis of the most complicated compounds is successfully attempted. Graebe and Liebermann accomplish the synthesis of alizarin; and later, in a magnificent study of indigo, Baeyer is able to state that the position of every atom in the molecule of this dye has been experimentally determined. From these researches issue the different syntheses of indigo. Finally Emil Fischer achieves the syntheses of the sugars and so opens new horizons to biology.

For fifty years, the chemistry of carbon has formed a separate chapter, and has presented a marvelous spectacle in its development and in its important industrial applications. From the standpoint of research, organic chemistry — the fruitful theories of which have been slowly transformed — no longer finds any difficulty in determining the composition of the innumerable derivatives that it studies. Inorganic chemistry, on the contrary, though it has aroused so many efforts to establish the qualitative and quantitative analysis of its various compounds, is still far from completion. It is still in a stage of evolution, in spite of the recent work of Gooch, Clarke, and so many others. The reason for this is that some of the elements are as yet very incompletely studied. The large number of simple bodies included in inorganic chemistry increases the difficulty.

When a good part of the atomic weights had been established, the amount of effort that had to be devoted to organic chemistry caused the number of researches in inorganic chemistry to decrease. To-day, however, when the main lines of organic chemistry have been traced, and when in place of the virgin forest, as Hofman called it, there appears a complete city, beautifully laid out, the study of inorganic chemistry has come again into honor.

However, inorganic chemistry has been continuing its discoveries in the mean time. A certain number of new, and for the most part rare, elements have been isolated in the last thirty years. Lecoq de Boisbaudran, in 1875, obtained from Asturian blende a new and curious metal melting at 50°, gallium. Winkler, after very delicate analysis, obtained germanium from Freiberg argyrodite. Also, in 1886, the author of this lecture succeeded in isolating fluorin, which, though having a fairly wide distribution in nature, had previously resisted the efforts of Humphry Davy, Louyet, the Knox brothers, Fremy, and Gore.

Within the last few years, another series of discoveries has aroused the keen interest of the scientific world. As the result of delicate experiments for determining the density of nitrogen when prepared by chemical reaction, and when obtained from the air, Lord Rayleigh declared that the difference, which affected only the third decimal of his figures, was to be attributed to the existence of a gaseous element heavier than nitrogen, present in the atmosphere. Following up this physical determination, Lord Rayleigh and Sir William Ramsay isolated argon; and Sir William Ramsay obtained the satellites of argon, such as krypton, xenon, and neon. These studies led him also, turning his attention to the surface of the earth, to observe and study helium, the spectrum of which had been simultaneously discovered in the sun's rays by Sir Norman Lockyer and by Janssen.

These are splendid results, and they are the more curious since they deal with a series of gaseous bodies which, because of their chemical inertness, are a great embarrassment to the scientist and the philosopher.

But there is a group of metals which, in spite of the continued efforts of chemists, has never yet been fully studied. I refer to the rare earths, divided into the two series of cerium and yttrium.

In 1751, Cronstedt discovered cerite in a mine at Bastnaës. In 1794, Gadolin pointed out a rare earth, yttria, in a heavy black mineral which was found abundantly in the neighborhood of Ytterby, and which was afterwards named gadolinite. Cerium was characterized as an element, in 1804, by Berzelius and Hisinger in Sweden, and by Klaproth in Germany.

This first work was followed by numerous rather confused investigations, until Mosander, in 1839 and 1842, separated lanthanum and didymium from the true cerium. The study of cerium and its compounds was completed by the masterly researches of Cleve, and by Marignac, Brauner, Wyrouboff, and Verneuil. Still later, Mosander's didymium was separated by Auer von Welsbach into two elements, praseodymium and neodymium.

Samarium was studied by Cleve, Lecoq de Boisbaudran, Demarçay, Brauner, and Bettendorf. While examining the action of samarium, Demarçay proved the existence of a new element, europium.

Terminating his work on cerium, Mosander at once took up the study of yttria and from it separated erbia and terbia. This study was continued by Cleve, Marignac, Crookes, Delafontaine. In 1879, Cleve made it certain that erbia was a mixture of several earths, and since that epoch, many researches have followed up this subject. Four elements, yttrium, ytterbium, erbium, and Nilson's scandium, seem proved beyond question. The splendid work of Cleve has shown that there are yet other elements belonging to this group, in particular holmium. From the yttria group, too, Marignac has isolated an earth which has been named by Lecoq de Boisbaudran, oxide of gadolinium.

In spite of the continued efforts of the Swedish school, in spite of the researches of so many authorities, Berzelius, Mosander, Cleve, Nilson, Crookes, Marignac, Lecoq de Boisbaudran, Demarçay, Brauner, Wyrouboff, and Verneuil, this great problem of the rare earths is far from being finished. The separation of these different oxides remains one of the most difficult operations of chemistry, and yet when one compares elements so closely akin as these, one feels what interest for science would attach to a complete study of them.

In short, inorganic chemistry has never ceased progressing; and it has taken advantage of all the discoveries achieved in the other sciences.

The most striking example of this is spectral analysis. We may recall that Wollaston had in 1802 indicated the discontinuity of the solar spectrum. Later, in 1815, Frauenhofer studied the darkened rays of the solar radiation, and the luminous rays of certain spectra. Though numerous studies of this subject were made by Brewster, Wheatstone, Alter, Ångström, Masson, and Plücker, it was not till Kirchhoff's great discovery, in 1860, that the perfect correspondence between the luminous rays of different spectra and the black portions of the solar and stellar radiations became known.

Spectral analysis was thereupon inaugurated by Kirchhoff and Bunsen, and its value was immediately demonstrated by their discovery of the new elements, rubidium and cæsium. Inorganic chemistry appropriated the new method. Sir William Crookes indicated the existence of thallium, which was isolated soon after by Lamy. Reich and Richter discovered indium. Next came the discovery of gallium. Finally, in the hands of many authorities, Bunsen, Thalén, Cleve, Nilson, Crookes, Lecoq de Boisbaudran, Demarçay, Becquerel, Benedicks, this method was applied to that difficult problem of the rare earths.

The simple phenomenon of reversed lines was to extend the field of analytical chemistry to the limits of the furthest visible stars. It was destined to demonstrate that the same matter was distributed throughout the whole universe. In fact, Kirchhoff detected in the atmosphere of the sun the presence of sodium, calcium, and barium; of manganese, iron, chromium, copper, and zinc. Subsequently, Ångström and Thalén proved the existence in the sun of hydrogen, magnesium, and aluminium. Sir Norman Lockyer, in his beautiful spectral studies on the analysis of the heavenly bodies, showed that the sun contained also cadmium, strontium, cerium, lead, and potassium. Higgins followed this up by examining the spectra of stars and nebulæ, in which he met with the same elements. Le P. Secchi showed that the spectrum of comets gave the rays of the hydrocarbons.

This whole great question was reviewed and put into shape by use of new methods, by Rowland, professor in the university at Baltimore. He has given us the most important results that we possess on the composition of the sun as determined by the study of its spectrum. He has distinguished 20,000 rays, only a third of which are surely coincident with our terrestrial rays. It is true, however, that among the coincidences occur the most powerful rays of elementary bodies. Rowland concludes from this that if the earth were raised to the temperature of the sun, it would give almost the same spectrum.

Inorganic chemistry has further utilized spectral analysis for the study of band spectra, which serve the chemist as a means of analysis.

Were there any need of another example to show the fusion of inorganic chemistry and physics, we could recall the many applications of electrolysis which are utilized by chemists. Scarcely had Volta published his great discovery of the electric pile when Carlisle and Nicholson put it into use for the decomposition of water, and only a few years afterwards Humphry Davy prepared by this process the metals of the alkalies and alkaline earths. These metals in their turn served for the isolation of boron, silicon, magnesium, and aluminium.

Since that time, not a year passes without calling in electrolysis to enlarge the field of our discoveries. Many metalloids and metals are obtained to-day by this means, and the most active agent in inorganic chemistry, fluorin, could be isolated by no other method. But we ought also to recall that the study of electrochemistry, and the splendid researches of Faraday on electric conductivity, completed and extended as they were by Kohlrausch, have started chemistry in a new direction which has led to most valuable results. So true is this that Lord Rayleigh could say, at the Montreal meeting of the British Association, "It is by the study of electrolysis that we can hope to increase our knowledge of chemical reactions and of the forces that produce them; in my opinion, the next advance of the science will be by that road."

This penetration of physics into chemistry became more complete as the result of the masterly studies of Henri Sainte Claire Deville on dissociation. By systematically examining the incomplete decompositions of a certain number of substances and by showing the close connection between this dissociation and evaporation, Deville broke down the barriers that separated physical and chemical phenomena. He explained many little-understood reactions, and showed how inverse reactions were produced and how the minerals were formed in metallic veins.

Henri Debray soon demonstrated the value of Deville's ideas by his elegant experiments on the decomposition of carbonate of lime and of hydrated salts.

This matter of dissociation had also certain points of contact with the phenomena of equilibrium of which mention was made in the important memoirs of Berthelot and Péan de Saint Gills, on speeds of etherification. But I do not wish to enter upon the history of this question, for my colleague Mr. van't Hoff will speak to you about it at the Congress of Physical Chemistry, much better than I could. I will only say that at all times the two sciences of physics and chemistry have been of mutual aid and support. Victor Regnault began this great movement of physical chemistry, illumined by the brilliant discoveries of Deville, enlarged by the work of Joule, and continued with such success by Gibbs, van der Waals, van 't Hoff, Arrhenius, and Ostwald.

Passing to a different order of ideas, we may recall the splendid work of Pasteur on molecular dyssymmetry, from which started the very original investigations of Le Bel and van't Hoff on the isomerism of substances possessing rotatory power.

At every turn, inorganic chemistry depends on the data of physics. The determination of physical constants is an everyday performance in our laboratories, and often is the only guarantee of the purity of our preparations. In doubtful cases, when it becomes difficult to establish an atomic weight, the law of Dulong and Petit gives us valuable information. The whole of thermo-chemistry, indeed, founded with such success by Berthelot and by Thomson, makes use only of the methods of calorimetry.

There is another branch of physics which is called upon to render service to inorganic chemistry, and which has had a great development in the last few years; I refer to the easy production of high and low temperatures.

Metallurgy and ceramics have for thousands of years made use, industrially, of high temperatures for obtaining metals, glass, and terra-cotta. These high temperatures were secured by the combustion of wood or coal. Later, savants concentrated the solar heat by means of mirrors or burning-glasses for accomplishing some interesting experiments. Two centuries ago, the importance of the action of heat in the different reactions was so well appreciated that it served as the basis for Stahl's theory of phlogiston. And when chemistry established itself as a science, the ideas of Lavoisier on combustion were the starting-point of this profound transformation.

The employment of the oxyhydrogen blowpipe enabled Robert Hare, professor in Philadelphia, to obtain, in 1802, temperatures higher than those of the most powerful industrial furnaces, and to carry out on a small scale several very curious experiments, such as the fusion of platinum and the volatilization of silica. You know how happily Deville and Debray later applied the oxyhydrogen jet for studying the metallurgy of the platinum group. The question of the heating of ordinary furnaces was after long discussion answered both practically and theoretically by the work of Ebelmen and the important researches of Siemens.

Each of these advances was followed by a set of chemical discoveries consisting either in the more profound study of certain reactions or in the appearance of new compounds which enriched first science, and finally industry.

But the oxyhydrogen jet does not permit the attainment of a higher temperature than 1800°. The melting-point of platinum, as determined by M. Violle, is 1775°. It would be useful to study our chemical reactions above that temperature.

When we wished to reproduce the diamond, we soon saw that our study must be extended to include the various forms of carbon. So generalized, the question included the interesting topic of the solubility of carbon in melted metals. Now, as some of the metals had very high melting-points, we tried experiments with the aid of the oxyhydrogen blowpipe. Under these conditions, the fusion of the metal, in presence of an excess of carbon, occurred in an atmosphere rich in watery vapor, and therefore oxidizing. On the other hand, the combustion of the coal, and the vapor of carbon, furnished a reducing medium. The consequence was that unless a constant temperature was maintained, it was impossible to get a definite equilibrium between these opposite reactions. Besides, in these conditions complete reactions could not be obtained, and the results were variable from one experiment to another.

Already different investigators, among both scientific and industrial workers, had tried to utilize the high temperature of the electric arc, discovered a century ago by Humphry Davy. But these attempts could not be successful until the perfection of the dynamo-electric machine. Gramme's discovery and the gradual improvement of the dynamo finally placed in the hands of chemists a powerful source of electric current which was easily transformed into heat.

By a curious coincidence, our science has been able, within a few years, to thrust back the known frontiers of both heat and cold. After the important experiments of M. Cailletet, which served as the starting-point of these new studies, and after the original investigations of Raoul Pictet, Olszewski and Wroblewski, Sir James Dewar was able to obtain liquid hydrogen in the static condition, and by vaporizing this to reach the lowest temperature yet attained, that of the solidification of hydrogen, −252.5°, or 20.5° above absolute zero. Thus the usable scale of temperature has been considerably lengthened.

Less fortunate than Sir James Dewar, we have not succeeded, in a long series of experiments made by use of the electric furnace, in determining exactly the extreme limit of temperature reached. As the outcome of some delicate experiments, M. Violle has assigned as the boiling-point of carbon the temperature of 3500°. But as we shall prove further on, the temperature of the arc increases with the intensity of the current, and the measurement of these high temperatures requires further investigation. In order to fix the conditions of our experiments, we carefully stated the voltage and amperage of the current and the duration of each experiment. The diameter of the electrodes and the capacity of the furnace had been determined beforehand, and remained constant.

At the very start, we found that at the temperature of our electric furnace, the metallic oxides, hitherto regarded as irreducible, were easily decomposed. Also reactions which were only partial at the highest temperatures of ordinary furnaces became total here. A large number of compound substances were dissociated at these high temperatures, and on the other hand, new series of combinations, definite and crystallized, were obtained. We thus prepared unknown compounds of great stability, such as the carbides, borides, and silicides. Most of these new binary compounds can also be partly or wholly broken up by still further increasing the intensity of the current and with it the temperature.

Some of these carbides will furnish us a very definite scale of dissociation. We also meet here, in the neighborhood of 3000°, the same general laws which govern the decomposition of substances by heat at lower temperatures. Moreover, the boiling of a mixture of copper and lead, of tin and lead, or of copper and tin, presents the same peculiarities between 2000° and 3000° as does a mixture of water and ether, of water and alcohol, or of water and formic acid, at much lower temperatures. The laws of the fractional distillation of two liquids apply therefore to the boiling of metals at very high temperatures.

In using our electric furnace, we operate in a reducing atmosphere, and if a strong enough current is employed we get very quickly a constant temperature, which is the boiling-point of quicklime. If, on the contrary, the substance to be studied is put very close to the arc, that is to say, very close to the gaseous conductor composed of carbon vapor which unites the electrodes, the temperature rises with the intensity of the current. A chemical reaction proves this. With a current of 100 amperes and 50 volts, the reduction of titanic acid by the carbon gives an oxide of an indigo blue color. With

500 amperes and 70 volts, a fused mass of yellow nitride is obtained; while the high temperature of an arc of 1200 amperes and 70 volts gives a carbide of titanium free from nitrogen. With so intense a current as this last the nitride of titanium can no longer be formed; its dissociation by heat is complete and only the carbide can remain.

In pursuing this study, we have found still other examples of combination and then decomposition under the action of an electric arc of greater and greater intensity.

Organic chemistry comes into contact with biology; whence its greatness and also its difficulties.

Biological chemistry could not be developed till after a systematic study of the chemistry of carbon had been made. For a century it was thought that physiological chemistry needed in its manifold transformations only the four elements, carbon, hydrogen, nitrogen, and oxygen. But in recent years our ideas on this point have changed considerably. It has long been known that iron was indispensable in both the animal and vegetable kingdoms. Further, Raulin had proved by some curious experiments the important influence of traces of foreign metals on the culture of aspergillus niger. These experiments had been forgotten; they came too early.

But to-day discoveries relating to this point appear in constantly greater numbers. For example, the fine work of Frederick and of Henze has shown that copper is a constituent of hemocyanin in the blood of cuttlefishes and crustacea. We know now that iodin and bromin should be found in the thyroid gland; these elements are seen to be indispensable to the regular course of normal life. The existence of arsenic in animal tissues was a thing unheard of a few years ago. Professor Armand Gautier has now established by very delicate experiments that arsenic is always present in the horny tissues and in the thyroid gland; and M. Gabriel Bertrand has demonstrated the normal existence of arsenic in the living cells of fishes taken from the sea-bottom at a depth of 3000 meters.

In the same way, a trace of another element, such as manganese, may intervene in the form of a soluble ferment, in the oxydases. One understands then the importance of the different elements and sees that sometimes, in traces, they may play a physiological rôle of capital importance. We have long known that sulphur forms part of the proteid molecule, although we are still quite ignorant of the transformations which bring this element into complex compounds.

It is quite evident that on this point great discoveries still await their realization. We are only beginning to-day the study of the different elements in their combinations with carbon, from the physiological point of view; it may be said that the physiology of the cell remains wholly to be made. We are happy to know that a start in this direction is being made by means of microchemical reactions.

Biology therefore unites again inorganic and organic chemistry. The truth is, there is but one chemical science; every separation is artificial. Just as energy is one, chemistry is one. The splendid researches of Curtius on nitrohydric acid, and our own investigations of the metallic carbides and of the hydrides, of the alkalies and alkaline earths, show how the two chemistries constantly interpenetrate, and demonstrate the unity of the science.

It is true, however, that inorganic chemistry has a technique of its own. To make discoveries there, the precision of physics must be applied. A few examples will make my thought clearer.

Lavoisier only overthrew the theory of Stahl as the result of rigorous experiments prepared with the greatest care and exactitude. We may refer in this connection to his experiments on combustion, on respiration, and on fermentation.

Cavendish, when studying the action of the electric spark on a mixture of oxygen and nitrogen, pushed the experiment till there remained but a very small quantity of gas incapable of uniting with oxygen. He mentions its existence. Since his time, over a century ago, in how many universities, lycea, and gymnasia has this experiment of Cavendish been repeated? And yet no one, during the century, completed the experiment. It was always begun, but never finished. Any one who had carried it on patiently till the nitrogen was entirely absorbed would have discovered argon. It was needful that Lord Rayleigh should determine the densities of the gas, vouching for the third decimal place, in order that the discovery should be realized. The method is elegant, but the path of discovery is rather circuitous.

Shall I cite you another example? When Gay Lussac, in 1815, discovered cyanogen, that first example of a compound playing the part of an element, that first radical formed of nitrogen and carbon, he prepared it by moderately heating pure, dry cyanide of mercury. The cyanide in these conditions split into cyanogen gas and mercury. The experiment is of the simplest. Only a few years before, Proust also had heated cyanide of mercury in a retort. He had obtained ammonia, an apparently oily compound, nitrogen, carbon dioxide and monoxide. The reason was that Proust used damp cyanide. This difference in manner of conducting the same experiment between two men of the ability of Gay Lussac and Proust seemed to me very interesting.

To return to Gay Lussac's preparation of cyanogen. He had left in the bottom of his retort a small quantity of a black powder. After establishing the formula of cyanogen, the existence of hydrocyanic acid and of the cyanides and cyanates, he made an analysis of this powder. It had exactly the same composition as cyanogen. Gay Lussac notes this fact, but he takes care not to go farther, and chemistry had to wait till the researches of Troost and Hautefeuille, published in 1873, before knowing the laws of the transformation of cyanogen into its polymere paracyanogen.

I might further cite for you on this point Humphry Davy's method of work; I might recall to your minds the fact that Wöhler was a master of chemical analysis, and outline for you the excellent studies of Berzelius or of Stas. If I have lingered on this topic, it is because I regard it as most important. Many great investigations remain to be made in inorganic chemistry, but to get them done, the methods must be refined and attain great precision. In a word, experimental research in chemistry should have the rigor of physical experiments.

But to return to the relations of chemistry with the other sciences. We have already spoken of physics and biology. I do not wish to enlarge beyond measure on this point. I will remind you that astronomy, thanks to spectrum analysis, the joint product of physics and chemistry, has been able to extend and develop certain of its theories to include the remotest star visible in our horizon. Moreover, the spectroscopic method of Doppler and Fizeau has been of great service in determining the speed of the heavenly bodies.

Our chemistry also comes into contact with mathematics at two important points. It comes into contact with statics in stereochemistry and the special grouping of atoms, in questions of symmetry and in combinational analysis which studies the combinations of objects associated in different conditions. It comes into contact with mathematics also on the dynamic side, in that it involves the principles of molecular mechanics in connection with the conservation of energy and the mechanical theory of heat.

Chemical analysis is one of the foundations of mineralogy. It is of the greatest service to geology, which could not do without it. The majority of the sciences have need of its assistance, and even the historian comes to it to inquire the age of the successive foundations of the ruins of Babylon, bringing to it the bronze or copper objects which the latest excavations have put into his hands.

When it comes to industrial applications of the various sciences, very few of them are not in debt to chemistry. The engineer has constant need of it. Studies of the metals and their alloys have given all their efficiency to machines, ships, and firearms. Two chapters, however, in the applications of science will depend absolutely on the progress of chemistry; we refer to the chemical industry and to rural economy, — so important that they change the destinies of nations, mingle the stocks of peoples, and modify the conditions of their existence.

It is not our part to enlarge upon this side of the question; it is enough to have mentioned it, and to recall, in closing, the vast sum of effort which these researches have demanded. In the midst of these incessant transformations, this continued progress, we see that scientific research has never had but one method: experiment. Faraday's dictum is always true: "Chemistry is essentially an experimental science."

THE PRESENT PROBLEMS OF INORGANIC CHEMISTRY

BY SIR WILLIAM RAMSAY

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To discuss the "present problems of inorganic chemistry" is by no means an easy task. The expression might be taken to mean an account of what is being actually done at present by those engaged in inorganic research; or it might be taken to relate to what needs doing — to the direction in which research is required. To summarize what is being done in an intelligible manner in the time at my disposal would be an almost impossible task; hence I will choose the latter interpretation of the title of my address. Now, a considerable experience in attempting to unveil the secrets of nature has convinced me that a deliberate effort to discover some new law or fact seldom succeeds. The investigator generally begins unmethodically, by random and chance experiments; or perhaps he is guided by some indication which has struck his attention during some previous research; and he is often the plaything of circumstances in his choice. Experience leads him to choose problems which most readily admit of solution, or which appear likely to lead to the most interesting results. If I may be excused the egotism of referring to my own work, I may illustrate what I mean by relating the following curious coincidence: After Lord Rayleigh had announced his discovery that "atmospheric nitrogen" was denser than "chemical nitrogen," I referred to Cavendish's celebrated paper on the combination of the nitrogen and the oxygen of the air by means of electric sparks. Fortified by what I read, and by the knowledge gained during the performance of lecture-experiments that red-hot magnesium is a good and fairly rapid absorbent of nitrogen, it was not long before a considerable quantity of nearly pure argon had been separated from atmospheric nitrogen. Now it happens that I possess two copies of Cavendish's works; and some months afterwards I consulted the other copy and found penciled on the margin the words "look into this." I remembered the circumstance which led to the annotation. About ten years before, one of my students had investigated the direct combination of nitrogen and hydrogen, and I had read Cavendish's memoir on that occasion. I mention this fact to show that, for some reason which I forget, a line of work was not followed up, which would have been attended by most interesting results; one does not always follow the clue which yields results of the greatest interest. I regard it therefore as an impossible task to indicate the lines on which research should be carried out. All that I can do is to call attention to certain problems awaiting solution; but their relative importance must necessarily be a matter of personal bias, and others might with perhaps greater right suggest wholly different problems.

The fundamental task of inorganic chemistry is still connected with the classification of elements and compounds. The investigation of the classification of carbon compounds forms the field of organic chemistry; while general or physical chemistry deals with the laws of reaction, and the influence of various forms of energy in furthering or hindering chemical change. And classification centres at present in the periodic arrangement of the elements, according to the order of their atomic weights. Whatever changes in our views may be concealed in the lap of the future, this great generalization, due to Newlands, Lothar Meyer, and Mendeléeff, will always retain a place, perhaps the prominent place, in chemical science.

Now it is certain that no attempt to reduce the irregular regularity of the atomic weights to a mathematical expression has succeeded; and it is, in my opinion, very unlikely that any such expression, of not insuperable complexity, and having a basis of physical meaning, will ever be found. I have already, in an address to the German Association at Cassel, given an outline of the grand problem which awaits solution. It can be shortly stated then: While the factors of kinetic and of gravitational energy, velocity, and momentum, on the one hand, and force and distance, on the other, are simply related to each other, the capacity factors of other forms of energy; — surface, in the case of surface-energy; volume, in the case of volume-energy; entropy for heat; electric capacity when electric charges are being conveyed by means of ions; atomic weight, when chemical energy is being gained or lost; — all these are simply connected with the fundamental chemical capacity, atomic weight, or mass. The periodic arrangement is an attempt to bring the two sets of capacity factors into a simple relation to each other; and while the attempt is in so far a success, inasmuch as it is evident that some law is indicated, the divergences are such as to show that finality has not been attained. The central problem in inorganic chemistry is to answer the question, why this incomplete concordance? Having stated the general question, it may conduce to clearness if some details are given.

(1) The variation of molecular surface-energy with temperature is such that the surface-energy, for equal numbers of molecules distributed over a surface, is equal for equal intervals of temperature below the temperature at which surface-energy is zero — that is, the critical point. This gives a means of determining the molecular weights of liquids, and we assume that the molecular weight of a compound is accurately the sum of the atomic weights of the constituent elements.

(2) The volume-energy of gases is equal at equal temperature from that at which volume-energy is zero — *i. e.*, absolute zero. And it follows that those volumes of gases which possess equal volume-energy contain equal numbers of molecules — again, a close connection with atomic weights.

(3) The specific heats of elements are approximately inversely proportional to their atomic weights; and of compounds to the quotient of their molecular weights divided by the number of atoms in the molecule. Specific heat and entropy are closely related; hence one of the factors of thermal energy is proportional (nearly) to the reciprocal of the atomic weights.

(4) The ion carries in its migration through a solution one or more electrons. Now, the ion is an atom carrying one or more charges — one for each equivalent. Here we have the capacity for electric charge proportional to the equivalent.

(5) The factors of chemical energy are atomic weight and chemical potential; and as the former is identical numerically, or after multiplication by a simple factor with equivalent, electric potential is proportional to chemical potential.

We see, therefore, that surface, volume, thermal, electrical,and, no doubt, other forms of energy have as capacity factors magnitudes, either identical with, or closely related to, units of chemical capacity; while kinetic and linear energy are not so related, except through the periodic arrangement of the elements.

It appears, therefore, to be a fundamental problem for the chemist to ascertain, first, accurate atomic weights, and, second, to investigate some anomalies which still present difficulties. In America, you have excellent workers in the former branch. Mallet, Morley, Richards, and many others have devoted their time and skill to perhaps the best work of this kind which has been done; and F. W. Clarke has collated all results and afforded incalculable help to all who work at or are interested in the subject. Valuable criticisms, too, have been made by Hinrichs; but it must be confessed that in spite of these, which are perhaps the best determinations which have been made, the problem becomes more, and not less, formidable.

There are lines of work, however, which suggest themselves as possibly likely to throw light on the question. First, there is a striking anomaly in the atomic weight of nitrogen, determined by analysis and determined by density. Stas obtained the number 14.04 ($O = 16$), and Richards has recently confirmed his results; while Rayleigh and Leduc consistently obtained densities which, even when corrected so as to equalize the numbers of molecules in equal volumes, give the lower figure 14.002. The difference is 1 in 350; far beyond any possible experimental error. Recently, an attempt to combine the two methods has led to a mean number; but that result can hardly be taken as final. What is the reason of the discrepancy? Its discovery will surely advance knowledge materially. I would suggest the preparation of pure compounds of nitrogen, such as salts of hydrazine, methylanine, etc., and their careful analysis; and also the accurate determination of the density and analysis of such gaseous compounds of nitrogen as nitric oxide and peroxide. I have just heard from my former student, R. W. Gray, that he has recovered Stas's number by combining $2NO$ with O_2; while the density of NO leads to the lower value for the atomic weight of nitrogen.[1]

The question of the atomic weight of tellurium appears to be settled, at least so far as its position with regard to the generally accepted atomic weight of iodin is concerned; recent determinations give the figures 127.5 (Gutbier), 127.6 (Pellini), and 127.9 (Köthner). But is that of iodin as accurately known? It would appear advisable to revise the determination of Stas, preparing the iodin preferably from an organic compound, such as iodoform, which can be produced in a high state of purity. The heteromorphism of selenates and tellurates, too, has recently been demonstrated; and it may be questioned whether these elements should both belong to the same group.

The rare earths still remain a puzzle. Their number is increasing yearly, and their claim to individuality admits of less and less dispute. What is to be done with them? Are they to be grouped by themselves as Brauner and Steele propose? If so, how is their connection with other elements to be explained? Recent experiments in my laboratory have convinced me that in the case of thorium, at least, ordinary tests of purity such as fine crystals, constant subliming point, etc., do not always indicate homogeneity; or else that we are sadly in want of some analytical method of sufficient accuracy. The change of thorium into thorium *X* is perhaps hardly an explanation of the divergences; yet it must be considered; but of this, anon.

To turn next to another problem closely related to the orderly arrangement of the elements, — that of valency, — but little progress can be chronicled. The suggestions which have been made are speculative, rather than based on experiment. The existence of many peroxidized substances, such as percarbonates, perborates, persulphates, and of crystalline compounds of salts with hydrogen peroxide, makes it difficult to draw any indisputable conclusions as regards valency from a consideration of oxygen compounds. Moissan's brilliant work on fluorides, however, has shown that SF_6 is capable of stable existence, and this forms a strong argument in support of the hexad character of sulphur. The tetravalency of oxygen, under befitting conditions, too, is being acknowledged, and this may be reconciled with the existence of water of crystallization, as well as of the per-salts already mentioned. The adherence of ammonia to many chlorides, nitrates, etc., points to the connecting link being ascribable to the pentavalency of nitrogen; and it might be worth while investigating similar compounds with phosphoretted and arseniuretted hydrogen, especially at low temperatures.

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[1] Note added February, 1906: Researches by Gray and by Guye have since shown that Stas's results are in error; and determinations by Richards allow the same conclusion to be drawn. The actual atomic weight cannot differ much from 14.007.

The progress of chemical discovery, indeed, is closely connected with the invention of new methods of research, or the submitting of matter to new conditions. While Moissan led the way by elaborating the electric furnace, and thus obtained a potent agent in temperatures formerly unattainable, Spring has tried the effect of enormous pressure, and has recently found chemical action between cuprous oxide and sulphur at ordinary temperature, provided the pressure be raised to 8000 atmospheres. Increase of pressure appears to lower the temperature of reaction. It has been known for long that explosions will not propagate in rarefied gases, and that they become more violent when the reacting gases are compressed: but we are met with difficulties, such as the non-combination of hydrogen and nitrogen, even at high temperature and great pressure; yet it is possible to measure the electromotive force (0.59 volt) in a couple consisting of gaseous nitrogen and gaseous hydrogen, the electrolyte being a solution of ammonium nitrate saturated with ammonia. Chemical action between dissolved hydrogen and nitrogen undoubtedly occurs; but it is not continuous. Again we may ask, Why? The heat evolution should be great; the gain of entropy should also be high were direct combination to occur. Why does it not occur to any measurable extent? Is it because for the initial stages of any chemical reaction, the reacting molecules must be already dissociated, and those of nitrogen are not? Is that in any way connected with the abnormally low density of gaseous nitrogen? Or is it that, in order that combination shall occur, the atoms must fit each other; and that, in order that nitrogen and hydrogen atoms may fit, they must be greatly distorted? But these are speculative questions, and it is not obvious how experiments can be devised to answer them.

Many compounds are stable at low temperatures which dissociate when temperature is raised. Experiments are being made, now that liquid air is to be purchased or cheaply made, on the combinations of substances which are indifferent to each other at ordinary temperatures. Yet the research must be a restricted one, for most substances are solid at −185°, and refuse to act on each other. It is probable, however, that at low temperatures compounds could be formed in which one of the elements would possess a greater valency than that usually ascribed to it; and also that double compounds of greater complexity would prove stable. Valency, indeed, appears to be in many cases a function of temperature; exothermic compounds, as is well known, are less stable, the higher the temperature. The sudden cooling of compounds produced at a high temperature may possibly result in forms being preserved which are unstable at ordinary temperatures. Experiments have been made in the hope of obtaining compounds of argon and helium by exposing various elements to the influence of sparks from a powerful induction coil, keeping the walls of the containing-vessel at the temperature of liquid air, in the hope that any endothermic compound which might be formed would be rapidly cooled, and would survive the interval of temperature at which decomposition would take place naturally. But these experiments have so far yielded only negative results. There is some indication, however, that such compounds are stable at 1500°. It might be hoped that a study of the behavior of the non-valent elements would have led to some conception of the nature of valency; but so far, no results bearing on the question have transpired. The condition of helium in the minerals from which it is obtainable by heat is not explained; and experiments in this direction have not furnished any positive information. It is always doubtful whether it is advisable to publish the results of negative experiments; for it is always possible that some more skilled or more fortunate investigator may succeed, where one has failed. But it may be chronicled that attempts to cause combination between the inactive gases and lithium, potassium, rubidium, and cæsium have yielded no positive results; nor do they appear to react with fluorin. Yet conditions of experiment play a leading part in causing combination, as has been well shown by Moissan with the hydrides of the alkali-metals, and by Guntz, with those of the metals of the alkaline earths. The proof that sodium hydride possesses the formula NaH, instead of the formerly accepted one, removes one difficulty in the problem of valency; and SrH_2 falls into its natural position among hydrides.

A fertile field of inorganic research lies in the investigation of structure. While the structure of organic compounds has been elucidated almost completely, that of inorganic compounds is practically undeveloped. Yet efforts have been made in this direction which appear to point a way. The nature of the silicates has been the subject of research for many years by F. W. Clarke; and the way has been opened. Much may be done by treating silicates with appropriate solvents, acid or alkaline, which differentiate between uncombined and combined silica, and this in some cases, by replacement of one metal by another, gives a clue to constitution. The complexity of the molecules of inorganic compounds, which are usually solid, forms another bar to investigation. It is clear that sulphuric acid, to choose a common instance, possesses a very complicated molecule; and the fused nitrates of sodium and potassium are not correctly represented by the simple formulæ $NaNO_3$ and KNO_3. Any theory of the structure of their derivatives must take such facts into consideration; but we appear to be getting nearer the elucidation of the molecular weights of solids. Again, the complexity of solutions of the most common salts is maintained by many investigators; for example, a solution of cobalt chloride, while it undoubtedly contains among other constituents simple molecules of $CoCl_2$, also consists of ions of a complex character, such as$(CoCl_4)''$. And what holds for cobalt chloride also undoubtedly holds for many similar compounds.

In determining the constitution of the compounds of carbon, stereochemistry has played a great part. The ordinary structural formulæ are now universally acknowledged to be only pictorial, if, indeed, that word is legitimate; perhaps it would be better to say that they are distorted attempts at pictures, the drawing of which is entirely free from all rules of perspective. But these formulæ may in almost every case be made nearly true pictures of the configuration of the molecules. The benzene formula, to choose an instance which is by no means the simplest, has been shown by Collie to be imitated by a model which represents in an unstrained manner the behavior of that body on treatment with reagents. But in the domain of inorganic chemistry, little progress has been made. Some ingenious ideas of the geologist Sollas on this problem have hardly received the attention which they deserve; perhaps they may have been regarded as too speculative. On the other hand, Le Bel's and Pope's proof of the stereo-isomerism of certain compounds of nitrogen; Pope's demonstration of the tetrahedral structure of the alkyl derivatives of tin; and Smiles's syntheses of stereo-isomeric sulphur compounds give us the hope that further investigation will lead to the classification of many other elements from this point of view. Indeed, the field is almost virgin soil; but it is well worth while cultivating. There is no doubt that the investigation of other organo-metallic compounds will result in the discovery of stereo-isomerides; yet the methods of investigation capable of separating such constituents have in most cases still to be discovered.

The number of chemical isomerides among inorganic compounds is a restricted one. Werner has done much to elucidate this subject in the case of complex ammonia derivatives of metals and their salts;

but there appears to be little doubt that if looked for, the same or similar phenomena would be discoverable in compounds with much simpler formulæ. The two forms of SO_3, sulphuric anhydride, are an instance in point. No doubt formation under different conditions of temperature and pressure might result in the greater stability of some forms which under our ordinary conditions are changeable and unstable. The fact that under higher pressures than are generally at our disposal different forms of ice have been proved to exist, and the application of the phase rule to such cases will greatly enlarge our knowledge of molecular isomerism.

The phenomena of catalysis have been extensively studied of recent years, and have obviously an important bearing on such problems. A catalytic agent is one which accelerates or retards the velocity of reaction. Without inquiring into the mechanism of catalysis, its existence may be made to influence the rate of chemical change, and to render bodies stable which under ordinary conditions are unstable. For if it is possible to accelerate a chemical change in such a way that the usually slow and possibly unrecognizable rate of isomeric change may be made apparent and measurable, a substance the existence of which could not be recognized under ordinary circumstances, owing to its infinitesimal amount, may be induced to exist in weighable quantity, if the velocity of its formation from an isomeride can be greatly accelerated by the presence of an appropriate catalytic agent. I am not aware that attempts have been made in this direction. The discovery of catalytic agents is, as a rule, the result of accident. I do not think that any guide exists which would enable us to predict that any particular substance would cause an acceleration or a retardation of any particular reaction. But catalytic agents are generally those which themselves, by their power of combining with or parting with oxygen, or some other element, cause the transfer of that element to other compounds to take place with increased or diminished velocity. It is possible, therefore, to cause ordinary reactions to take place in presence of a third body, choosing the third body with a view to its catalytic action, and to examine carefully the products of the main reaction as regards their nature and their quantity. Attempts have been made in this direction with marked success; the rate of change of hydrogen dioxide, for example, has been fairly well studied. But what has been done for that compound may be extended indefinitely to others, and doubtless with analogous results. Indications of the existence of as yet undiscovered compounds may be derived from a study of physical, and particularly of electrical, changes. There appears to be sufficient evidence of an oxide of hydrogen containing more oxygen than hydrogen dioxide, from a study of the electromotive force of a cell containing hydrogen dioxide; yet the higher oxide still awaits discovery.

The interpretation of chemical change in the light of the ionic theory may now be taken as an integral part of inorganic chemistry. The ordinary reactions of qualitative and quantitative analysis are now almost universally ascribed to the ions, not to the molecules. And the study of the properties of most ions falls into the province of the inorganic chemist. To take a familiar example: The precipitation of hydroxides by means of ammonia-solution has long led to the hypothesis that the solution contained ammonium hydroxide; and indeed, the teaching of the text-books and the labels on the bottles supported this view. But we know now that a solution of ammonia in water is a complex mixture of liquid ammonia and liquid water; of ammonium hydroxide, NH_4OH; and of ions of ammonium $(NH_4)'$, and hydroxyl $(OH)'$. Its reactions, therefore, are those of such a complex mixture. If brought into contact with a solution of some substance which will withdraw the hydroxyl ions, converting them into water, or into some non-ionized substance, they are replaced at the expense of the molecules of non-ionized ammonium hydroxide; and these, when diminished in amount, draw on the store of molecules of ammonia and water, which combine, so as to maintain equilibrium. Now the investigation of such changes must belong to the domain of inorganic chemistry. It is true that the methods of investigation are borrowed from the physical chemist; but the products lie in the province of the inorganic chemist. Indeed, the different departments of chemistry are so interlaced that it is impossible to pursue investigations in any one branch without borrowing methods from the others; and the inorganic chemist must be familiar with all chemistry, if he is to make notable progress in his own branch of the subject. And if the substances and processes investigated by the inorganic chemist are destined to become commercially important, it is impossible to place the manufacture on a sound commercial basis without ample knowledge of physical methods, and their application to the most economical methods of accelerating certain reactions and retarding others, so as to obtain the largest yield of the required product at the smallest cost of time, labor, and money.

I have endeavored to sketch some of the aspects of inorganic chemistry with a view to suggesting problems for solution, or at least the directions in which such problems are to be sought. But the developments of recent years have been so astonishing and so unexpected, that I should fail in my duty were I not to allude to the phenomena of radioactivity, and their bearing on the subject of my address. It is difficult to gauge the relative importance of investigations in this field; but I may be pardoned if I give a short account of what has already been done, and point out lines of investigation which appear to me likely to yield useful results.

The wonderful discovery of radium by Madame Curie, the preparation of practically pure compounds of it, and the determination of its atomic weight, are familiar to all of you. Her discovery of polonium, and Debierne's of actinium, have also attracted much attention. The recognition of the radioactivity of uranium by Becquerel, which gave the first impulse to these discoveries, and of that of thorium by Schmidt, is also well known.

These substances, however, presented at first more interest for the physicist than the chemist, on account of the extraordinary power which they all possess of emitting "rays." At first, these rays were supposed to constitute ethereal vibrations; but all the phenomena were not explicable on that supposition. Schmidt first, and Rutherford and Soddy later, found that certain so-called "rays" really consist of gases; and that while thorium emits one kind, radium emits another; and no doubt Debierne's actinium emits a third. The name "emanations" was applied by Rutherford to such radioactive bodies; he and Soddy found that those of radium and thorium could be condensed and frozen by exposure to the temperature of liquid air, and that they were not destroyed or altered in any way by treatment with agents which are able to separate all known gases from those of the argon group, namely, red-hot magnesium-lime, and it was later found that sparking with oxygen in presence of caustic potash did not affect the gaseous emanation from radium. The conclusion therefore followed that in all probability these bodies are gases of the argon group, the atomic weight of which, and consequently the density, is very high; indeed, several observers, by means of experiments on the rate of diffusion of the gas from radium, believe it to have a density of approximately 100, referred to the hydrogen standard. This conclusion has been confirmed by the mapping of the spectrum of the radium emanation, which is similar in general character to the spectra of the inactive gases, consisting of a number of well-defined, clearly cut brilliant lines, standing out from a black background. The volume of the gas produced spontaneously from a given weight of radium bromide in a given time has been measured; and it was incidentally shown that this gas obeys Boyle's law of pressures. The amount of gas thus collected and measured, however, was very minute; the total quantity was about the forty-thousandth of a cubic centimeter.

Having noticed that those minerals which consist of compounds of uranium and thorium contain helium, Rutherford and Soddy made the suggestion that it might not be impossible that helium is the product of the spontaneous change of the emanation; and Soddy and I were able to show that this is actually the case. For, first, when a quantity of radium salt which has been prepared for some time is dissolved in water, the occluded helium is expelled, and can be recognized by means of its spectrum; further, the fresh emanation shows no helium spectrum, but after a few days the spectrum of helium begins to appear, proving that a spontaneous change is in progress; and last, as the emanation disappears its volume decreases to zero; and on heating the capillary glass tube which contained it, helium is driven out from the glass walls, into which its molecules had been imbedded in volume equal to three and a half times that of the emanation. The a rays, as foreshadowed by Rutherford and Soddy, consist of helium particles.

All these facts substantiate the theory, devised by Rutherford and Soddy, that the radium atom is capable of disintegration, one of the products being a gas, which itself undergoes further disintegration, forming helium as one of its products. Up till now, the sheet-anchor of the chemists has been the atom. But the atom itself appears to be complex, and to be capable of decomposition. It is true that only in the case of a very few elements, and these of high atomic weight, has this been proved. But even radium, the element which has by far the most rapid rate of disintegration, has a comparatively long life; the period of half-change of any given mass of radium is approximately 1100 years. The rate of change of the other elements is incomparably slower. This change, too, at least in the case of radium and its emanation, and presumably also in the case of other elements, is attended with an enormous loss of energy. It is easy to calculate from heat measurements (and independent and concordant measurements have been made) that one pound of emanation is capable of parting with as much energy as several hundred tons of nitroglycerine. The order of the quantity of energy evolved during the disintegration of the atom is as astonishing as the nature of the change. But the nature of the change is parallel to what would take place if an extremely complicated hydrocarbon were to disintegrate; its disruption into simpler paraffins and olefines would also be attended with loss of energy. We may therefore take it, I think, that the disintegration hypothesis of Rutherford and Soddy is the only one which will meet the case.

If radium is continually disappearing, and would totally disappear in a very few thousand years, it follows that it must be reproduced from other substances, at an equal rate. The most evident conjecture, that it is formed from uranium, has not been substantiated. Soddy has shown that salts of uranium, freed from radium, and left for a year, do not contain one ten-thousandth part of the radium that one would expect to be formed in the time. It is evident, therefore, that radium must owe its existence to the presence of some other substances, but what they are is still unascertained.

During the investigation of Rutherford and Soddy of the thorium emanation, a most interesting fact was observed, namely, that precipitation of the thorium as hydroxide by ammonia left unprecipitated a substance, which they termed thorium X, and which was itself highly radioactive. Its radioactive life, however, was a short one; and as it decayed, it was reproduced from its parent thorium at an equal rate. Here is a case analogous to what was sought for with radium and uranium; but evidently uranium is not the only parent of radium; the operation is not one of parthenogenesis. Similar facts have been elicited for uranium by Crookes.

The α rays, caused by the disintegration of radium and of its emanation, are accompanied by rays of quite a different character; these are the β rays, identical with electrons, the mass of which has been measured by J. J. Thomson and others. These particles are projected with enormous velocity, and are capable of penetrating glass and metal screens. The power of penetration appears to be proportional to the amount of matter in the screen, estimated by its density. These electrons are not matter; but, as I shall relate, they are capable of causing profound changes in matter.

For the past year, a solution of radium bromide has been kept in three glass bulbs, each connected to a Topler pump by means of capillary tubing. To insure these bulbs against accident, each was surrounded by a small beaker; it happened that one of these beakers consisted mainly of potash glass; the other two were of soda glass. The potash-glass beaker became brown, while the two soda-glass beakers became purple. I think there is every probability that the colors are due to liberation of the metals potassium and sodium in the glass. They are contained in that very viscous liquid, glass, in the colorless ionic state; but these ions are discharged by the β rays, or negative electrons, and each metal imparts its own peculiar color to the glass, as has been shown by Maxwell Garnett. This phenomenon, however interesting, is not the one to which I desire to draw special attention. It must be remembered that the beakers have been exposed only to β rays; α rays have never been in contact with them; they have never been bombarded by what is usually called matter, except by the molecules of the surrounding air. Now these colored beakers are radioactive, and *the radioactive film dissolves in water*. After careful washing, the glass was no longer radioactive. The solution contains an emanation, for on bubbling air through it, and cooling the issuing air with liquid air, part of the radioactive matter was retained in the cooled tube. This substance can be carried into an electroscope by a current of air, after the liquid air has been withdrawn, and as long as the air-current passes, the electroscope is discharged; the period of decay of this emanation, however, is very rapid, and on ceasing the current of air, the leaves of the electroscope cease to be discharged. In having such a short period of existence, this emanation resembles the one from actinium.

Owing to the recess, only a commencement has been made with the investigation of the residue left on evaporation of the aqueous solution. On evaporation, the residue is strongly active. Some mercurous nitrate was then added to the dissolved residue, and it was treated with hydrochloric acid in excess, to precipitate mercurous chloride. The greater part of the active matter was thrown down with the mercurous chloride, hence it appears to form an insoluble chloride. The mercurous chloride retained its activity unchanged in amount for ten days. The filtrate from the mercurous chloride, on evaporation, turned out to be active; and on precipitating mercuric sulphide in it, the sulphide precipitate was also active; but its activity decayed in one day. The filtrate from the mercuric sulphide gave inactive precipitates with ferric salts and ammonia, with zinc salts and ammonium sulphide, with calcium salts and ammonium carbonate; and on final evaporation, the residue was not radioactive. Hence the active matter forms an insoluble chloride and sulphide. The precipitated mercurous chloride and mercuric sulphide were dissolved in *aqua regia*, and the solution was evaporated. The residue was dissolved in water, and left the dish inactive. But the solution gave an insoluble sulphate, when barium chloride and sulphuric acid were added to it; hence the radioactive element forms an insoluble sulphate, as well as an insoluble chloride and sulphide.

This is a sample of the experiments which have been made. It may be remarked that the above results were obtained from a mixture of the potash and soda glass; somewhat different results were obtained from the potash glass alone. These changes appear to be due to the conversion of one or more of the constituents of the glass into other bodies. Needless to say, neither of the samples of glass contained lead.

I have mentioned these experiments in detail, because I think they suggest wholly new lines of investigation. It would appear that if energy can be poured into a definite chemical matter, such as glass, it undergoes some change, and gives rise to bodies capable of being tested for; I imagine that radioactive forms of matter are produced, either identical with or allied to those at present known. And just as radium and other radioactive elements suffer degradation spontaneously, evolving energy, so I venture to think that if energy be concentrated in the molecules of ordinary forms of matter, a sort of polymerization is the result, and radioactive elements, probably elements with high atomic weight, and themselves unstable, are formed. Of course further research may greatly modify these views; but some guide is necessary, and Mr. Ternent Cook, who has helped me in these experiments, and I, suggest this hypothesis (in the words of Dr. Johnstone Stoney, an hypothesis is "a supposition which we hope may be useful") to serve as a guide for future endeavor.

In the light of such facts, speculation on the periodic arrangement of the elements is surely premature. It is open to any one to make suggestions; they are self-evident. Most of you will agree with the saying, "It is easy to prophesy after the event." I prefer to wait until prophecy becomes easy.

I must ask your indulgence for having merely selected a few out of the many possible views as regards the Problems of Inorganic Chemistry. I can only plead in excuse that my task is not an easy one; and I venture to express the hope that some light has been thrown on the shady paths which penetrate that dark region which we term the future.

SECTION B — ORGANIC CHEMISTRY

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(*Hall* 16, *September* 21, 3 *p. m.*)

CHAIRMAN: PROFESSOR ALBERT B. PRESCOTT, University of Michigan.
SPEAKERS: PROFESSOR JULIUS STIEGLITZ, University of Chicago.
PROFESSOR WILLIAM A. NOYES, National Bureau of Standards.

THE Chairman of the Section of Organic Chemistry was Professor Albert B. Prescott, of the University of Michigan, who opened the proceedings by saying that "we are indebted to every one of the speakers so far heard in the Department of Chemistry for important studies of the element carbon, whether studies of the history, the present problems, or the co-relations of chemical science. We are under special obligations to the departmental address, yesterday, on the 'Fundamental Conceptions of Chemical Change,' by a devotee and a master of the investigation of carbon compounds. We found the keenest interest, this morning, in the utterances of authority and mature judgment upon questions touching the nature and relationship of chemical elements in general, all bearing upon the character of this element, whose unlimited synthetic powers have enlisted so large a share of the labor of the chemical world. It but exemplifies the unity of scientific truth, that all the divisions of chemical science interweave with each other, so that each is strengthened and directed by the growth of all the others. And in the addresses in Sections to-morrow, in Physical Chemistry at ten and in Physiological Chemistry at three, I confidently expect that organic chemists will find no less direct an interest bearing upon their own labors."

THE RELATIONS OF ORGANIC CHEMISTRY TO OTHER SCIENCES

BY JULIUS STIEGLITZ

[**Julius Stieglitz**, Professor of Chemistry, University of Chicago, since 1905. A.M. and Ph.D., University of Berlin, 1889; University Scholar, Clark University, 1890; Chemical Laboratory, Detroit, 1890–92; Docent in Chemistry, University of Chicago, 1892–93; Assistant, *ibid.* 1893–94; Instructor, *ibid.* 1894–97; Assistant Professor, *ibid.* 1897–1902; Associate Professor, *ibid.* 1902–05.]

THE very name of the branch of chemistry on whose relations to other sciences I have the privilege of addressing you to-day tells us with what sciences in particular, other than sister branches of chemistry itself, organic chemistry must stand in closest relationship. Since Wöhler in 1828 by the synthesis of urea showed that there is no fundamental difference between compounds prepared in the laboratory and the same compounds formed in living organisms under the influence of what until then was known as "vital force," organic chemistry has become knitted more and more closely with all branches of the great science of organic life. Its achievements in the past culminated, we may say, in Fischer's synthesis of the important hexoses and his magnificent development, with the aid of van 't Hoff and Le Bel's great theory, of the fact that there is an intimate connection between the stereochemical configurations of organic compounds and their production and assimilation in living organisms. Great as these and similar achievements have been, they can be but an earnest of what must still be done and is being done to have organic chemistry do its full duty in the study of life's development, its maintenance, its decay. The very fact that every stage of life in the animal and the vegetable kingdom, in the lowest and the highest orders, is indissolubly connected with the formation or transformation of very complex organic compounds shows us where the path of the organic chemist must lead to, difficult as the way may be. The plant physiologists, physiological chemists, physiologists, anatomists, bacteriologists have piled up questions for us at a far greater rate than we have been able to answer them.

Before this host of questions there is one to which I wish to call your attention in particular this afternoon in the time at our disposal, and to whose answer I wish to bring a small contribution based on work done with Messrs. Derby, McCrackon, and Schlesinger. The composition, structure, and configuration of the innumerable compounds connected with vital phenomena are problems of the highest importance. But the questions as to *how* and *why* such molecules are formed and transformed seem equally important, for if the transformation ceases, life also ceases, even in the presence of abundance of valuable molecules that build up organisms. Now we are all aware of the fact that there is almost no vital transformation of matter that is not regulated by so-called "catalytic" agents,—enzymes, acids, bases, salts, — in fact, as not long ago was pointed out, an organism seems an almost perfectly regulated machine for the transformation of matter, and the regulators seem to be the catalytic agents! They determine the speed of chemical changes, and, as Euler states, it seems almost certain that without the catalyzer there would be no transformation, no chemical action at all. When, a short time ago, Jacques Loeb startled the world by the artificial fertilization of eggs of sea-urchins and other marine animals by salt solutions of definite composition and concentrations of their ions, he suggested in one of his addresses that the key to his results would most likely be found in the fact that in all eggs there is a tendency to develop, but that if the development were not hastened so as to reach a certain stage within a given time-limit, death would follow without the production of the young animal. But if the development were accelerated sufficiently, a normal development of life would follow. According to Loeb, then, even this fundamental life-fact, the fertilization of eggs, involves probably to a large extent a question of an accelerated reaction, or, as we may say, a "catalytic" problem. In Loeb's experiments and hundreds of others we know what the ultimate results of the catalytic reactions are, but we are just beginning to have any experimental answers to the question why and how catalyzers exert their marvelous accelerating influences. It may be there is no general answer possible to this question which would cover all cases — we can only know after the study of a large number of individual cases. In this semi-darkness we may distinguish for the present two classes of catalytic reactions, first those produced by so-called heterogeneous or physical agents, like platinum black, and secondly those produced by what may be called homogeneous chemical catalyzers such as acids, alkalies, salts. In an endeavor to ascertain in some individual case the exact manner in which a catalytic agent acts, a reaction of the second simpler class was chosen, namely, the thoroughly studied catalysis of an ester like methyl acetate under the influence of acids and water. As we all know, the facts are the following: the saponification of methyl acetate by water according to the equation

$$CH_3CO_2CH_3 + H_2O \rightleftarrows CH_3CO_2H + CH_3OH$$

proceeds exceedingly slowly. Acids greatly accelerate the saponification proportionately to the concentration of the hydrogen ions used. It has been established by Knoblauch and Kistiakonsky that the ultimate condition of equilibrium of the reversible reaction is not sensibly altered by the catalyzer, in other words, that the acid accelerates the reaction velocity in either direction to the same extent. In the third place, the catalytic agent appears to act by its presence simply; it appears, at least, to remain unchanged throughout the course of the reaction. These three properties have been assumed by some to be necessary and typical characteristics of catalytic action. But in this investigation, in order not to overlook possibly the real answer to our problem, the vital fact of acceleration alone was considered as characteristic and it was left to the rigorous application of well-known fundamental laws of chemistry to develop why, incidentally, in this and similar cases the equilibrium is not disturbed sensibly and why the catalyzing acid appears to have no share in the reaction. From this point of view, in endeavoring to imagine just how an acid could affect the speed of the above reaction, the most fundamental fact concerning acids was recalled, the fact that they have the power to form salts with bases and oxides. Here we have the acid and the oxide, and the idea was at once suggested that methyl acetate has basic properties and that salt formation with the acid is the cause of its catalysis. It was clear, however, that the basic functions of a substance like methyl acetate must be far too weak for quantitative measurements of its constants and for a rigorous quantitative test of the idea just developed. Under these circumstances it was thought best to study all these conditions in a class of closely related bodies, the imido-esters, with which quantitative measurements of all important factors could readily be carried out. As the name implies, these are esters in which we have an imide group (: NH), replacing the oxygen atom of the ester, as in imidomethyl acetate, $CH_3C(:NH)OCH_3$. They are markedly basic substances which form well-defined salts. The free bases, for instance benzimido-esters, are very slowly decomposed by water, chiefly according to the reaction

$$C_6H_5C(:NH)OCH_3 + H_2O \rightarrow C_6H_5CONH_2 + CH_3OH \quad (1)$$

and yet more slowly according to

$$C_6H_5C(:NH)OCH_3 + H_2O \rightarrow C_6H_5CO_2CH_3 + NH_3 \quad (2)$$

Both reactions are practically non-reversible. The addition of hydrochloric acid enormously increases the velocity of the second reaction, and it becomes almost the exclusive one. Again the question arises, *how* does the acid accelerate the action. Of course the acid forms the hydrochloride, but as imido-esters are weak bases we have in the aqueous solution partial hydrolysis and a condition of equilibrium according to

$$C_6H_5C(NH_2Cl)OCH_3 + H_2O \rightleftharpoons C_6H_5C(NH_2OH)OCH_3 + HCl \quad (3)$$

The reaction presents, therefore, at least three possibilities, — the velocity may be proportionate to the concentration of the salt present at any moment, or to that of the free base, or to either indifferently, to the total substance, as expressed in:

$$\frac{dx}{dt} = k_s x \text{ (Salt)} \tag{I}$$

$$\frac{dx}{dt} = k_b x \text{ (Base)} \tag{II}$$

$$\frac{dx}{dt} = k_{su} x \text{ (Substance)} \tag{III}$$

In order to decide between these three possibilities and thus answer our question, it was necessary to determine two things experimentally, first the actual change, x in time t, and second the proportions of salt, free base, and acid present at any moment t. The latter may be determined on the basis of the well-known equation for the solution of a hydrolyzed salt, namely, according to Arrhenius:

$$\frac{\text{(Positive Ion)}}{\text{(Base) } x \text{ (H)}} = \frac{k_{\text{base}}}{k_{\text{water}}} = k_{\text{hydrolysis}}$$

The constant k was determined experimentally by conductivity measurements, and with its aid the concentrations of salt, base, and acid for the above differential equations calculated. The experimental results show unmistakably that the true course of the reaction is given by equation (I), which alone leads to a true constant.

For instance, we have among our many results for methyl imido benzoate:

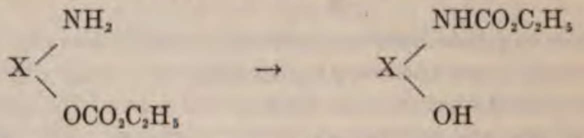
$43430\ k_{\text{salt}} = 246$; 246; 256; 238; 236; 234.
257; 239; 259; 249; 237; 242; 252; 248.
$43430\ k_{\text{subst}} = 202$; 184; 183; 172.
231; 201; 188; 175; 168; 163; 158; 138.
$100\ k_{\text{base}} = 72$; 67; 59; 51; 46; 42.
58; 49; 44; 41; 35; 33.

For the corresponding nitrobenzoate we have:

$10{,}000\ k_{\text{salt}} = 256$; 252; 246; 255; 252; 248; 263; 261; 257.
$4343\ k_{\text{subst}} = 102$; 98; 96; 93; 90; 87;
$k_{\text{base}} = 1.17$; 1.23; 1.01; 0.78; 0.69.

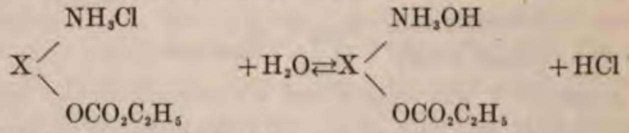
It is therefore certain that hydrochloric acid, which enormously increases the velocity of saponification of the imido-ester according to equation (II), does so simply and quantitatively through salt formation, as was expected. As the experiments were carried out in dilute solutions in which the salts are practically completely ionized, it is obvious that it is the positive ion which is decomposing in the direction given and the acceleration is exclusively due to the formation of more such active or unstable ions.

The accelerating, or let us call it the catalytic, action of the acid is here surely due then to salt or ion formation, or in other words, to the formation of a different, less stable, or more reactive molecule. Now if this is the correct explanation of the catalytic action of acids, it is clear that by this same salt or ion forming power, they should in certain cases *retard* action instead of accelerating it, provided the ion or salt is more stable than the free base. That must be an inevitable consequence of this theory, and we have brought its complete experimental confirmation in work recently published on the molecular rearrangement of certain organic bases according to



$$X\left\langle\begin{matrix}NH_2\\OCO_2C_2H_5\end{matrix}\right. \rightarrow X\left\langle\begin{matrix}NHCO_2C_2H_5\\OH\end{matrix}\right.$$

by a shifting of a carbethoxy group. The bases are exceedingly weak ones, and their salts, again, are hydrolyzed according to



$$X\left\langle\begin{matrix}NH_3Cl\\OCO_2C_2H_5\end{matrix}\right. + H_2O \rightleftharpoons X\left\langle\begin{matrix}NH_3OH\\OCO_2C_2H_5\end{matrix}\right. + HCl$$

Hydrochloric acid retards but does not prevent the rearrangement, and it was proved that it retards it quantitatively by salt formation and that the velocity of the rearrangement remains rigorously proportionate to the concentration of the free base present at any moment. For instance the velocity constant was found to be $k_{base} =$ .0566 at the beginning of the reaction, and .0567 at its end ten hours later.

Now these great changes in speed of reaction are the main characteristics of catalytic action; and we have in these cases a very simple explanation of it. It remained, however, to ascertain whether the two other important characteristics for many catalytic reactions are also in agreement with our conception of salt formation when rigorously applied — first, as explained for the catalysis of methyl acetate, the fact that the catalyzing acid need not *appear* to combine with any of the reacting substances, and second the fact that in a reversible reaction it need not measurably change the final condition of equilibrium. These points were tested by the application of our fundamental conception to the catalysis of methyl acetate. The intimate connection with the work on the imido-ethers is recognized as follows: we found above the velocity of saponification of imido-esters to be

$$\frac{dx}{dt} = \mathrm{k} \times (\mathrm{Salt})$$

But according to Walker and Arrhenius

$$(\mathrm{Salt}) = K \times (\mathrm{Base}) \times (\mathrm{H})$$

and therefore

$$\frac{dx}{dt} = k' \times (\text{Base}) \times (\text{H})$$

But this is exactly the velocity equation for the ester catalysis, (Ester) being substituted for (Base):

$$\frac{dx}{dt} = k \times (\text{Ester}) \times (\text{H})$$

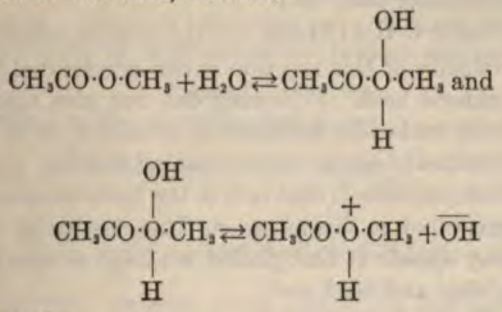
And so if the ester could form salts with acids its saponification could undoubtedly be due to the saponification only of its salt or positive ion. This important link in the chain of argument has been supplied by the discovery of Baeyer and others that the esters do form well-defined salts with acids, very unstable ones, but still salts. They are almost certainly salts of quadrivalent oxygen bases or oxonium salts. Coehn has proved that they are true electrolytes by showing that the positive ion of dimethyl pyronium hydrochloride moves to the negative pole when the solution is electrolyzed.

Now if we start from the idea that it is only the positive ion of methyl acetate which is saponified by water, we can put for the reaction:

$$CH_3CO_2CH_3 + H_2O \rightarrow CH_3CO_2H + CH_3OH$$

$$\frac{dx}{dt} = k_{\text{sap}} \times (\text{Posit. Ester Ion}) \times (H_2O)$$

as was proved experimentally for the imido-esters. For the combination of methyl acetate with water to form an oxonium base and for the ionization of this base, we have



$$CH_3CO{\cdot}O{\cdot}CH_3 + H_2O \rightleftarrows CH_3CO{\cdot}\overset{\displaystyle OH}{\underset{\displaystyle H}{O}}{\cdot}CH_3 \text{ and}$$

$$CH_3CO{\cdot}\overset{\displaystyle OH}{\underset{\displaystyle H}{O}}{\cdot}CH_3 \rightleftarrows CH_3CO{\cdot}\overset{+}{\underset{\displaystyle H}{O}}{\cdot}CH_3 + \overline{OH}$$

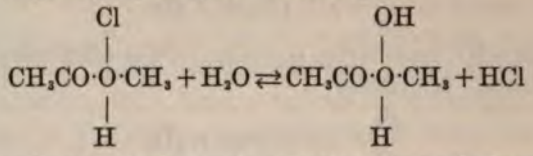
and consequently

$$(\text{Posit. Ester Ion}) = \frac{K_{\text{base}}}{K'} \times (\text{Ester}) \times (\text{H})$$

By the substitution of this value for the concentration of the positive ester ion in the above equation, we obtain for the velocity of saponification of methyl acetate by water alone

$$V_{\text{sap}} = \text{k}_{\text{sap}} \times \frac{K^{\text{base}}}{K'} \times (\text{Ester}) \times (\text{H}) \times (H_2O)$$

If we add hydrochloric acid some salt must be formed, which will be almost completely hydrolyzed according to



$$CH_3CO\cdot\overset{\overset{Cl}{|}}{\underset{\underset{H}{|}}{O}}\cdot CH_3+H_2O\rightleftarrows CH_3CO\cdot\overset{\overset{OH}{|}}{\underset{\underset{H}{|}}{O}}\cdot CH_3+HCl$$

Then according to Arrhenius's equation for hydrolyzed solutions of salts of weak bases with strong acids we have

$$(\text{Posit.Ester Ion})=\frac{K_{\text{base}}}{K'}\times(\text{Ester}-y)\times(\text{H}')$$

For an almost completely hydrolyzed salt the change in the concentration of the ester will not be perceptible, y will be entirely negligible, and we obtain then for the velocity of saponification of methyl acetate in the presence of hydrochloric acid:

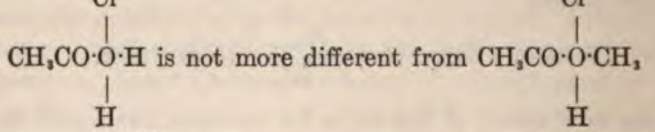
$$V_{\text{sap (HCl)}}=k_{\text{sap}}\times\frac{K_{\text{base}}}{K'}\times(\text{Ester})\times(\text{H}')\times(\text{H}_2\text{O})$$

By a comparison of the two velocity equations, for the reaction in the presence of water alone and in the presence of added acid, we find that the velocity must in fact increase directly proportionate to the concentration of the hydrogen ions, since all other factors remain unchanged. This consequence of our theory is evidently in perfect agreement with the well-known experimental results.

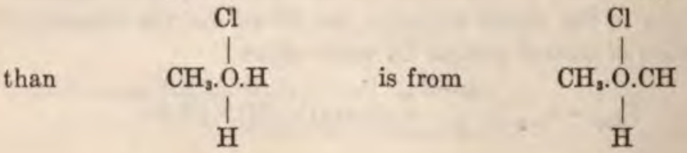
And now we come to the last important fact, namely, that the reaction is a reversible one, viz.:

$$CH_3CO\cdot O\cdot H+CH_3OH\rightarrow CH_3COOCH_3+H_2O$$

and that the velocity of this reaction is also accelerated by the addition of hydrochloric acid. Following out our idea rigorously, this increased velocity under the influence of an acid must be due to minimal basic properties of acetic acid or methyl alcohol. It could easily be shown, if time permitted, that it is to the basic properties of acetic acid that we must look in this instance. This conclusion is not so surprising as it may appear at first glance, we have so many substances that are both basic and acid, and



$$CH_3CO\cdot\overset{\overset{Cl}{|}}{\underset{\underset{H}{|}}{O}}\cdot H\text{ is not more different from }CH_3CO\cdot\overset{\overset{Cl}{|}}{\underset{\underset{H}{|}}{O}}\cdot CH_3$$



$$\text{than}\quad CH_3.\overset{\overset{Cl}{|}}{\underset{\underset{H}{|}}{O}}.H\quad\text{is from}\quad CH_3.\overset{\overset{Cl}{|}}{\underset{\underset{H}{|}}{O}}.CH$$

It may be added that Rosenheim from a comparison of compounds of acetic acid and its esters with metal chlorides arrived by a different way at the conclusion that acetic acid must form oxonium salts. Euler has also decided that acetic acid must have some basic functions. Applying again this conception and the laws of equilibrium to the study of the velocity of esterification, we find the velocity, in the absence of any acid, in the same way as was used above,

$$V_{\text{est}} = k_{\text{est}} \times (\text{Posit. Acetate Ion}) \times (\text{CH}_3\text{OH})$$

$$= k_{\text{est}} \times \frac{K'_{\text{base}}}{K''} \times (\text{Acet. Acid}) \times (\text{H}) \times (\text{CH}_3\text{OH})$$

In the presence of acid:

$$V_{\text{est(HCl)}} = k_{\text{est}} \times \frac{K'_{\text{base}}}{K''} \times (\text{Acet. Acid}) \times (\text{H}') \times (\text{CH}_3\text{OH})$$

We find again that the change in the velocity of esterification demanded by the application of this theory is simply proportionate to the change in concentration of the hydrogen ions — which agrees with experience.

When equilibrium is established between the two reversible reactions in the absence of hydrochloric acid we have

$$V_{\text{sap}} = V_{\text{est}}, \text{ or}$$

$$k_{\text{sap}} \times \frac{K_{\text{base}}}{K'} \times (\text{Ester}) \times (\text{H}_2\text{O}) \times (\text{H}) =$$

$$k_{\text{est}} \times \frac{K'_{\text{base}}}{K''} \times (\text{Acet. Acid}) \times (\text{CH}_3\text{OH}) \times (\text{H})$$

An inspection of the equations shows that we also have then:

$$V_{\text{sap(HCl)}} = V_{\text{est(HCl)}}$$

since according to the equation just given

$$k_{\text{sap}} \times \frac{K_{\text{base}}}{K'} \times (\text{Ester}) \times (\text{H}') \times (\text{H}_2\text{O})$$

must equal $k_{\text{est}} \times \frac{K'_{\text{base}}}{K''} \times (\text{Acet. Acid}) \times (\text{CH}_3\text{OH}) \times (\text{H}')$

In other words the addition of the catalyzing acid will not affect the ultimate condition of equilibrium between ester, water, acid, and alcohol.

We find thus the theory that acids may act as catalytic agents simply through salt or ion formation, as proved by the experiments with the imido-esters and the rearranging amino carbonates, leads by the rigorous application of our laws of dynamics also directly to the very facts in regard to the catalysis of methyl acetate which have long been known as vital results of experimental observation. The three important characteristic features of the catalysis — a velocity proportionate to the concentration of the hydrogen ions, the catalyzer apparently acting only by its presence and not changing the final condition of equilibrium, all are in perfect agreement with this simple conception of the manner in which the catalyzer produces its apparently marvelous result. The future must determine how many of the catalytic actions which are of such fundamental importance in the economy of organic life will be capable of explanations equally simple and quantitative in their ultimate terms, however much these terms may vary in details.

PRESENT PROBLEMS OF ORGANIC CHEMISTRY

BY WILLIAM ALBERT NOYES

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THERE is a strong tendency on the part of some chemists, at the present time, to claim that chemical science in the true sense includes only such portions of our knowledge as can be stated in accurate mathematical terms. One distinguished representative of this school of chemistry has said, "It is not in the province of science to explain phenomena," and another has written, "It is not a part of its ultimate object [*i. e.*, of natural science] to acquire knowledge in regard to mentally conceived existences, such as the atoms of matter, or the particles of luminiferous ether, which are of such a magnitude and character as to lie far beyond the limits of human conception." I think that nearly all of those now actively engaged in working over the problems of organic chemistry would dissent strongly from these statements. Long experience in dealing with the cumulative, non-mathematical evidence upon which our knowledge of chemical structure is founded has led to a very firm conviction that human knowledge is not bounded by the limits of sense-perception. We are inclined rather to the view that, while there are, undoubtedly, many things which will always remain beyond any direct cognizance of our senses, yet, so far as these have a real existence, we may in the end secure, regarding them, very practical and positive knowledge. It is impossible to conceive that those theories with regard to structure which have guided the work of thousands of chemists for the last fifty years do not in some measure express the actual truth with regard to atoms and their relation to each other in organic compounds.

Let us follow, for a few moments, in very brief outline, the steps which have led to the present standpoint. So far as the matters which interest us most are concerned, there was practically no knowledge of organic chemistry before the nineteenth century. The first steps were, of course, the preparation of pure substances and the development of accurate methods of analysis. In both of these fields Liebig was the great master. The formulæ which were calculated were, at first, of little value except to check the accuracy of the analyses and as a simple expression for empirical composition. I need not dwell on the confusion which existed throughout the first half of the century because there was no agreement as to the basis for molecular weights or atomic weights, nor upon the large part played by the study of organic compounds in finally clarifying the view of chemists upon these matters. Yet, in spite of this confusion, two discoveries of fundamental importance date from this period: (1) That the empirical composition alone does not fix the nature of a compound, *i. e.*, the fact of isomerism; (2) that certain groups of atoms may remain together in passing from one compound to another through a whole series. The first fact furnishes one of the strongest reasons why an empirical formula for an organic compound is not enough; and the second fact furnishes the most important experimental basis at the foundation of our structural formulæ.

The studies of this period furnished a knowledge of the empirical composition of many natural products and of the products obtained from these by oxidation, reduction, and the action of various agents. But while some might, perhaps, be inclined to look upon this mass of empirical knowledge as the most valuable acquisition of that time and to think that the theories in vogue were so imperfect or erroneous as to be of no value, such a view is certainly superficial. There were plenty of chemists in that day, too, who were ready to decry theories which seemed to them worthless, and it is interesting to read to-day what the great Laurent said upon this matter. He wrote in 1837:[1] "If I could believe that the purpose of my work was only to find a few new compounds or that it would end in my being able to say that there is an atom more or less in this compound or that, I would give it up on the spot. Only the desire of finding an explanation for some phenomena and of proposing some more or less general theories can give me the courage to follow a course in which I have found so little encouragement and where I have met with so many obstacles to overcome." Any one who has followed the story of how the older theories of radicals paved the way for the theory of types and of how the typical formulæ were so easily transformed into structural formulæ when the fact of valence was once grasped, cannot fail to see that the larger and fuller view is an outgrowth from the earlier theories. And we must acknowledge that Laurent was right and that the theories upon which he was working were of vastly more importance than the mass of empirical facts which furnished him with their scaffolding.

Do not misunderstand me. There were two theories of radicals at that time — one which devised radicals in the study which should accord with the electro-chemical theories held at the time and which did not attempt to secure evidence of their existence from the conduct of the compounds containing them, another which kept in much closer touch with the facts discovered in the laboratory. It was only the latter theory which contributed much to the growth of our knowledge. A theory which cannot secure for itself a sound experimental basis is, of course, of only ephemeral value.

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[1] *Ann. d. Chem.* (Liebig), 22, 143.

These, then, are the steps which have led to our present standpoint in organic chemistry: The discovery of isomerism, the discovery of radicals, the older radical theory, the theory of types, the establishment of true molecular weights, the discovery of the fact of valence, the determination of structure.

I think that all workers in organic chemistry will accept the following as a conservative statement of our present knowledge: (1) That in organic compounds, at least, each atom is attached *directly* to only a limited, small number of other atoms; (2) that in the sense of the order of the successive direct attachments the structure of a very large number of compounds is known with a degree of probability that amounts to practical certainty.

This brings me to the task which has been set, an attempt to outline the problems which lie before us in the further development of our science.

In the first place, there is still much to be done to extend our knowledge of compounds found in nature. This field is much less cultivated, relatively, than was the case sixty years ago. There has been good reason for this because of the problems of absorbing interest which have arisen in the preparation and study of new compounds and in the extension of our knowledge of old ones. But there must still remain many compounds to discover among both animal and vegetable products. On this side organic chemistry resembles the descriptive sciences of botany, zoölogy, and mineralogy. And just as botanists think it worth their while to secure as complete a description as possible of the plants to be found on the earth, so it lies in our province to isolate and identify the carbon compounds of the animal and vegetable worlds — with the difference that in our case each compound, new or old, may be the starting-point for the preparation of an almost endless number of others. But here most of us recognize that unless a compound has some further interest than that it is new it is not worth the time taken in its preparation. I am afraid, however, as we look over the pages of our journals, there is too much evidence that not every one lives up to this view. Our ever-increasing army of nascent doctors must needs have something to do, and it is so easy to make new compounds, and so difficult to find something new of larger scope and really worth the doing.

There still remains much to do in the determination of the structure of compounds which have long been known. The study of a single compound often involves an incredible amount of work. Baeyer worked with indigo for fifteen years before his labors were crowned with a successful synthesis, and twenty years more and the work of very many chemists were needed before the scientific achievement could become a commercial success.

It was nearly twenty-five years after the first structural formula was proposed for camphor before Bredt was fortunate enough to suggest the true arrangement of its atoms, and it was ten years longer, and required in all the work of more than fifty chemists, before Bredt's suggestion was confirmed by Komppa's beautiful synthesis.

More than thirty formulæ were proposed for camphor, and those who think little of organic chemistry have some reason if they say that we jump at conclusions too hastily and propose too many formulæ that are mere guesses. Some might even say that the last formula is not worth much, but those who have followed the matter know that step by step we have arrived at an almost positive certainty even in this complex problem.

The final solution of a problem with regard to the structure of a compound of natural origin is not usually considered to have been satisfactorily attained until its synthesis has been effected. Those who have attempted work of this character know that months or even years of work are frequently spent to obtain the synthesis of a single compound. In spite of the wealth of methods at our command, — a wealth so great that it is often very difficult to select between several which are equally unpromising, — it is evident that these methods of synthesis need improvement at many points. Not only do we need new and better methods, but many old methods require further study to disclose why they succeed in some cases and fail in others, and to secure a fuller knowledge of secondary reactions which often occur. As recent remarkable achievements in this field of synthetic methods may be mentioned the brilliant results obtained by Grignard with magnesium compounds, Bouveault's elegant new solution of the old problem of transforming an acid into the corresponding alcohol, and Scheuble's reduction of the amides of bibasic acids to the corresponding glycols.

Work along the lines suggested needs to be done in order to fill out and complete our knowledge in a systematic way, and occasionally work along such lines is rewarded by results of epoch-making significance, as when Gomberg discovered triphenylmethyl in his endeavor to prepare hexaphenylethane. Such work is not likely, however, to greatly advance our insight into the real nature of carbon compounds, and we all feel that there are far more fundamental problems which demand attention.

As outlined above, the theories of valence and of structure now universally accepted imply a certain amount of knowledge of the arrangement of atoms in space. So far as the original and fundamental conceptions are concerned, however, this knowledge is quite vague. The much more definite conception proposed by van 't Hoff, and in a somewhat different manner by Le Bel, is, of course, familiar to you all. In discussing any hypothesis it is always important to have clearly before us the facts upon which it is based. As I have already hinted, I believe that the theory of valence and the theory of structure in the sense of a sequence of atoms within the molecule are supported by our knowledge of such a vast accumulation of consistently interrelated phenomena that we are justified in believing that we have positive knowledge with regard to the structure of the molecules of organic compounds. I am as ready as any one to demand that every theory, no matter how old or how universally accepted, shall be continually brought back to the test of agreement with experimental facts, but I am not willing to admit that we may not, in the end, acquire positive knowledge by the process of inductive reasoning.

Assuming, then, the fact of a knowledge of the sequence of atoms in organic compounds, we have this basis for van 't Hoff's hypothesis: (1) When four unlike atoms or groups are combined with a single carbon atom, optical activity results in such a manner that there may always be found two compounds having identical sequence of the atoms within the molecule, and exactly equal rotary power, but of opposite signs. (2) That when two adjacent carbon atoms are combined each with three unlike groups, two compounds may result which, while optically inactive and having the same sequence of atoms, still differ in physical properties. An illustration of this is found in racemic and mesotartaric acids. (3) Rings containing five and six atoms are formed with especial ease, those containing three, four, and seven atoms less readily, and rings containing more than seven atoms are scarcely known. (4) Derivatives of cyclopropane, cyclobutane, cyclopentane, and cyclohexane having two substituents combined with different carbon atoms often exist in two isomeric forms in which the sequence of the atoms is the same. (5) Derivatives of ethylene often exhibit a similar isomerism.

Assuming as true that we have acquired a knowledge of the sequence of atoms in carbon compounds, the facts which I have enumerated lead almost inevitably to the corollary that the four atoms attached to a given carbon atom are arranged in approximate symmetry around the centre of that atom for their position of most stable equilibrium. The relation between this conclusion and the theory of the sequence of atoms in carbon compounds, or what is ordinarily understood as structure, is very similar to the relation between the atomic theory and Avogadro's law. If we accept the atomic theory, there seems to be no rational escape from the acceptance of Avogadro's law. In a similar manner, if we accept the theory of the sequence of atoms in carbon compounds, there seems no reasonable possibility other than that van't Hoff's hypothesis is true in its broad outlines.

I hope I may be pardoned here for a brief digression. I am aware that Franz Wald [1] believes that he can give a satisfactory explanation of the laws of fixed and multiple proportion and of combining weights without the aid of the atomic theory, and that Professor Ostwald in his recent Faraday lecture [2] has accepted and expanded the same thought. I will say frankly that their reasoning does not appear to me conclusive. Ostwald defines a chemical individual as "a body which can form hylotropic phases within a finite range of temperature and pressure," [3] and deduces from this the fact that a given hylotropic phase must have a fixed composition. He appears to forget that the existence of these hylotropic phases implies that the properties of matter are discontinuous, or, in other words, that there is a finite number of hylotropic bodies, one of the facts for which the atomic theory gives an explanation.

There is another characteristic, too, of a chemical compound which all chemists will agree is at least as important as that it shall consist of a "hylotropic phase." This is that the compound must not only have a fixed composition, but this composition must bear a definite relation to those numerical quantities which represent the proportion in which each element of which it is composed always combines with other elements. I need hardly add that these numerical quantities are so deeply seated in the properties of matter that, having adopted a unit, all chemists are absolutely agreed in selecting one and only one such quantity for each of the well-known elements.

In attempting to deduce this law of combining weights Ostwald assumes that three elements form the compounds AB, AC, BC, and ABC, and adds, "There shall be but one compound of every [each] kind." With this assumption, his reasoning may be sound, but I fail to see how it applies when we find ten thousand compounds ABC instead of one. The case which he supposes is so far theoretical that I have been unable to find an actual case where the compound ABC can be formed, by the union both of AB with C and of AC with B.[4] But I have taken too much time with a matter which is aside from my main purpose. Before leaving this topic I must add, however, that I have used the phrase "Avogadro's law" advisedly, in spite of the fashion set by some chemists of calling it Avogadro's hypothesis.[1]

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[1] *Ztschr. Phys. Chem.*, 24, 633, 1897.

[2] *J. Chem. Soc.* (London), 35, 506.

[3] *Ibid.*, p. 515.

[4] It is quite possible that such an illustration may be found, but, in any case, Professor Ostwald's deduction cannot be made to apply to those cases in which the compound ABC does not exist, nor to those cases where the compound ABC cannot, even theoretically, be supposed to consist in turn of a known compound AB combined with C and of another known compound AC combined with B. Such cases are common because of the fact of valence. In its simplest form the law of combining weights is quite independent of the existence of the compound ABC and may be stated thus: If the composition of two compounds AB and BC

I remarked, a few moments ago, that the facts which have been outlined almost compel us to the acceptance of van't Hoff's hypothesis in some form. It is of the utmost importance for us to recognize, however, that we are here at the very confines of our present knowledge, and that we must, at every step, bring ourselves back to the rigorous test of experimental fact. In accepting the hypothesis we are not compelled to consider molecules as set pieces of mechanism; on the contrary, there is strong reason for thinking that the positions assumed by the atoms are positions of dynamic and not of static equilibrium. While there have been many speculations in the matter, we have no strong reason for assuming, as yet, any definite shape for the carbon atom, nor even that there are within it definite points of attraction for other atoms. All that seems to be thoroughly established is that for their position of most stable equilibrium the four atoms or groups attached to a given carbon atom are arranged in approximate symmetry around its centre. I say *approximate* symmetry because the existence of compounds containing rings of three and four carbon atoms demonstrates that the symmetry is not always absolute, and makes it probable that in cases where the four atoms or groups are unlike the symmetry is also imperfect. So far as I am aware, no fact inconsistent with this fundamental conception is known, while very many facts about optically active and cyclic compounds find in this conception the only satisfactory explanation which has thus far been given. It is true, also, that many facts with regard to optically active compounds indicate that when one group is exchanged for another the exact configuration is often retained, or, in other words, the entering group takes the same position with

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has been determined, the composition of a series of compounds between A and *C* can be predicted and a compound which does not belong to this series has never been discovered. A still more general statement of the law, and one which includes, by implication, all of those facts which are used in the selection of atomic weights, is given above. In that form it is more properly called the law of atomic weights.

[1] Two reasons may be given for this usage. My own view is that we have, by a process of inductive reasoning, acquired such positive knowledge of the existence of atoms and molecules that the expression "Avogadro's law" is fully justified. But even if we admit the contention of those who think that the atomic theory must always remain an unproved hypothesis, it is possible to frame a definition of the word molecule which would be merely a generalized statement of those empirical facts which lie at the basis of our atomic and molecular theories. Such a generalized, empirical definition must, of course, be very complex, but it would not include the concept of discrete particles. Yet it will be still true of these empirically defined molecules that equal volumes of gases contain equal numbers under the same conditions of temperature and pressure. For instance, the term gram-molecule may be considered as a purely empirical generalization, and it is true that a gram-molecule of one gas occupies the same volume as a gram-molecule of any other. But this is, in essence, Avogadro's law.

regard to the other three atoms or groups as was held by the group which was displaced. The manner in which it has been possible to work out, consistently, the complex relations between a considerable number of sugars, gives a very strong experimental basis for this statement. On the other hand, it is well known that such reactions often give racemic mixtures, which indicates that a shifting of groups with regard to a central carbon atom takes place much more easily than the shifting of a group from one carbon atom to another, at least in saturated compounds. There are also a number of extremely interesting cases where a reaction gives rise to the optical antipode. Thus Walden has shown[1] that l-chlorsuccinnic acid is converted by silver oxide into l-malic acid, while potassium hydroxide converts it into the dextro-rotatory acid. It is evident that in one case or the other there has been a shifting of the groups. Again Ascham [2] has shown that when d-camphoric acid is heated with hydrochloric and acetic acids it may be about half converted into l-isocamphoric acid, and that the latter suffers a similar transformation. This case is more complicated, as a "cis" and "trans" isomerism of cyclic compounds is involved as well as the optical difference. Not many cases of this character are known, at present, but such cases certainly deserve further study and must be reckoned with in considering the question we have before us. Le Bel [3] has already pointed out the theoretical significance of Walden's work.

While we may feel that we have comparatively sure ground in the application of the theory of van 't Hoff and Le Bel to optically active and to cyclic compounds, the case is quite different when we come to the consideration of what are commonly known as "double" and "triple" unions. Professor Michael has done a very great service to chemistry in showing that the supposition of a more or less definite tetrahedral shape for the carbon atom and of "favored" configurations often leads to conclusions which are at variance with the facts. Philips [4] and Blanchard [5] and myself have found a case in which the addition of hydrobromic acid to an unsaturated compound produces an optically active body which evidently has the same configuration as the amino and hydroxy acids from which the unsaturated body is formed by the loss of ammonia or of water. We have here, apparently, a potential asymmetry occasioned by the double union which it is difficult to reconcile with the prevailing conception of such unions. This case is complicated by the presence of a second asymmetric carbon atom in the molecule and is worthy of further study. Rabe and

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[1] *Ber. d. Chem. Ges.* 32, 1855 (1899).
[2] *Ibid.* 27, 2004.
[3] *J. Chim. Phys.* 2, 344 (1904).
[4] *Am. Chem. J.* 24, 428.
[5] *Ibid.* 26, 281; 27, 428.

Billmann [1] have recently described a similar case, but very few instances of this kind are known.

Pfeiffer [2] has recently suggested a new interpretation of van 't Hoff's hypothesis as applied to unsaturated compounds. Pfeiffer assumes that unsaturated compounds retain essentially the same configuration as the saturated compounds, from which they are derived. On this side his interpretation is closely related to the old theory of free valences, which, if I understand him correctly, is favored by Professor Michael. Pfeiffer also brings his interpretation into a close relationship to Werner's theory of inorganic metallic compounds. The most serious objection to the theory is that it supposes the existence either of trivalent carbon atoms or of free valences, in ethylene and its derivatives, an objection which has appeared to most chemists very strong in the past. Pfeiffer points out, it is true, that since the discovery of triphenylmethyl we can no longer deny the possible existence of a trivalent carbon atom.[3] It would seem, however, that the great difference between the intense chemical activity of triphenylmethyl and the comparative inactivity of ethylene demonstrates that, if the latter does in reality have free valences, the fact that there are two such valences reduces the activity of each enormously. The inactivity of carbon monoxide may be significant in this connection.

A more serious objection to Pfeiffer's hypothesis lies in the fact that he supposes so slight a difference in the configuration of fumaric and of racemic acids that it is difficult to see why the former as well as the latter might not be split into a pair of optically active bodies.

We must admit, then, that we have, at present, no satisfactory theory of double and triple unions, and that we have here a problem which demands a large amount of further work before it is solved. When the solution is reached we shall probably gain a new insight into the perennial question of the structure of benzene, and our knowledge of tautomerism will cease to be, as it is at present, almost purely empirical. It is possible, perhaps probable, that Thiele's "conjugated double unions" will contribute toward the solution.

While I have no comprehensive theory with regard to double unions to advance, I will, with a good deal of hesitation, venture to express some thoughts with regard to the combination of atoms in general which have some bearing on this question. We are all familiar with Faraday's law, that, if a current of electricity is passed through a number of cells filled with solutions of different electrolytes and arranged in series, exactly equivalent amounts of the various components will be liberated at the electrodes in the successive cells. The beautiful experiments of Professor T. W. Richards have demonstrated that we are dealing here with a law which is true for different solvents and over a wide range of temperature; and also that the law is true with a degree of absolute accuracy which is of the same order as the laws of the combination of elements by weight. We are compelled, then, to believe that there is associated with each valence of an ion as it is transported through a solution, or at least as it separates at an electrode, a quantity of electricity which is invariable and independent of the nature of the ion. In other words, we have here a natural electrical unit which can be defined in its relation to atomic weights with a degree of accuracy which seems to be limited only by the refinement of our manipulations.

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[1] *Ann. d. Chem.* (Liebig), 332, 25.

[2] *Ztschr. Phys. Chem.* 43, 40.

[3] The fact that triphenylmethyl exists as a doubled molecule in solution should not, I think, lead us to discard the monomolecular formula for it any more than we consider that acetic acid has, in the ordinary sense of structure, a doubled molecule because it exists as a doubled molecule in solution in benzene or in the state of vapor just above its boiling-point, nor because it forms acid salts. In these cases the chemical evidence appears to be more important and more conclusive than the physical. It is probable that the doubled physical molecule is the result of forces which do not produce a stable structure in the ordinary sense.

It is not always recognized as clearly as it should be that this unit quantity of electricity which is associated with one valence of any ion is not a unit of electrical energy. If it were, the same energy would be required to decompose the equivalent quantity of one electrolyte as of every other, which is manifestly not true. While the same current causes the separation of equivalent quantities in the different cells, the differences of potential, and so the amounts of energy required for the separation, vary greatly. It is evident then that when we say that a unit quantity of electricity is associated with each valence of every ion, we do not use the term *quantity* in the sense of quantity of electrical energy. Instead of this, when this conception of a unit quantity of electricity is examined, it will be seen that it is a conception of something whose properties are those of matter rather than those of energy. The facts appear to be consistent with the idea that the unit quantity of electricity of which we are speaking is of a material nature, and you have doubtless already perceived that I have the theory of electrons in mind. The ingenious experiments of J. J. Thomson have given us considerable reason for thinking that the negative electrons are capable of an independent existence and have also given a probable estimate of their mass, which is small in comparison with the mass of the hydrogen atom.

It has been customary to think of the unit charge of electricity as being involved only in those reactions which occur in solution. If, however, we accept the theory of electrons, it is evident that the electrons must be present in the molecule of an electrolyte, no matter in what manner it is formed. It is but a step further to the conclusion that the electrons are involved in every combination or separation of atoms, and, indeed, may be the chief factor in chemical combination.

Professor Kahlenberg[1] has shown that a practically instantaneous reaction takes place between hydrochloric acid and copper oleate in a solution in dry benzene, although the solution does not conduct an electric current and there is no evidence of the dissociation of either the copper oleate or of the hydrochloric acid. Professor Kahlenberg points out very justly that there is no apparent difference between these reactions and those which take place in aqueous solutions, where we have much independent evidence of the existence of ions. He draws the conclusion that no ions exist in either case. It would seem that we are equally justified in supposing that a substance not already in the form of ions may separate into them under the influence of a second substance with which it can react.

Some time ago Mr. Lyon and myself[2] showed that the primary reaction between chlorine and ammonia gives nitrogen tri-chloride, nitrogen, and hydrochloric acid, and that these products are formed in such proportion as to lead to the conclusion that three molecules of ammonia react simultaneously with six molecules of chlorine. It was pointed out at the time that the simplest explanation of this result is to be found in supposing that chlorine atoms separate during the reaction into positive and negative ions, while the ammonia separates partly into positive nitrogen and negative hydrogen and partly into negative nitrogen and positive hydrogen.[3] This hypothesis has met with some approval,[4] but has also received the criticism that such a dissociation as is supposed would result in the spontaneous decomposition of ammonia into nitrogen and hydrogen.[5]

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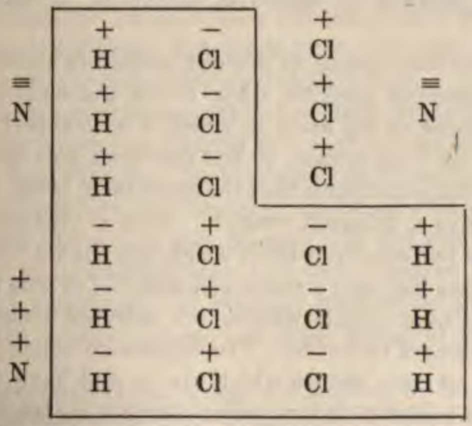
[1] *J. Phys. Chem.* 6, 1.

[2] *J. Am. Chem. Soc.* 23, 460.

[3] This was represented graphically thus:



[4] Stieglitz, *J. Chem. Soc.* 23, 707.

[5] *Ztschr. Phys. Chem.* 41, 378.

This criticism loses its force if we suppose that the separation into ions takes place only under the immediate influence of the chlorine with which the ammonia reacts. It has been pointed out by many different authors [1] that a separation of atoms from each other must occur either before or at the same time that they enter into combination with other atoms. The only part essentially new in the hypothesis proposed is that this separation is into positive and negative parts and that the same atom may be sometimes positive and sometimes negative. The idea of a dissociation which occurs under the influence of a reacting substance appears to be implied in a part of Professor Nef's discussion of methylene dissociation, but it is not always clear whether he has in mind chiefly a dissociation of this sort or one which is independent of the interaction of different compounds.

The thought that the same atom may be at one time positive and at another time negative is related to the older electrochemical theory which supposed water to be positive in acids and negative in bases.

We assume, then, that in every combination of atoms each union involves an attraction between the positive and negative electrons which are associated with the two atoms that unite. In saying this I do not lose sight of the fact that such a thing as attraction *per se*, in the sense that one body can influence another at a distance without an intervening medium, is apparently inconceivable. I think of the attraction as probably caused by some motion of the electrons which enables them to act on each other through the aid of the ether. It is convenient, however, to speak of this effect as an attraction, since our conception of its real nature is, of necessity, very vague. One advantage of the idea that the attraction of the electrons is of a kinetic nature is that we may conceive of the same electron as becoming positive or negative, according to the nature of its motion.

The common conception, at present, is that an atom which has lost an electron becomes positive, while either the electron in its independent existence or the atom to which it is attached becomes negative. So far as I am aware, it has not been pointed out that this view leads to the conclusion that the same atom must, under different conditions, have a different weight. Thus a bivalent copper atom which has lost two electrons must weigh less than a univalent copper atom, which has lost only a single electron. It is true that our methods of determining atomic weights are scarcely accurate enough to detect differences of this order. The suggestion which is made is that the electrons of two atoms which are united have motions which correspond to positive and negative charges, respectively, and that when the atoms separate these motions may be retained, or lost as

[1] See Erlenmeyer, Jr., *Ann. Chem.* (Liebig), 316, 50.

in the case of a mercury atom which is uncombined, or that the motions may be reversed. In accordance with the hypothesis outlined above, we must assume that when two atoms separate either one may become positive; dependent partly on their nature, partly on the nature of the reacting substance. The conception here proposed is that of something very similar to the action of the pole of a magnet, which may attract another pole of the opposite kind, or induce the formation of a pole of the opposite kind, or it may reverse the polarity of another magnet.[1] This is, perhaps, simpler than to suppose the transfer of an electron from one atom to another in those cases where the electrical charges of the atoms are reversed in the ionization. A very accurate determination of the atomic weight of cupric copper as compared with that of cuprous copper might possibly decide between the two hypotheses.

It should be noted that the hypothesis that the electrical charges associated with the atoms are of a kinetic nature, and that these charges may be transferred without gain or loss of matter, is quite independent of the first hypothesis, which is that the atoms are ionized when they separate from each other and that the same atom may become either positive or negative.

In following farther the thought of the attraction between electrons as the cause of chemical combination, we must suppose that in addition to the effect of this attraction in holding together the atoms which are immediately attached, there is a residual effect upon other atoms within the molecule. This gives a rational explanation of the very great difference in the stability of the union between carbon atoms in different compounds as, for instance, the instability of acetic acid in comparison with butyric acid, occasioned by the substitution of an oxygen atom for two hydrogen atoms of the latter. The study of organic compounds has given us a knowledge of a large number of cases of this sort, and our text-books contain many empirical rules about them; but there have been few, if any, attempts to give for such facts any rational explanation.

In considering double unions three explanations suggest themselves: (1) We may suppose with Pfeiffer that such unions are in reality single unions and free valences. In this case the presence in adjacent carbon atoms of positive and negative electrons which are uncombined would reduce the attraction of each for the electrons of another molecule, thus explaining why two free valences are so much less active than a single one. (2) We may suppose that the carbon atoms are in reality doubly united, but that, owing to the localization of the electrons in definite parts of the carbon atoms, the four electrons involved cannot approach as near to each other as is the case in a single union. This is Baeyer's theory of strain, and is much better in accord than is the theory of free valences with the fact that cyclopropane and propylene appear to be about equally unsaturated, as evidenced by their heats of combustion and by their conduct toward bromine. On the other hand, it seems to lead logically to conclusions with regard to the addition of bromine to triple unions, which Professor Michael has shown are contrary to the facts. (3) Without a condition of strain, we may suppose that the presence of both a positive and a negative electron in each of the atoms united by the double union causes a lessening of the attraction of the electrons. This would result in such a union being less stable than a single union. The second and third views appear, at present, most in accord with the facts — possibly the truth lies in some combination of the two.

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[1] This is, of course, only an analogy and must not be pressed too far; just as the electrical charges of atoms or ions conduct themselves very differently from those of masses. The latter divide themselves between two bodies in contact; the former may be transferred *completely* from one ion to another.

Whatever view we may take, it is noteworthy that double unions are usually formed by the loss of a positive and negative atom or group from adjacent carbon atoms, as hydrogen and hydroxyl or hydrogen and bromine. It is also true that in many double unions one of the carbon atoms is more positive than the other, causing the addition of halogen acids in a definite manner which may be predicted in accordance with Michael's "positive negative law." Applying this thought to conjugated double unions, we see that of the four atoms involved the two central ones are likely to be positive and negative respectively and neutralize each other's attraction for outside atoms, while an intensified attraction for outside atoms would be found in the exterior atoms. The effect may be analogous to that of the attractive forces of a magnet which exhibit themselves chiefly at the ends.

But I have permitted myself to wander much farther in the field of speculation than was my first intention — farther than is at all profitable, I fear, for these questions furnish, at present, few points for experimental study, and speculations divorced from experiment have usually been profitless. I should be very sorry if what has been said should give encouragement to such speculations. On the other hand, I have a very firm conviction that we should not be content with rounding out organic chemistry as a descriptive science, nor even with adding to the number of empirical rules which enable us to predict certain classes of phenomena. We must, instead, place before ourselves the much higher ideal of gaining a clear insight into the nature of atoms and molecules and of the forces or motions which are the real reason for the phenomena which we study. When we consider the progress which has been made and the knowledge of structure we now possess, which would have appeared sixty years ago to lie beyond the limits of possible acquirement, it is not presumptuous to think that a more complete knowledge of these questions will at some time be gained. This fuller knowledge will take account, too, of many lines of work upon which I have no time to dwell, such as the question of changing atomic volume to which Professors Richards and Traube have directed our attention, and the knowledge of heats of combustion, of molecular refraction and dispersion, of color, viscosity, dielectric constants, and other physical properties. The future must give to us a new theory, or a development of old ones, which shall include all of these phenomena in one comprehensive view.

SHORT PAPER

Professor Oswald Schreiner, of the United States Department of Agriculture, read a short paper on "A Study of the Sesquiterpene Class of Hydrocarbons."

SECTION C — PHYSICAL CHEMISTRY

SECTION C—PHYSICAL CHEMISTRY

(*Hall* 16, *September* 22, 10 *a. m.*)

CHAIRMAN: PROFESSOR WILDER D. BANCROFT, Cornell University.
SPEAKERS: PROFESSOR J. H. VAN 'T HOFF, University of Berlin.
PROFESSOR ARTHUR A. NOYES, Massachusetts Institute of Technology.
SECRETARY: MR. W. R. WHITNEY, Schenectady, N. Y.

THE Chairman of the Section of Physical Chemistry was Professor Wilder D. Bancroft, of Cornell University, who opened the work of the Section with the following remarks:

"Twenty years ago physical chemistry was not recognized as a subdivision of chemistry. To-day nearly every larger university in this country has a chair of physical chemistry, and we have our regular place on the Programme along with inorganic and organic chemistry. In fact, Professor Clarke in his address rather implied that physical chemistry now dominates the whole of chemistry. Certain it is that physical chemists are much in demand at this Congress and that to hear them all you must go to many Sections. Van 't Hoff is to speak to you here this morning, Arrhenius delivers an address the next hour before the Section of Geophysics, Ostwald is to speak this afternoon as a philosopher, while Sir William Ramsay was one of the chief speakers yesterday before the Section of Inorganic Chemistry.

"In addition to the two longer addresses, our Programme includes shorter papers on the chemical affinity between solvent and solute, on the chemistry of liquid ammonia, on transference in acetic acid solutions, and on the application of physical chemistry to agriculture. The first address is on the history of physical chemistry by the man who made that history possible, Professor van 't Hoff of Berlin."

THE RELATIONS OF PHYSICAL CHEMISTRY TO PHYSICS AND CHEMISTRY

BY JACOBUS HENRICUS VAN 'T HOFF

[**Jacobus Henricus van 't Hoff**, Member of the Academy of Sciences, Berlin; Ordinary Honorary Professor, University of Berlin, Germany. **b.** August 30, 1852, Rotterdam. Ph.D. Polytechnic School, Delft; M.D. University of Utrecht; LL.D. University of Chicago; *ibid.* Harvard University. Honorary course, Griefswald and Utrecht. Tutor in Clinics, Veterinary School, Utrecht, 1876–78; Professor in Chemistry, Mineralogy and Geology, University of Amsterdam, 1878–96. Member of various societies in Amsterdam, Bologna, Christiania, Delft, Erlangen, Frankfurt, Göttingen, Batavia, Copenhagen, Lund, Mexico, New York, Philadelphia, Rotterdam, St. Petersburg, Turin, Utrecht, Vienna, Venice, Washington.]

According to the Programme, I have to consider the "General Principles and Fundamental Conceptions which connect Physical Chemistry with the Related Sciences, reviewing in this way the development of the science in question itself."

Let me begin by defining physical chemistry as the science devoted to the introduction of physical knowledge into chemistry, with the aim of being useful to the latter. On this basis I can limit my task to the relations of physical chemistry to the two sciences it unites, chemistry and physics.

But even if I limit myself to these relations, which are not the only two,[1] I wish to restrict myself yet more, in order, in the spirit of this Congress, to call your attention to broad views. So I shall follow up only two lines, in answering two questions regarding two fundamental problems in chemistry: (1) What has physical chemistry done for our ideas concerning matter? (2) What has it done for our ideas concerning affinity?

The small table which I have the honor to put before you will enable us to answer these questions by appeal to the scientific development of our science, which also I have to review:

I. *Ideas concerning Matter*

(1) Lavoisier, Dalton (1808).
(2) Gay-Lussac, Avogadro (1811).
(3) Dulong, Petit, Mitscherlich (1820).
(4) Faraday (1832).
(5) Bunsen, Kirchhoff (1861).
(6) Periodic System (1869).

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[1] In Chicago I devoted to this subject eight lectures, which have since appeared in the Decennial Publications under the title 'Physical Chemistry in the Service of the Sciences,' Chicago, 1903.

(7) Pasteur (1853), Stereochemistry (1874).
(8) Raoult, Arrhenius (1886–87).
(9) Radioactivity (Becquerel, Curies).

II. *Ideas concerning Affinity*

(1) Berthollet, Guldberg, Waage (1867).
(2) Berzelius, Helmholtz (1887).
(3) Mitscherlich, Spring (1904).
(4) Deville, Debray, Berthelot.
(5) Thomson, Berthelot (1865).
(6) Horstmann, Gibbs, Helmholtz.

I. *Physical Chemistry and our Ideas concerning Matter*

The Concepts of Atoms and Molecules. Regarded as a whole, we may say that the initial application of physical knowledge for the purpose of developing our ideas of matter consisted chiefly in the employment of physical methods and instruments in the study of the properties of matter. This stood foremost in physical chemistry in the first period of its existence.

Reviewing the history of chemistry, we must acknowledge that one of the first fundamental steps was made by the study of the physical property of weight, and the introduction of a physical instrument, the balance, for this purpose. It was, in large part, on this basis that Lavoisier was the great innovator of chemistry; and it was due solely to the following of chemical change with the balance that chemistry got its fundamental laws of constant weight and of constant and multiple proportions. These were summarized by Dalton in the fruitful though hypothetical conception of atoms, which, as is well known to you all, asserts that every element exists in the form of small unchangeable particles, identical for a given element, but differing with the latter.

As the study of weight led to the idea of atoms, so the study of another physical property, that of volume and density, led to our idea of molecules. These molecules, which might be described as constellations of atoms, were a necessity with Dalton's conception; but, in a binary compound, for instance, they might consist of two atoms or of twenty. Now, it hardly needs to be recalled that Gay-Lussac, and especially Avogadro, in following the volume relations of gases in chemical action, drew the conclusion that the molecules of gases occupy equal volumes under identical conditions. Thenceforward we had a reliable method for determining the relative weights of such molecules.

As the study of the physical properties weight and volume led to the concepts of atoms and molecules, so sharply defined that the relative weights of these entities form the fundamental constants of chemistry, so a further study of physical properties has led to broad generalizations concerning the nature of atoms and molecules, which we shall now outline.

Properties of Atoms. As to atoms, I would call your attention to four peculiarities which seem to me of fundamental importance. First, Dulong and Petit found that the physical property called heat capacity is nearly the same for different atoms, *i. e.*, that the quantity of heat requisite to produce a given rise of temperature does not vary greatly for atomic quantities, for 7 parts of lithium and for 240 parts of uranium.

Second, Faraday, in studying the electrical conductivity of electrolytes, *e. g.*, of aqueous solutions of salts, found that the quantity of electricity which atoms can transport varies as the whole numbers, — from one in potassium to two in zinc. This fundamental property, which gives the sharpest expression to our notion of valency, was brought by Helmholtz into a very clear form by the assumption that electricity as well as matter consists of atoms, either negative or positive, and that material atoms are able to combine with them, — potassium with one of the positive kind, zinc with two, chlorine with a negative one, — and so transport them in electrolysis.

The third great step was made by the study of light, a physical property again. Bunsen and Kirchhoff found that, heated in the gaseous state, every atom emits a definite set of light-waves, producing a characteristic line-spectrum which is yet the sharpest test of the kind of atoms one is dealing with, and which so became the most fruitful guide in the detection of new kinds.

The last generalization that I have to mention, and which we owe to Newlands, Mendeléeff, and Lothar Meyer, includes physical properties in general, and asserts that they vary with increasing atomic weight in a periodic way. This shows itself most sharply in the atomic volume, which passes through maximum values in lithium (7), sodium (23), potassium (39), rubidium (85), and cæsium (133). A corresponding periodicity is observed in other properties, as, for example, that of combining with electrical atoms, or valency, which in the said elements passes through unity. Analogous behavior is exhibited by the melting-points and boiling-points, which for these metals are exceptionally low.

If my programme did not to a certain extent exclude quite recent investigations, confining me to a view of past history, I should like to consider one more physical property, that of radioactivity, which also seems to be a property of atoms. I can only insist on the fact that it was physical properties again, the making the air conductive for electricity, and the spectrum, which revealed radium.

Properties of Molecules. Turning to molecules, I have three predominant generalizations to outline. The first is Mitscherlich's discovery of the fact that analogous molecular constitution corresponds to analogous outer crystalline form, to so-called isomorphism. Let me add that there is hardly any more satisfactory proof of the soundness of our concept of the internal structure of matter than, *e. g.*, the identity of the crystalline forms of the alums, which we consider to have corresponding internal structure.

A second step, to a certain extent a similar one, was made by Pasteur when he deduced disymmetry of molecular constitution from disymmetry in behavior, optically as well as crystallographically. For instance, the dextrorotatory ordinary tartaric acid and its lævorotatory antipode showed this disymmetry both in optical rotation and in the particular so-called enantiomorphous crystalline form. The molecules were supposed to have analogous structures differing from each other as the right hand from the left. As is well known, it was only later that the probable molecular structure was sharply defined, and stereochemistry was founded.

The third great step was the opening of a way to determine the molecular weights of dissolved substances. It was chiefly the application of Avogadro's law to osmotic pressures, in connection with Raoult's measurements of freezing-points and vapor-pressures, that opened the way. We may now assert that the liquid state is not characterized by high molecular complexity. But the great innovation, introduced by Arrhenius and immediately brought into relation with the achievement in question, was the admission of the existence of ions in electrolytes — for example, the presence of negatively charged chlorine atoms and positively charged sodium atoms in an ordinary salt solution. Once more it was a physical property, the electrical conductivity, that led to this extremely fruitful supposition.

Conclusion. If, after this short summary of its properties, we try to look into the nature of matter, we conclude that matter is not continuous, but that there are centres of action which seem to have an eternal existence, changing only in the place that they occupy — these are the atoms. They keep together in some way and form the molecule; how, it is pretty hard to say. The planetary constellation, with ordinary attraction and centrifugal force in equilibrium, is excluded by the consideration that at the absolute zero there is no movement at all. The repulsive force that we want might be of electrical nature; and so we come to our combination of material and electrical atoms. There is indeed something fascinating here, and when we admit for carbon that it may unite to four equally charged electrical atoms and hold them by a force of the nature of elasticity, we have at once a possible equilibrium and the tetrahedral grouping. My only difficulty is that an uncharged atom of carbon, coming into contact with the ions just described, would take away half the electric charge, and so the valency of any element might be reduced to uni The latest supposition, that matter is built up of electricity alone, l again beyond the scope of this address.

Let me now turn to the second part of my subject, and touch up the problem of affinity; indeed, the action that keeps atoms togetl must be closely related to affinity.

II. *Physical Chemistry and our Notions concerning Affinity*

While physical chemistry, in the first period of its developme was chiefly devoted to the study of the physical properties of matt the second and present period is characterized by the predomina place of the problem of affinity.

This change in the general aspect of our science goes hand in ha with a different way of working: in the development of our ideas matter, physical chemistry introduced physical methods and instr ments for the study of physical properties; in the development of o ideas of affinity, physical chemistry has introduced physical pri ciples.

Affinity considered as Force. The first line of thought consider affinity as a force, and in this direction it was natural to think of t Newtonian attraction as the chemical agent. So it was that Bertholl and with far more success Guldberg and Waage, applied the laws mass-action to problems of affinity, formulating a relation still know as the mass-law, according to which affinity is proportional to t weight in the unit of volume.

Now, as we all know, affinity is of a specific nature, and does n depend on weight merely; on the contrary, the least heavy elemen are generally the most active. So Berzelius built up his syste founded on the notion that elements have a specific electrical chara ter, either positive or negative, and, in combining, act by electric attraction. In this direction Helmholtz made a further step in taki into account the quantitative side. Considering the electrical charg involved in Faraday's law, he pointed out as very important that t attraction due, for instance, to the negative charge in chlorine a the positive one in hydrogen far exceeds the gravitational attracti of the masses. Yet a satisfying notion of affinity was not obtained this way.

Affinity measured as Work. A second line of thought took in consideration not the force but the work that affinity represents; a it seemed a decisive step when Thomson and Berthelot declared th the heat developed in chemical change corresponds to the work th affinity can produce. Indeed, it was in this way that in many cases a *a priori* calculation of the heat development of a reaction permitte prediction of the direction in which the process would proceed, th direction being that of the evolution of heat. Yet this principle, however weighty, is not absolutely reliable. The chemical actions that produce cold, as that of hydrochloric acid on sodium sulphate, are objections not to be overcome.

The step really leading to a clear and unobjectionable notion of affinity was made in the study of the so-called reversible chemical changes. This reversible character perhaps needs some explanation, easily to be provided by an illustration. Kill a chicken and prepare chicken soup; it would then be very difficult to get your chicken again. This is because preparing chicken soup is not reversible. On the contrary, let water evaporate or freeze; it will be easy to reproduce the water.

Now, at first sight, chemical change does not seem reversible; and indeed it often is not, as in the explosion of gunpowder. But investigations of Berthelot and Péan de St. Gilles on the mutual action of acids and alcohols, and those of Deville and Debray on high temperature action, which even splits up water, have shown that many chemical changes can be reversed. Indeed, we have types corresponding absolutely to evaporation, as the loss of water-vapor from hydrates: and others corresponding as well to freezing and melting, as the splitting of double salts into their components at definite temperatures. *e. g.*, copper calcium acetate at 77° C. Also in analogy with physical phenomena, we have in these reversible chemical changes the possibility of equilibrium, the two chemically different forms of matter coexisting, as do water and its vapor at a maximum pressure.

Such a reversal of chemical change can take place under the influence of temperature, of electricity, of light, of pressure. And the easiest way to arrive at a measure of affinity is presented in the last case, as was foreseen by Mitscherlich. Let us take gypsum as an example. Burnt commercial gypsum, mixed with water, will combine with the water. We know that this chemical change can produce pressure, and that it may be prevented by sufficient pressure and be reversed by it, as Spring succeeded in pressing out sulphuric acid from sodium bisulphate. And it is possible in such cases exactly to determine the limiting pressure, such that a higher one presses out the sulphuric acid while a lower one is overpowered by the affinity action. If the chemical change takes place under a pressure only slightly less than that which would prevent it, thus practically taking place under the limiting pressure, we get out of affinity the greatest quantity of work that it can-possibly produce; and this quantity is the same whatever the nature of the opposing action, be it electricity, light, or anything else. Therefore, in this maximum work we have a sound measure of affinity.

It was a very happy coincidence indeed, that this conception of affinity made possible the application of a physical principle known as the second law of thermodynamics. This principle may be formulated in different ways. For my purpose let me say that it limits the possibility of natural processes to the occurrence of those in which a difference of intensity is diminished. If there is a difference of pressure in two parts of a gas, a movement will occur producing equality; if there is a difference of temperature, heat will be transported so as to produce equality once more. It is curious that such simple necessities, which we all feel as such, can be converted into far-reaching sharply formulated equations, as was done by Carnot and Clausius. These principles were first applied in chemistry by Horstmann. Then, by successive application to chemical problems by Massieu, Gibbs, Helmholtz, and others, was won a system of relations touching the problem of affinity, to which I can give only brief attention:

(1) Affinity may be defined as the maximum quantity of work that a chemical change can produce. Equilibrium ensues when this quantity is zero.

(2) The mass-law can be obtained in a well-founded and somewhat modified form, restricted to dilute gases and solutions.

(3) The Thomson-Berthelot principle assumes a modified form in the rule that a fall of temperature induces the formation of that which develops heat. It is, for instance, in accordance with this rule that at ordinary temperatures water is stable in comparison with detonating gas, and that at high temperatures this relation is reversed, as it was found by Deville to be.

(4) Lastly, we have the phase rule, indicating, for example, in what cases chemical phenomena will be comparable with melting and freezing, and in what cases they will be comparable with evaporation and condensation.

Most curious of all, we can treat problems of affinity in an absolutely trustworthy way, so that our calculations furnish a check upon experiment, without admitting anything concerning the nature of affinity or of the matter wherein the affinity is supposed to reside.

THE PHYSICAL PROPERTIES OF AQUEOUS SALT SOLUTIONS IN RELATION TO THE IONIC THEORY.

BY ARTHUR A. NOYES

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It is generally recognized that the further progress of physical science will be greatly facilitated by a better systematization of the knowledge already accumulated, and this is true in an especially high degree of the newly developed branch of science in which this Section is directly interested. It has therefore seemed to me that the most valuable contribution that I could make toward the solution of the present problems of physical chemistry in correspondence with the aims of this Congress would be a formulation of the present status of some of our knowledge relating to important classes of phenomena which are being actively investigated, but which have not yet received a final interpretation. It was my original hope to discuss several such classes of phenomena; but the effort involved in the collation and criticism of the available data connected with the problem which was first studied forced me to confine my attention to that alone. This problem concerns *the physical properties of aqueous salt solutions in relation to the ionic theory*. This is the subject which I shall attempt to present to you: I hope that its importance and the greater definiteness that can be given to its treatment may compensate for the somewhat limited scope of this paper.

Permit me to say in advance that I have studied this subject primarily from an empirical standpoint, and that it will be my aim to present to you a series of generalized statements of the experimental results, formulated in such a way as to show their relation to the important hypotheses connected with the ionic theory. Unfortunately, it will not be possible in this address to reproduce, or even fully refer to, the data upon which these conclusions are based — a defect serious in a work of this kind, which will be remedied in a subsequent publication. I shall, however, try to show the general character of the evidence for each conclusion and the degree of accuracy within which it has been confirmed. I wish to add that I have been most ably assisted in the preparation of the material upon which this paper is based by Dr. J. W. Brown and Dr. M. S. Sherrill, of the Massachusetts Institute of Technology.

The principles to be first presented have reference to two of the main hypotheses which are commonly employed in quantitative applications of the ionic theory. One of these hypotheses is that *the migration-velocities of the ions of a salt do not vary appreciably with its concentration, at least up to a moderate concentration; and consequently, that the degree of ionization is equal to the ratio of the equivalent conductivity at the concentration in question to the limiting value of the equivalent conductivity at zero concentration* — a ratio which I will hereafter call simply the conductivity-ratio. The other hypothesis is that *ions, and also the un-ionized molecules accompanying them, produce an osmotic pressure substantially equal to the pressure exerted by the same number of gaseous molecules at the same temperature, at least up to a moderate concentration;* an hypothesis which may be more briefly expressed by the statement that the osmotic pressure-constant for dissolved electrolytes is identical with the gas-constant. It is evident that with the help of this hypothesis we can calculate, either from measurements of osmotic pressure or from those of any other property which is thermodynamically related to osmotic pressure, the number of mols in the solution resulting from one formula weight of salt, that is, the quantity which van 't Hoff has represented by the letter i. From the latter, provided the ionization is not complicated by the formation of complex molecules or ions, the degree of ionization is readily derived.

The first of these hypotheses cannot be independently tested, because no direct method of determining the change of migration-velocity with the concentration is known. But the following principle, which has an important significance with reference to the *relative* influence of concentration on the velocities of different ions, has been established by measurements of the concentration-changes at the electrodes attending the electrolysis of salt solutions.

The transference number, or ratio of the conductivity of one ion to the sum of the conductivities of both ions, is constant within one per cent, between the concentrations of $\frac{1}{200}$ *and* $\frac{1}{10}$ *normal, for all salts thus far accurately investigated, except lithium chloride, the halides of bivalent metals, and cadmium sulphate.*

This principle holds true, according to the results of various investigators, in the case of potassium and sodium chlorides, hydrochloric and nitric acids, silver nitrate, barium nitrate, potassium sulphate, and copper sulphate — thus in the case of salts of the three different ionic types, which I will speak of as the uni-univalent, the uni-bivalent, and the bi-bivalent types, in correspondence with the valences of the two ions composing the salt.

Two conclusions are to be drawn from this result. The first is, that complex ions are not present in important quantity in the solutions of these salts. And the second is, that the migration-velocities of the two ions of a salt vary by the same percentage amount, if they vary at all, with changes in its concentration. It is scarcely admissible, however, to regard this last fact even as an indication that the hypothesis of constant migration-velocities is correct; for any change in the character of the liquid medium might well affect the velocities of different ions not far from equally.

Important evidence in regard to this hypothesis and that stating that ions and the un-ionized molecules associated with them have a normal osmotic pressure is, however, furnished by the agreement of the ionization values derived, on the one hand, from the conductivity-ratio, and, on the other, from the properties thermodynamically related to osmotic pressure. Three of these properties have been measured with sufficient accuracy with certain electrolytes to make the results of significance, namely, the freezing-point lowering, the electromotive force of concentration-cells, and the heat of solution in relation to change of solubility with the temperature. Under the assumption that osmotic pressure and gaseous pressure are equal under identical conditions, a relation between each of these properties and the degree of ionization of an electrolyte can be derived with the help of the second law of energetics. Then, either this ionization value may be directly compared with the conductivity-ratio, or, assuming provisionally that the latter is a correct measure of ionization, the magnitude of the property in question may be calculated, and the result compared with that obtained by direct measurement. In the case of the freezing-point lowering, I have adopted the first of these methods. For the five salts for which both reliable freezing-point determinations and accurate conductivity-measurements at 0° exist, the ionization values corresponding to both of these properties have been computed. Especial attention was given to the selection of the best value of the freezing-point lowering constant and to the extrapolation of the conductivity for zero concentration, the details of which cannot be here described. The results may be summarized as follows:

In case of the two uni-univalent salts and the three uni-bivalent salts hitherto carefully investigated, the ionization values derived from freezing-point lowering do not differ from those derived from conductivity, between the concentrations of $\frac{1}{200}$ *and* $\frac{1}{4}$ *normal, by more than* 2 *or* 3 *per cent.*

The five salts referred to are potassium and sodium chlorides, potassium and sodium sulphates, and barium chloride. The two sets of values for potassium chloride, for which an abundant experimental material exists, exhibit no pronounced or systematic differences; but for the other four salts the freezing-point leads to values which are in general from two to three per cent higher at all concentrations than the conductivity-ratio. The fact that these differences do not, as a rule, increase with increasing concentration indicates that they may be due to some constant experimental error, or to an error in the extrapolated conductivity value.

Accurate measurements have been made by Jahn of the electromotive force of concentration-cells consisting of two silver or mercury electrodes covered with silver chloride or mercurous chloride, one of which is immersed in a weak solution and the other in a strong solution of sodium or potassium chloride. These measured values were compared by him with those calculated from the thermodynamic relation between electromotive force and the concentrations and degrees of ionization of the salt in the cell. Unfortunately, however, the thermodynamic relation employed involved the assumption that the ionization varies with the concentration in accordance with the Mass-Action Law — an assumption which is known not to be true of the ionization values derived from conductivity. The assumption is, therefore, an irrational one — one by which the question at issue is prejudged. What should be done in calculating the electromotive force so as to determine whether the conductivity-ratio gives ionization values consistent with the measured electromotive forces is evidently to assume that the ionization changes with the concentration in the way that the conductivity indicates that it does. Arrhenius recognized this error and partially corrected for it by a method of approximation. I have repeated the calculations by an exact thermodynamic formula based on an empirical law expressing the change of the conductivity-ratio with the concentration, to which I will refer later. The results are summed up in the statement that, *when the conductivity-ratio is assumed to represent the degree of ionization of the salt, the calculated values of the electromotive force of concentration-cells exceed the measured ones by only about one per cent in the case of potassium and sodium chloride between the concentrations of* $\frac{1}{600}$ *and* $\frac{1}{20}$ *normal.* The measured electromotive force corresponds to an ionization value at the latter concentration about one per cent less than the conductivity-ratio.

The thermodynamic relation involving heat of solution has been accurately tested with only one salt — potassium perchlorate; but since it is a different salt from those used in the other experiments, and since its concentration was fairly high — $\frac{1}{8}$ normal — the result is of interest. It was found that the *measured heat of solution was less by only* 1.1 *per cent than that calculated under the assumption that the conductivity-ratio is equal to the degree of ionization.* The measured heat of solution corresponds to an ionization value $2\frac{1}{2}$ per cent lower than the conductivity-ratio.

With respect to these small deviations of the results obtained by the three methods of comparison, it is important to note that they lie in opposite directions, the freezing-point lowering corresponding to larger values of the ionization, and the measured electromotive forces and heat of solution to smaller ones than the conductivity-ratio. This fact makes it almost certain that they are due to experimental errors. Nevertheless, further exact measurements of all these properties are highly desirable.

From a theoretical standpoint these three methods are based on the same hypotheses — namely, that the osmotic pressure-constant for ions and un-ionized molecules is identical with the gas-constant; that the conductivity-ratio is a correct measure of ionization, and that complex molecules or ions are not present in the solution. The concordance of the results furnishes, therefore, a strong confirmation of the correctness of these fundamental hypotheses. The only alternative conclusion is that an error in one of these hypotheses is compensated by an error of opposite effect in one of the others; but it seems very improbable that such a compensation could occur in the case of so many salts of different chemical nature and different types through the range of concentration ($\frac{1}{200}$ to $\frac{1}{4}$ normal) for which the agreement of the experimental results has been shown to hold true. It is certainly more consistent with the modern methods of science to adopt these simpler hypotheses, which are in full accord with the considerable number of facts thus far known, than deliberately to introduce more complicated assumptions for which there is at present no experimental warrant.

The combination of these hypotheses with the experimental values of the quantities involved at varying concentrations makes necessary the further conclusion that the *degree of ionization of salts, whether derived from the conductivity-ratio or from thermodynamic relations involving the equality of the osmotic pressure-constant and the gas-constant, does not vary with the concentration even approximately in accordance with the Law of Chemical Mass-Action.*

This empirical consequence of the fundamental hypotheses of the ionic theory has led several investigators to raise a theoretical objection to them, it being contended that the laws of thermodynamics require that the validity of these hypotheses involves that of the Mass-Action Law itself. This apparent inconsistency between the inductive and deductive conclusions makes it probable that some unproved, erroneous assumption is tacitly involved in the theoretical derivation. That there is, in fact, a possible alternative, which has, I believe, been previously overlooked in the thermodynamic discussions, will be evident from the following considerations. The thermodynamic relations between ionization and freezing-point, electromotive force, or heat of solution, involve only the assumption that the work done in reversibly separating water from a solution at constant concentration is equal to that done in producing the same volume-change in a gas, which implies, of course, that the ions and un-ionized molecules have in the presence of each other normal osmotic pressures. On the other hand, the derivation of the Mass-Action Law equation is based on cyclical processes which necessarily involve the separate introduction and removal of the un-ionized molecules and of the ions into or from solutions of different concentrations, and it further involves the assumption that this introduction or removal of molecules or ions can be effected by the application of an external pressure equal to that osmotic pressure which each of them possesses in the mixture; that is, the possibility is ignored that the separation of the molecules from the ions may itself give rise to some new force, and may involve, consequently, another quantity of work than that corresponding to the osmotic pressure. The ionic theory would evidently predict a result of this kind if an attempt were made to separate the positive ions from the negative, even though their osmotic pressures when present together were perfectly normal; and it is quite conceivable, even though the reason for it be not apparent, that the separation of the un-ionized molecules from the ions, with which they may be in electrical as well as chemical equilibrium, should involve an abnormal quantity of work. The assertion that the validity of the osmotic-pressure principle necessarily implies that of the Mass-Action Law is therefore unwarranted from a deductive standpoint; while the inductive evidence, pointing strongly as it does to the substantial correctness of the former principle and the complete inadequacy of the latter one, makes it highly probable that the *separation of un-ionized molecules from ions does involve the expenditure of other work than that corresponding to their osmotic pressures.*

Since the ionization does not change with the concentration in accordance with the Mass-Action Law, it is natural to inquire what the law of its change is. This matter has been investigated from an empirical standpoint by several investigators with the help of the conductivity data. The results justify the statement of the following principles:

The un-ionized fraction of a salt as determined from the conductivity-ratio is proportional to the cube root of its total concentration, or to that of its ion-concentration, between $\frac{1}{2000}$ *and* $\frac{1}{10}$ *normal, in the case of both uni-univalent and uni-multivalent salts.* That is, $1-\gamma=Kc^{\frac{1}{3}}$, or $1-\gamma=K(c\gamma)^{\frac{1}{3}}$, where γ is the degree of ionization, c the concentration and K a constant. The first of these functions was proposed by Kohlrausch, the second by Barmwater. Owing to the relatively small variation of the ionization, these two functions cannot differ much as to their constancy, but on the whole the experimental data indicate that the second function is somewhat more constant. The average deviations of the actual measurements from the values corresponding to this function are $\frac{1}{4}$ per cent in the case of ten uni-univalent salts, $\frac{1}{3}$ per cent in the case of nine uni-bivalent salts, and also $\frac{1}{3}$ per cent in the case of three uni-tri- and uni-quadrivalent salts. The maximum deviations are two or three times as great. It is of interest to note that the strong mineral acids, hydrochloric and nitric, behave like salts in this respect. These functions have been shown to apply to potassium and sodium chlorides through a range of temperature extending from 18° to 306°. They do not apply at all closely to such salts of the bi-bivalent type as magnesium and copper sulphates, perhaps owing to appreciable hydrolysis. Nor do they represent satisfactorily the experimental data for any kind of salts at the very low concentrations lying between $\frac{1}{1000000}$ and $\frac{1}{2000}$ normal, nor at concentrations higher than $\frac{1}{5}$ normal.

The experimental results are also well expressed by the statement that *in the case both of uni-univalent and uni-bivalent salts, between the concentrations of* $\frac{1}{100000}$ *and* $\frac{1}{5}$ *normal, the concentration of the un-ionized molecules is proportional to the concentration of the ions raised to a constant power, varying somewhat with the salt and the temperature, but as a rule only between the limits of* 1.43 *and* 1.56. That is, $c\,(1-\gamma) = K(c\gamma)^n$, where $n > 1.43$ and < 1.56.

This general function was first applied by Storch and was afterward further discussed by Euler and Bancroft. It has the advantage over the previous ones that it represents the data with accuracy even up to the highest dilutions, and therefore can be used for obtaining the limiting conductivity at zero concentration.

The applicability to the salts of different types of either of these principles governing the change of ionization with the concentration leads to the important conclusion that *the form of the concentration function is independent of the number of ions into which the molecules of the salt dissociate.* This remarkable fact, though previously recognized, has not been sufficiently emphasized, and it has been often ignored in discussions of the cause of the deviation of the ionization of salts from the requirements of the Mass-Action Law. It seems to me to show almost conclusively that chemical mass-action has no appreciable influence in determining the equilibrium between ions and un-ionized molecules. How complete the contradiction with the Mass-Action Law is may be illustrated by citing the specific facts that for di-ionic, tri-ionic, and tetra-ionic salts this law requires that the concentration of the un-ionized molecules be proportional to the square, the cube, and the fourth power, respectively, of the concentration of the ions; while the experimental data show that it is approximately proportional to the $\frac{3}{2}$ power of that concentration, whatever may be the type of salt.

Having seen in what manner the degree of ionization varies when the concentrations of both ions of the salt are simultaneously varied by dilution, it is of interest to determine the effect of changing the concentration of either ion separately. A study of the conductivity and the freezing-point of mixtures of two salts having one ion in common throws much light upon this question, for the following simple principle has been found to represent this phenomenon: *The conductivity and the freezing-point lowering of a mixture of salts having one ion in common are those calculated under the assumption that the degree of ionization of each salt is that which it would have if present alone at such an equivalent concentration that the concentration of either of its ions were equal to the sum of the equivalent concentrations of all the positive or negative ions present in the mixture.*

This somewhat complicated statement may be illustrated by the following example: Suppose that a mixed solution is 0.1 normal with respect to sodium chloride and 0.2 normal with respect to sodium sulphate, and that it is 0.18 normal with reference to the positive or negative ions of these salts. The principle then requires that the ionization of either of these salts in the mixture be the same as it is in water alone when its ion-concentration is 0.18 normal.

This principle in regard to the conductivity of mixtures, which has been definitely stated by Arrhenius, is shown by the existing data to hold true, almost, if not quite, within the small experimental error of the determinations both for mixtures of salts of the same type and for those of salts of different types up to a concentration of at least $\frac{1}{2}$ normal. Experiments confirming this principle have been made upon eight pairs of uni-univalent salts by Arrhenius, Manson, and Barmwater. In addition, the principle has been shown by several Canadian investigators, Archibald, McKay, and Barnes, to hold true for mixtures of potassium and sodium sulphates, potassium and copper or magnesium sulphates (up to 0.1 normal), potassium sulphate and chloride, barium and sodium chlorides, and zinc and copper sulphates — thus for almost every possible typical combination of uni-uni-, uni-bi-, and bi-bivalent salts. That the same principle is true of the freezing-point lowering is shown by the measurements of Archibald with mixtures of potassium and sodium sulphate. This proves that the phenomenon really has reference to the degree of ionization and that it does not arise from a possible variation in the migration-velocities of the ions.

Of especial interest is the relation of this principle to the validity of the Mass-Action Law. Almost all investigators of the conductivity of mixtures have concluded, from the fact that upon mixing solutions of equal ion-concentration there is no change in ionization, that the results do conform to this law. Yet it is scarcely conceivable that this law can apply to mixtures of salts in which the concentration of one ion is varied while maintaining that of the other constant, in view of the fact that it is known not to hold true for the variations of the concentrations of both ions produced by dilution. And in reality this conclusion, if regarded as a general expression of the facts, is entirely unwarranted. It is true that for certain typical combinations of salts — those for which from one molecule of each salt results by ionization not more than one ion of the kind not common to the salts — the principle here stated does coincide with the requirement of the Mass-Action Law. But for combinations not so characterized the Mass-Action Law predicts, as is readily seen upon formulating the equations, a conductivity of the mixture widely divergent from that actually found, and, therefore, from that expressed by the principle under consideration. This last statement applies, for example, to the mixtures before referred to of potassium sulphate with sodium sulphate, and of potassium sulphate with copper or magnesium sulphate, the first of which have been studied both with respect to their conductivity and freezing-point. The Law of Chemical Mass-Action here again shows itself entirely inapplicable to the phenomena connected with the ionization of salts. The opinion of some investigators that the deviations from this law indicated by the conductivity were only apparent, and that they were attributable to variations in the migration-velocity, has arisen, no doubt, from the fact that they have confined their attention to di-ionic salts, and have failed to recognize, on the one hand, the striking divergences from it exhibited by tri-ionic salts, and, on the other, the substantial correspondence of the conductivity and freezing-point results.

Combining this principle in regard to the ionization of mixed salts in solution with the empirical concentration law of Storch for single salts, we are led to the conclusion that the ratio of the concentration of the un-ionized part to the product of the concentrations of the two ions (but in the case of tri-ionic salts *not* raised to a power corresponding to the requirements of the Mass-Action Law) is a function of the sum of the equivalent concentrations of all the ions in the solution and of that alone.[1] This ratio is, moreover, roughly inversely proportional to the square root of the total ion-concentration.

The correctness of this principle is further demonstrated by the fact that with its aid the conductivity of a mixture of two salts without a common ion can be computed from their separate conductivities. This is shown by the conductivity measurements, made by Archibald and more recently by Sherrill, upon mixtures of potassium chloride and sodium sulphate, or of sodium chloride and potassium sulphate. Up to at least 0.2 normal concentration, the agreement between the observed and calculated values is within 0.5 per cent. On the other

[1] This is expressed mathematically by the following equation in which c_1 and c_2 represent the equivalent concentrations of the two salts, and γ_1 and γ_2 their degrees of ionization in the presence of each other:

$$\frac{c_1\gamma_1(c_1\gamma_1+c_2\gamma_2)}{c_1(1-\gamma_1)}=K_1(c_1\gamma_1+c_2\gamma_2)^{n-1}$$

hand, the divergence of the observed values from the requirement of the Mass-Action Law amounts to many per cent.

It seems appropriate at once to supplement these principles in regard to the form of the concentration function by a statement of two general rules which have been found to express the magnitude of the ionization of salts of different types. These rules, unlike the preceding principles, are only crude approximations; but, nevertheless, they prove of some assistance in rough applications of the ionic theory, and undoubtedly possess an important theoretical significance not yet recognized. They may be stated as follows: (1) *the decrease of ionization with increasing concentration is roughly constant in the case of different salts of the same type;* and (2) *the un-ionized fraction at any definite molal concentration is roughly proportional to the product of the valences of the two ions in the case of salts of different types.* Thus, at 0.1 normal concentration the mean value of the degree of ionization for 17 uni-univalent salts measured at 18° is 83.3 per cent, the average deviation of the separate values from this mean is 2.1 per cent, and the maximum deviation of any of them is 5.4 per cent, of the mean value; while for fourteen uni-bivalent salts the mean value is 69.8 per cent, the average deviation 5 per cent of this, and the maximum deviation about 10 per cent of it. The un-ionized fraction in $\frac{1}{20}$ molal solution is 13½ per cent for these univalent salts; 30 per cent, or about twice as great, for the uni-bivalent salts; and 60 per cent, or about four times as great, for the three bi-bivalent salts investigated (zinc, magnesium, and copper sulphates). The salts of mercury and cadmium are pronounced exceptions to the rule.

Far more extensive material for testing these rules is furnished by the measurements made at 25° between the concentrations of $\frac{1}{32}$ and $\frac{1}{1024}$ normal. In the case of the uni-univalent salts, data exist at this temperature and these concentrations for thirty-six inorganic salts, about sixty-five sodium salts of organic acids, and about an equal number of hydrochlorates of organic bases. A consideration of all these data shows that, with only three or four exceptions not of a pronounced character, the values of the degree of ionization of all these salts in $\frac{1}{32}$ normal solution lie between the limits of 84 and 90 per cent and are fairly uniformly distributed throughout this range of 6 per cent. For sixty-seven uni-bivalent salts the corresponding limits of the ionization values are 72 and 81 per cent, while for only four such salts do the values lie beyond these limits. For the six uni-trivalent salts investigated the range is from 67 to 76 per cent; for the three uni-quadrivalent salts from 59 to 63 per cent, and for twelve bi-bivalent salts from 49 to 63 per cent, while three such salts show more considerable variations. The values of the un-ionized fraction corresponding to the mean of these two limits for the different types of salts at the same equivalent concentration increase somewhat more slowly than the product of the valences of the ions. The proportionality becomes a fairly close one, however, when the salts are compared at the same molal instead of the same equivalent concentration. Thus, with the help of the Kohlrausch concentration function, it is calculated from the preceding values that the un-ionized fractions in $\frac{1}{32}$ molal solution are as follows:

13 per cent for the uni-univalent salts,
29½ per cent for the uni-bivalent salts,
41 per cent for the uni-trivalent salts,
62 per cent for the uni-quadrivalent salts,
55 per cent for the bi-bivalent salts,—

which are seen to be approximately the required multiples of the constant factor 14.

Before leaving this subject it should be stated that the results conform, on the whole, about equally well to the rule that *the decrease of equivalent conductivity, (instead of ionization) is roughly constant for salts of the same type;* and when the comparison is made at the same equivalent concentration, distinctly better to the rule that the *decrease of equivalent conductivity is proportional to the product of the valences of the ions for salts of different types.* When compared at the same *molal* concentration, however, this rule does not apply. These rules were originally stated by Ostwald. They differ not inconsiderably from those expressing the change in ionization — namely, to an extent corresponding to the variations of the conductivities at extreme dilution. The deviations are so irregular, however, that, from an empirical standpoint, the choice between the two pairs of rules is arbitrary. In either form these rules seem to justify the inference that the degree of ionization of salts, unlike that of the organic acids and bases, is not primarily a specific chemical property determined by chemical affinity, but that it is determined, at least in the main, by the magnitude of the electric charges on the ions.

The establishment of the principle in regard to the ionization of a mixture of salts has a direct bearing on the phenomenon of the effect of one salt on the solubility of another with a common ion. It has been usually assumed that in a (not too concentrated) saturated solution the un-ionized molecules of the salt always have the same concentration; and, secondly, that the product of the *ion*-concentrations (each raised to a power corresponding to the number resulting from one molecule) also retains the same value. And the experimental results in several cases have been shown to accord fairly well with these two hypotheses. Yet their simultaneous validity is quite inconsistent with the principle in regard to the ionization in mixtures. In fact, when considered in the light of this principle, the existing data lead to the conclusion that the former hypothesis is not even approximately true, and that the latter one, at any rate in cases where the ionization is far from complete, is affected by a considerable error. One example may be cited: when thallous chloride and bromate, each of which alone has a solubility of about $\frac{1}{40}$ normal in water at 40°, are simultaneously present as solid phases, the solubility of each is reduced by the other to an extent which shows that the concentration of the un-ionized molecules is diminished by about 15 per cent and that the product of the ion-concentrations is increased by about 5 per cent. This case is a typical one; but what the quantitative law of the influence in question is, can be determined only by a further study of the phenomenon. In the case of tri-ionic salts, the ion-concentration product is even approximately constant, only when the square — not when the first power — of the concentration of the univalent ion is employed. This has been shown by experiments with lead iodide in the presence of potassium iodide, with lead chloride in that of other chlorides, and with calcium hydroxide in that of ammonium chloride.

I will close by calling your attention to a remarkable principle in regard to the properties of salt solutions, of a character quite distinct from those thus far considered. That many properties of dilute salt solutions can be expressed as the sum of values assigned once for all to the constituent radicals or ions was long ago recognized, and has often been cited as a corollary from the ionic theory. That this additivity of properties persists up to fairly high concentrations is a fact, however, that has received scant consideration, owing to its apparent lack of relationship to that theory. This fact is shown strikingly in the case of certain highly specific optical properties which are ordinarily found to be dependent in a high degree on molecular structure. Thus, the experimental data fully warrant the statement of the principle that *the optical activity and the color of salts in solution, when referred to equivalent quantities, are independent of the concentration and therefore of the degree of ionization of the salts, and are additive with respect to the properties of the constituent ions even up to concentrations where a large proportion of the salt is in the un-ionized state.* Abundant data might be cited in support of this principle, especially with reference to optical activity. But I can only illustrate the character of the evidence by presenting a few of the results obtained by Walden with the salts of α-brom-camphor-sulphonic acid. In $\frac{1}{30}$ normal solution he found the following values of the molal rotatory power:

Lithium salt	275	Acid itself	273
Sodium salt	272	Beryllium salt	274
Potassium salt	273	Zinc salt	272
Thallium salt	273	Barium salt	272

The values are seen to be substantially identical, although the conductivity shows the acid to have an un-ionized fraction of 7 per cent, the salts of the univalent metals one of 16 per cent, and those of the bivalent metals one of 30 per cent, and although the un-ionized molecules present contain in some cases the elements hydrogen, lithium, and beryllium of very small atomic weights, and in two others the elements thallium and barium of large atomic weights.

If there were not other evidence to the contrary, the existence of this general principle, which is also applicable to many other properties, would almost warrant the conclusion that the salts are completely ionized up to the concentration in question, and that the decrease in conductivity is due merely to a change in migration-velocity. But, in view of the apparently conclusive evidence against such an hypothesis, we can only conclude that the form of union represented by the un-ionized molecules of salts differs essentially from ordinary chemical combination, it being so much less intimate that the ions still exhibit their characteristic properties, in so far as these are not dependent upon their existence as separate aggregates.

These, then, are the empirical principles to which a critical analysis of the experimental data leads. Upon these principles must be based the rational, theoretical explanation of the phenomena in question. The discovery of that explanation constitutes one of the most important of the present problems of physical chemistry.

SHORT PAPERS

Dr. Frank K. Cameron, of the United States Department of Agriculture, presented a paper on "The Application of Physical Chemistry to Agricultural Chemistry," in which he stated that there was some difficulty in approaching this subject, for the reason that what constitutes agricultural chemistry cannot be clearly defined, and the boundary lines between it and other branches of applied chemistry are not always evident. Much of biological chemistry and much of geological chemistry along lines on which notable achievements have been made by the applications of the principles and methods which in recent years have come to be called physical chemistry, could with propriety be claimed also for agricultural chemistry. One finds agricultural chemists engaged in the examination of drugs, fertilizers, leathers, and tannins, etc., as well as in the examination of foods or soils. Important applications of physical chemistry are to be found along many of these lines which might be claimed for the agricultural chemist, but disputed as belonging to the field of the industrial chemist or others. The manufacture of nitric acid by electrochemical methods, while a problem of industrial chemistry, is important mainly because of the use of nitrates in agriculture. But confining one's self strictly to the work professedly done in the immediate interest of agriculture or farm practices, there is much evidence to be found of the increasing influence of physical chemistry.

A valuable and interesting paper followed on the close relation and applications of physical chemistry to the science of agriculture, and the speaker concluded by saying that "The problems presented by agricultural chemistry do not commend themselves to the investigator who is interested in chemistry alone for its own sake. They are generally complex and not well suited to the elucidation or illustration of hypotheses in pure chemistry. The pecuniary rewards which agricultural chemistry offers are not sufficient in comparison with other fields to tempt the man trained in physical chemistry who wishes to use his equipment to this end. But to the man who has the training and who cares not so much that his problems be pure science as that they may be undertaken in a scientific spirit and with scientific methods, the application of physical chemistry to agriculture offers many opportunities. He can have the satisfaction of not only doing good scientific work but directly helping an industry of ultimate importance to all his race and of immediate importance to the numerically largest class of the race."

Professor Henry Snyder, of the University of Minnesota, read a paper on "The Digestibility of Bread."

Professor Louis Kahlenberg, of the University of Wisconsin, read a paper "On the Relation between the Processes of Solution, Chemical Action, and Osmosis."

SECTION D—PHYSIOLOGICAL CHEMISTRY

SECTION D — PHYSIOLOGICAL CHEMISTRY

(*Hall* 16, *September* 22, 3 *p. m.*)

Chairman: Professor Wilbur O. Atwater, Wesleyan University.
Speakers: Professor O. Cohnheim, University of Heidelberg.
Professor Russell H. Chittenden, Yale University.
Secretary: Dr. C. L. Alsberg, Harvard University.

PROBLEMS IN NUTRITION

BY OTTO COHNHEIM

(*Translated from the German by Prof. J. L. R. Morgan, Columbia University*)

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The object of the papers read here is not so much the consideration of any one restricted branch of science as it is the discussion of those broader fields which lie between and are intimately connected with several branches of science. In accord with this I propose to speak on a subject belonging primarily to the physiology of nutrition, but one which at the same time has very great politico-economic importance. To-day, as the result of the great progress which has been made in the physiology of nutrition, we can in general give a definite answer to the question as to the extent of the agreement between the actually observed dietary of an individual or group of individuals, and the conclusions obtained theoretically. At any rate to-day we can account physiologically for, and regard as physiologically necessary, a whole series of phenomena which in the past could only be accepted as empirical facts. The physiological consideration of race-dietary, on the other hand, will show how it happened that social considerations for decades have directed and restricted physiological progress.

The food of man, as is well known, is composed of proteids, fats, and carbohydrates. In most food-stuffs we have all three classes of substances; only sugar and butter belong solely to one class, the former being a carbohydrate, the latter a fat. The proteids assume a particularly important position owing to the fact that our bodies themselves are composed to a very large extent of proteinaceous material and hence can only be built up by proteids. The major portion of the proteids we absorb are derived from bread and meat. The foods richest in proteids are meat, fish, eggs, cheese, milk, etc.; in short, those foods having an animal origin. The older physiology considered the material composition of the food as the essential characteristic, although even Liebig recognized metabolism as a process of combustion, and it is the work of the Voit school which has caused the calorimetrical value of food to attain its present central position. By its combustion, the nutriment absorbed supplies the energy which is required by the human body for its various purposes. The value of a food, then, can be expressed by the amount of energy it can produce, and this value can be stated clearly and accurately in the ordinary terms of energy, *i. e.*, in units of heat, or calories. As the result of years of work by various investigators it has been found that the individual foods can be almost completely represented by their calorimetrical values. Rubner, Zuntz, and Atwater, by differing methods, have all come to the same conclusion, viz. that for purposes of heat and muscular action, *i. e.*, for its principal requirements, proteids, fats, and carbohydrates, the organism can employ vegetable and animal foods equally well. That the civilized nations of Europe and America employ bread and meat as the principal source, while the Indians and Chinese use rice exclusively, and the Esquimos fat, is not due to any difference in physiological organization, or to differing needs of the body, but simply to the more or less easy attainment of the substances, fruitfulness of the soil, and other secondary circumstances.

The law of the calorimetrical equivalency of all food-stuffs has but one notable exception. So far as investigation has been carried out it has been found that the dietary of any man or race always contains a certain and apparently similar amount of proteinaceous material. The kind of material seemingly has little influence, but about 100 gr. of protein is found with great constancy in the daily food of the individual. In the food of a powerful man, who exerts a fairly large amount of muscular effort, Voit found 118 gr. of proteids per day, and he assumes this as a basis for the dietary of a soldier. Weaker men, doing less muscular work, require, according to Voit, a smaller quantity of proteids. For the poorly nourished, and also for those who are incapable of any intense effort, the hand-loom weavers of Zittau, the poor of Naples, and the poorest negroes of Alabama, von Rechenberg, Manfredi, and Atwater have found much lower amounts. During comparatively short laboratory experiments, Munk, Hirschfeld, Kumagawa, and especially Sivén have found considerably smaller quantities. For well-nourished men, during long periods, Chittenden, only, found less proteids; otherwise, physiological investigation, as well as the experience of daily life, has shown that it is not well to consume less than 100 gr. of proteids per day.

This amount, indeed, is rarely exceeded, for Chittenden has shown that even the diet of well-to-do Americans, which appears to us as the richest in proteids, scarcely ever exceeds 100 gr. of proteids a day, and the investigation of the freely-chosen fare of the most various individuals leads to the same result.

The question as to the need of the human body for 100 gr. of proteinaceous material per day has often been raised; and even to-day cannot be answered with certainty. During the last years, however, we have learned of a series of reasons which may serve to throw some light upon the subject.

That the growing organism requires proteids is self-evident, for in this way only can it obtain the materials of which it is composed. We know further, however, that the adult organism continually repairs and increases its organs and consequently also requires proteids. According to Zuntz a man increases his muscles when he does unaccustomed work (for example, when he learns a new sport) or even by increased exertion upon his usual work. Bunge attributes a considerable requirement of proteids in adults to the loss of organ-proteids in the sperm of man, and to menstruation, pregnancy, and lactation in woman. And later years have disclosed the genetic relations of many decomposition-products of the proteids with carbohydrates, with substances of the bile, and others, which are necessary, at any rate for a time, to neutralize poisons, or which are essential for the intermediate metabolism; and these relations appear to render desirable at least the presence of a copious supply of proteinaceous material.

A second reason is more difficult to grasp. Even the first metabolic experiments of Voit showed that although the proteids possess no higher nutritive (fuel) value than the carbohydrates, and a very much smaller nutritive (fuel) value than the fats, they burn very much more rapidly; and this has since been repeatedly confirmed. When the supply of proteids in the food is increased above the actual need of the body, the fats and carbohydrates are stored up, and the proteids are burned to a very much greater extent. The relations between the cells of our bodies and the substances absorbed as nutriment can best be illustrated by an analogy. For the neutralization of an alkali any acid may be employed; but when several acids of differing strength are present together, the strongest one will be partially saturated before the others even begin to react. In the same way protoplasm can supply its need with all three nutritive substances; but when all three are present at once the proteids burn first. With a large excess of the other two, however, the action of their masses becomes evident, exactly as in the illustration with the acids, and they protect the proteids from combustion. In the absence of fats and carbohydrates, the body readily goes into such a state that its own proteids are attacked, *i. e.*, it consumes itself. This is probably the most important reason why a definite minimum amount of proteids is essential in a small total amount of nutriment. That further differences exist among the individual organs themselves is still to be proven, but at present it appears quite probable.

A third ground has been disclosed during the past few years by the work of the great Russian investigator, Pawlow. We know from this that the nervous connection of the digestive system with the sense organs of the head determines the enjoyment of the food, and hence regulates the choice of that. We know further from Pawlow, Weinland, and Starling that this connection is not fixed once for all, but varies according to the needs of the time. When any such relation is observed we must always conclude that it is adapted to an end, for otherwise it would have disappeared within a short time. Pure proteids are tasteless and odorless, and also fail to act upon the sensitive nerves of the stomach and intestine; in all natural foods, on the other hand, the proteids are always associated with the pleasant-tasting constituents of nutriment, and those which stimulate digestion. For us, just as for the carnivorous animals investigated by Pawlow, the substances richest in proteids are always the most pleasant to the taste, and those which arouse the appetite the most. The foods which are poorer in proteids, as rice and potatoes, stimulate the digestion less and consequently are more difficultly digestible. A food-stuff free from proteids has already been shown in animal experiments to be impossible as a diet, and even in experiments with substances which are poor in proteids Sivén and Röhl encountered insurmountable difficulties owing to the tastelessness of the material.

Even though we do not as yet know all the reasons, it is at any rate obvious that, for long periods of time and for normal nutrition, Voit has discovered the correct condition, viz., that an amount of proteids equal to 100 gr. per day is essential, or at any rate can be designated as desirable.

Since in consequence of the special internal organization of the human body, and because it is the minimum amount used by all men, this amount of proteids is independent of the form of nourishment absorbed, and independent of the habits of life. Even as early as 1860 and 1866 Voit showed that the protein consumption of those doing hard work is not greater than of those who do none; and this result has been confirmed many times. The American physiologist, Atwater, has made an especial study of this question, using his respiration calorimeter. As the average of numerous experiments, carried out with the greatest exactness, he found that the subject of experiment, whether resting or working, decomposed the same amount of proteids, even when the production of calories by the work rose to double or more. Indeed, the decomposition of proteids can even be decreased by muscular activity, for the larger total of nutriment consumed prevents the decomposition of the proteids of the body.

The total amount of nutriment of a man is almost exclusively determined by the muscular work he performs. The mental work has nothing to do with nutrition; whether the brain is used intensely, or whether it is retained as inactive as is possible, as far as we know to-day, does not seem to affect the requirement of energy by the body, nor its requirement of food. The amount of energy required by the individual to sustain his bodily temperature differs but slightly, for the differences in external temperature are nearly compensated by the wonderfully acting heat regulation of our bodies, and the artificial heat regulation by our clothes and dwellings. The influence of muscular activity is very much greater. A man resting quietly in a warm room requires from 1500–1700 calories per day; while one working in the laboratory, or sitting, produces from 2100–2400 calories. For light hand labor this is increased to 2800, while for laborers, Liebig and others have observed from 4000–6000 calories, and Atwater and Wood found up to 8000 calories for the lumbermen of Maine. As the average of all his experiments, Atwater found 2270 calories for quiescent, and 4550 calories for hard-working people, *i. e.*, exactly double the value.

Although the total number of calories varies according to the work, the amount of proteids for all men remains approximately equal, and from this we can draw an important conclusion. The food of those not doing physical work must be relatively richer in proteids, for an equal absolute amount of proteids must be contained in a smaller total amount of food. The foods richest in proteids are meat and the other products of the animal kingdom, and it is evident that the diet must be the richer in meat, the less physical work done by the person. An illustrative example will make this quite clear. A laborer does hard physical work, and consequently requires a diet which produces 5000 calories per day. Consuming only bread, potatoes, and other vegetable products, he would obtain 100 gr. of proteids and even more without trouble. Let us assume that he moves to a city and changes his occupation, living a sedentary life. For this he would require but 2500 calories, and retaining the quality of his dietary he would have to do one of two things. Either he must eat the previous quantity, which would be impossible for any length of time, for the body could not use such an excessive amount, or he must decrease it to one half, whereby he would obtain the requisite number of calories, but with them only 50 gr. of proteids. To nourish himself properly, then, he will have to decrease his allowance of food to one half, and add to it 50 gr. of proteids, *i. e.*, about 250 gr. of meat. This example, of course, is extreme, and will not often be observed with such distinctness. The principle, however, is always to be observed. The food of those belonging to the well-to-do classes, *i. e.*, of those who do no hard physical work, in all countries contains the most meat. This, however, is no luxury, but is based upon physiological grounds. Comparing different countries, or different classes in the same country, we always obtain the following result. To the degree that pure hand labor is replaced by the work of the head, and that of overseeing machines, to that same degree is the consumption of meat increased. This is shown most obviously, however, by the comparison of the country population with that of the city. The modern mill-hand lives, it is true, by the work of his hands, but that work is quite different from that done by the farm laborer. The overseeing and directing of the complicated machines, as every other form of skilled labor, requires attention, intelligence and dexterity, but does not require the muscular exertion necessary for mowing, threshing, and the felling of trees. With the difference in activity there must also be a difference in the quantity and kind of food. The people in a city in general eat less in total amount, but this food is qualitatively different, *i. e.*, must consist of substances relatively rich in proteids, as meat and other animal products.

From the politico-economical, as well as from the medical, point of view the smaller amount of food consumed by the mill-hand, as compared to that of the farm laborer, is regarded as a sign of degeneration. This is obviously untrue, for there is no general standard of nutrition which is applicable, or even desirable, for all men. The nutriment, with respect to quantity, is dependent solely upon the amount of muscular work done. On the other hand, the increased consumption of meat, eggs, and other foods, agreeable to the taste because rich in proteids, has been attributed to the greediness of the urban population. Nothing could be more false. It is just for this large class that the enjoyment of meat and other foods rich in proteids is a physiological postulate; and for the other large class making up the urban population, merchants, officials, clerks, etc., this is true in even a more striking degree, for the physical work necessary in such occupations is still smaller in amount, and their food must consequently be even richer in proteids.

It is not for me to draw further conclusions from the physiological principle that the food of the urban population should contain less vegetable and more animal substance. I must rather consider the influence of these relations upon physiology itself. The classes not doing severe physical work are the higher and better-to-do, and, since they are great meat-eaters, it is but too easy to conclude that in general meat-eaters are the most valuable and best. This opinion is very rife in lay circles, and even physiology has not long been free from it. The great Liebig, the founder of the doctrine of scientific nutrition, held that meat is the only active form of food (for muscular activity), and ascribed to it a very high nutritive value. Liebig's theory was disproved 44 years ago by Voit and soon afterward by Fick and Wislicenius. But even to-day there are physiologists who hold fast to the Liebig doctrine, and indeed the relics of it are still to be found everywhere in physiology and medicine. The tenacity of life of this old error would be difficult to explain, were it not apparently supported by the daily experience that the well-to-do eat meat, eggs, etc., while the day laborers satisfy themselves with bread and potatoes.

From the difference in the diet of those who do severe muscular work and those who do none, there is a further conclusion to be drawn. The only indigestible constituent of human food is cellulose. Cellulose, being contained only in vegetable food, forms but a small constituent of the diet of the man doing little physical work. According to von Knieriem cellulose is of great importance in the process of digestion, for as indigestible, solid substance it stimulates the activity of the intestine. While carnivorous animals, with their short muscular intestine, do not require it, graminivorous animals, with their long, coiled, weak intestine, cannot do without it for any length of time. Man, in the organization of his digestive apparatus, stands midway between these two extremes, and while cellulose is not absolutely essential to him, its absence sometimes causes a motoric atrophy of the intestine which results in chronic constipation and its consequences. The connection between constipation and sedentary occupations has long been recognized, but people have been too prone to attempt to explain it mechanically, whereas the connecting link in reality is the dietary of the man leading the sedentary life. In his daily life such a man does but little physical work, and consequently in general eats little, and especially little of the vegetable food poor in proteids but rich in cellulose. We hear nothing of digestive troubles of the people living in the country, while city people, especially the well-to-do, suffer severely from them. In England and America, judging from the wide advertisement of purgatives, the trouble is much more common than in Germany; but in both lands the rye bread, which is comparatively rich in cellulose, is replaced by fine wheat bread, which is much poorer in cellulose, and the substitution of animal products for bread is also more common than with us.

The vegetarians have been agreed on this point for a long time. They observed how many digestive and other troubles are common to the dwellers in cities (*i. e.*, where few live like the vegetarian peasant), and all without knowledge of the physiological grounds held up the peasant as the ideal for the citizen. But what is correct for the peasant, who must produce 4000–5000 calories, is not correct for those who require but 2300 calories or less. He obtains, then, as explained above, too little of proteids, or aids himself by his fondness for the vegetables rich in proteids, but at the same time poor in cellulose, and hence fails utterly to attain his end.

It would be more correct if the transition to the vegetable diet is combined with a treatment which will increase the need of substance, as has been done from non-scientific sides and without knowledge of the physiological principle.

The only rational cure for the disturbances which can ultimately be traced to the lack of muscular activity is to devote one's self to this muscular work outside of one's daily occupation, as is possible by aid of the various sports. It is no accident, but rather a necessary physiological phenomenon, that the need of active sports has always developed wherever there is a class of society made up of those doing no intense physical work. Indeed, we can readily follow this in history; when the citizens of the Greek cities devoted themselves to athletic sports, when the knights of the Middle Ages jousted, there was always an aristocracy who did no manual labor. The home of our modern sports is England, the oldest industrial country. In Germany the first steps in the direction of sports were made at the universities, where thousands of young men did mental work. The scope of the sport of to-day is very much broader, however, for its followers include merchants and the workers in the various industries. Sports lead directly to a change in the food requirements of the individual; every bicyclist, every mountain climber knows that on his trips he can eat things which do not appeal to him at all when at home. I must be content here to restrict myself to these indications disclosing the scientific principles of this subject, which is apparently so far removed from our point of departure. By unwearying work the physiology of nutrition has established a scientific experimental foundation upon which other sciences may now build.

THE PRESENT PROBLEMS OF PHYSIOLOGICAL CHEMISTRY

BY RUSSELL HENRY CHITTENDEN

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In considering a proper presentation of the subject assigned me, I am impressed with the influence which a man's own field of work and his own line of thought will naturally exercise upon his point of view. It may be questioned whether his judgment can be wholly trusted, whether he will not in fact, unconsciously it may be, give a dwarfed or one-sided presentation of the subject from a natural habit of looking at things in their bearing upon the line of work and thought in which he himself is personally most interested. While this may not be wholly undesirable, of still greater advantage will be a brief but judicious presentation of all the more important problems that confront the physiological chemist of the present day; but whether this can be done satisfactorily in the time allotted is very questionable. However, the effort will be made to emphasize, so far as the time will allow, what to the writer seem the more significant and far-reaching problems in physiological chemistry that call for speedy solution.

Of fundamental importance is the question, what is the exact chemical constitution of proteid matter? The basis of all cell-life, the most complex molecule that enters into the structure of the living organism, proteid or albuminous material holds a peculiar position. A labile molecule, it is easily prone to change, and its many decomposition-products confront us on all sides in our study of life's processes. Yet to-day, in spite of all that has been accomplished, even with the brilliant work of Kossel and Emil Fischer, we still lack adequate knowledge of all the groups and radicles that are combined in this atomic complex.

In the study of metabolism and nutrition, both in health and in disease, in our conception of the anabolic processes of life, in our theories regarding the chemical relationships of the varied katabolites floating about through the organism, and in many other connections, we need for our guidance a full knowledge of the chemical nature of this most important class of substances. Thanks to the work of many brilliant investigators, our knowledge is progressing and broadening, but we still lack that comprehensive understanding of the inner structure of the molecule that would serve to illuminate our field of vision and give us a clear conception of the chemical constitution of this group of physiologically important ground substances in living protoplasm.

As is well known, the proteid bodies constitute a group of widely divergent substances. Of these, the basic protamines are undoubtedly the simplest and lowest in the scale, and it is quite probable, as suggested by Kossel, that these substances constitute the nuclei of all proteids. The protamines differ somewhat among themselves, but as a group they are characterized by their high content of diamino-acids, especially arginin. Thus, salmin yields on decomposition 84 per cent of arginin, clupein 82 per cent, cyclopterin 62 per cent, and sturin 58 per cent.[1] Sturin also contains 13 per cent of histidin and 12 per cent of lysin, while the other protamines appear to contain no diamino-acids aside from arginin. Further, the protamines contain diamido-valerianic acid, monoamido-valerianic acid, tyrosin or p-oxyphenyl-amidopropionic acid, skatolaminoacetic acid, a-pyrrolidincarbonic acid and serin.[2] Salmin [3] has also been shown to contain alanin, leucin, probably also phenylalanin and aspartic acid.

If we pass from the simplest of the proteid bodies to the most complex, as the nucleins, we find present in the latter not only arginin, lysin, and histidin, but, in addition, such bodies as thymin, the purin bases, leucin, aspartic, and glutamic acids, two sulphur-containing groups, furfurol-forming groups, pyrrolidincarbonic acid, a skatol-forming group, phosphoric acid, amidovalerianic acid, a levulinic acid-forming group, glycosamine, pentose, uracil, and probably phenylamido-propionic acid.[4] In the histon from the nucleohiston of the thymus, we find in addition to the hexone bases and the monoamido-acids characteristic of the ordinary albuminous bodies such substances as glycocoll, cystin, and alanin.

These statements, brief and incomplete though they are, will serve to illustrate the complexity of the proteid molecule, and at the same time they indicate the close genetic relationship which unquestionably exists between the varied members of this large group of substances. There is no doubt that Kossel and his co-workers, in their efforts to unravel the constitution of the protamines, are pursuing a wise course in paving the way for a comprehension of the exact nature of the more complicated proteids. There is no doubt that the protamines of one type or another are integral parts of every proteid molecule, and when their chemical constitution is made quite clear, much will have been accomplished toward a fuller understanding of the more complicated forms.

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[1] Kossel and his students. See Kossel and Dakin, *Ueber Salmin und Clupein, Zeitschrift für physiologische Chemie*, Band 41, p. 407.

[2] Kossel und Dakin, *Beiträge zum System der einfachsten Eiweisskörper, Zeitschrift für physiologische Chemie*, Band 40, p. 565.

[3] Abderhalden, *Die Monoaminosäuren des Salmins, Zeitschrift für physiologische Chemie*, Band 41, p. 55.

[4] See Kossel, *Über den gegenwärtigen Stand der Eiweiss Chemie, Berichte der Deutschen Chem. Gesellschaft*, Jahrgang 34, p. 3214.

It needs no imagination to foresee what a full knowledge of the chemical constitution of all types of proteid matter will mean for the physiologist and physiological chemist. Much that is now cloudy and uncertain in our understanding of cell and tissue metabolism, in our comprehension of nutritive changes in general, of digestive proteolysis and of intracellular autolysis, will become clear as crystal. The problem, however, is not a simple one, but is exceedingly complex, for it is to be remembered that just as the individual proteids differ from each other in superficial reactions and characteristics, so do they undoubtedly differ in their inner structure. Hence, we must expect to find variations in the make-up of the individual molecules, and it is one of the most important problems of to-day to ascertain the nature of these chemical variations, to recognize the individual groups that give character to the molecules, and to learn how these groups are bound together to make the typical proteid of this and that tissue or organ. The solution of this problem promises much for the advancement of physiological chemistry, but it holds out the promise of even more for the good of physiology in general, since there is bound up in the chemical structure of the proteid molecules a full and complete explanation of tissue changes, and of many metabolic phenomena which to-day are as sealed volumes.

The development of our knowledge regarding the cell as a physiological unit has led to a fuller recognition of the importance of discriminating between the primary and secondary cell constituents. As a result, the physiological chemist has come to realize the necessity of more exact knowledge as to the nature and distribution of the primary components of cells, because of the bearing this knowledge may have upon the general question of how far the lines of chemical decomposition characteristic of each group of cells are dependent upon the character of the anabolic processes by which that particular cell protoplasm is formed, and how far the peculiar katabolic or retrogressive changes of that group of cells are due to outside influences, exerted by specific nerve fibres, or by the character of the blood and lymph stream. The physiological chemist would know whether the secret of glandular secretion, of tissue changes, of metabolic activity, is to be found in the particular forms of protoplasm that enter into the structure of the component cells, whether it is associated in any way with some inherent quality of the primary cell constituents.

There is something marvelous in the unerring certainty with which a given group of cells performs its work, never deviating a hair's breadth from the beaten course, and turning out year after year a definite line of products for the specific purpose in view. Why is it that the epithelial cells of the salivary glands always manufacture mucinogen and ptyalin; the gastric gland cells pepsinogen, renninogen, and hydrochloric acid; the cells of the pancreas trypsinogen and steapsin; the hepatic cells bilirubin, biliverdin, and the specific bile acids; the cells of the thyroid iodothyrin, and the cells of the adrenals epinephrin? Essentially the same blood and lymph bathe all these cells with a like nutritive pabulum, and yet each group of cells performs its own line of work, never going astray, in health, and never even temporarily producing a product which rightfully belongs to the other class of cells. Are we to suppose that all these varied products are manufactured from the same cell protoplasm, from a common stock, that each one owes its origin to some particular force controlled by extracellular influences, each group of cells being made to manufacture a given product out of the same mother substance? Or, on the other hand, are we to assume that each group of cells, as it is developed, has as a birthright the quality of producing from its particular protoplasm a certain line of products, simply because of the peculiar chemical nature or constitution of that protoplasm?

In other words, do all the intricacies of cellular activity depend primarily upon the character of the anabolic processes by which that protoplasm is built up out of the food-materials by which the cells are nourished? It may be just as difficult to explain why and how the cells are able to manufacture a specific protoplasm out of a common pabulum, but the main problem which confronts us is surely capable of being solved. We need to know how far the primary cell constituents of different groups of cells, of the different organs and tissues, are similar to or unlike each other. If it is shown that the primary cell constituents differ for each glandular organ and tissue, that each group of individualized cells has a protoplasm characterized by some specific feature, then we shall have reason to believe that the anabolic processes are as much, if not more, responsible for individuality of function than the katabolic processes. We may conceive of all protoplasm being built, so to speak, on a certain general plan of structure, but with side-chains of varying nature, and that these side-chains determine in a measure the character of the katabolic or alteration products that result from the natural activity of the cell protoplasm. In other words, if this conception be true, it is the chemical constitution of the cell protoplasm that is primarily responsible for the character of the changes that take place in all active tissues and organs. The extent of oxygenation as influenced by the circulating blood, the direct and indirect influence of various nerve fibres, etc., may all act as modifying agents, but only to the degree of accelerating or inhibiting the rhythmical process which travels along a certain definite channel because of the peculiar chemical nature of the cell protoplasm. Once started, the process of katabolism takes a definite course, with formation invariably of the same products, because that particular cell protoplasm, owing to its peculiar make-up, tends to break down along certain definite lines of cleavage, as it were, and so the products split off are always the same.

We already have considerable knowledge which tends to indicate that the cells of individual organs and tissues have a certain individuality as regards their primary components, notably in the nucleo-proteids present, but our knowledge is by no means complete enough to permit of broad generalization. The problem is an interesting one, and permits of a definite answer by the application of thorough and persistent investigation.

As an allied question, more or less in harmony with what has just been said, reference may be made to the part which ferments and enzymes possibly play in initiating and carrying forward tissue changes, as well as the metabolic changes that occur in glandular organs. Ferments have come into such prominence of late years as responsible agents for so many transformations that we may well query whether their influence does not extend far beyond the limits originally assigned to their field of activity. The discovery of oxidases and the part which these agents may play in tissue changes, the undoubted existence of ferments in such glands as the thymus, suprarenal, spleen, etc., by which the recently studied autolytic changes in these glands are produced, raise the question whether ferments or enzymes are not far more largely responsible for the many transformations that take place in active tissues than has been hitherto supposed. Consider for a moment the peculiar products which result from the self-digestion (autolysis) of many of the glands so far studied. Note how the nucleo-proteid of the thymus, for example, breaks down, yielding xanthin and a little hypoxanthin, together with uracil, but no guanin, adenin, or thymin.[1] How the adrenal nucleo-proteid likewise yields by autolysis considerable xanthin, but only traces at the most of the other alloxuric bases (Jones). By the self-digestion of the spleen, guanin as well as hypoxanthin is conspicuous, but it is a noticeable fact that in the autolysis of the thymus, for example, there is no appreciable amount of leucin to be detected, thus indicating that the above autolytic changes are not due to any ordinary proteolytic enzyme, but to some peculiar enzyme which acts directly and solely upon the nucleo-proteids, splitting off certain of the contained alloxuric groups. In harmony with this view, Jones has just announced the presence in the pancreas, thymus, and adrenals, of an enzyme to which he gives the name of guanase, which has the power of transforming guanin into xanthin. The same investigator also claims the presence in the spleen of a related enzyme, called adenase, which transforms adenin into hypoxanthin. The inference is that in many glands and tissues there are specific enzymes, as yet undiscovered, which may be responsible for at least some of the transformations known to occur there.

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[1] Jones, *Ueber die Selbstverdanung von Nucleoproteiden*, *Zeitschrift für physiologische Chemie*, Band 42, p. 35.

That autolysis may be a possible explanation of the process of animal metabolism has been suggested by Levene [1] and also by Wells.[2] It has been clearly indicated by such able workers as Salkowski, Jacoby, and others, that practically all animal cells contain within themselves ferments or enzymes that are capable, under suitable conditions, of digesting or breaking down the cell-contents by a process similar to ordinary proteolysis, and it may perhaps be assumed that all active cells carry forward their ordinary metabolic processes by the agency of these intracellular ferments. Moreover, it is not inconceivable that ferments or enzymes of several kinds may exist side by side in a given group of cells, just as they are known to exist in the pancreas, by which we might infer the possibility of a series of transformations taking place at essentially the same time, through the harmonious action of a row of enzymes physiologically quite distinct.

Further, the recently discovered reversible action of enzymes, on which we have at command so much valuable work, suggests the possibility of a maintenance of cell-equilibrium through this peculiarity of action, thus affording a tangible explanation of the means by which intracellular nitrogenous or proteid equilibrium is maintained, the various cells of the body building up or breaking down the proteid matter of their own tissues as circumstances require. If these ideas are true, then our conception of ferment action must be considerably broadened, and we have before us the possibility of explaining many of the phenomena of tissue metabolism by the action and interaction of intracellular enzymes. This is a problem well worthy of broader study, with a view to the elucidation of the general laws that govern tissue changes in general. In this connection we also have suggested the possibility of interaction of another kind, viz., that interdependence of one tissue or gland upon another for the full development of its functional activity, as illustrated by the part played by the enterokinase of the intestinal glands in the development of an active trypsin from the zymogen of the pancreatic cells, and by the action of the internal secretion of the pancreas upon the inert constituents of the muscle to develop in the latter an active glycolytic enzyme. How far this general principle extends in the metabolic phenomena of the body is entirely problematical, but merits careful study. Here, then, we ave an added field of inquiry, worthy of careful consideration, if we re to possess a clear understanding of nature's processes.

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[1] *Die Endprodukte der Selbsiverdauung tierischer Organe, Zeitschrift für physiologische Chemie*, Band 41, p. 393.

[2] *On the Relation of Autolysis to Proteid Metabolism, Amer. Journal of Physiology*, vol. 11, p. 351.

Between the animal and the vegetable cell certain sharp lines of disinction are frequently drawn. Physiologists are wont to believe that he processes characteristic of the cells of animal tissues and organs are ssentially destructive, *i. e.*, that they are principally katabolic, while n vegetable tissues, on the other hand, constructive processes are ery conspicuous. In no way is this better illustrated than in the prealent opinions regarding the parts played by the two classes of cells n the metabolism of proteid matter. We are accustomed to think hat all proteid matter has its primary origin in the synthetical power f the vegetable cell, aided by its contained chlorophyll and the beneicent action of the sun's rays. The animal cell, on the other hand, can nerely transform and reconstruct the various proteids furnished by the vegetable world, being without power to manufacture proteid matter *de novo* out of the simple groups and radicles which the vegetable cell utilizes so rapidly. In ordinary proteid katabolism, the various nitrogenous decomposition-products are presumably all converted into urea and allied substances adapted for excretion. If, however, there is reversible ferment or enzyme action in the animal body, why may there not also be power to utilize, in some measure at least, the crystalline nitrogenous bases and amido-acids so abundantly formed in trypsin proteolysis, for the construction of fresh proteid matter? One may well query, considering the vigor of the proteolytic action of the enzymes poured into the alimentary tract, whether all these nitrogenous waste products represent just so much lost energy in their production and a further loss of energy in their immediate excretion from the body. In harmony with the "luxus consumption" theory we may assume wisdom and ultimate gain in this speedy decomposition of excessive proteid foods in the alimentary tract, but the argument is not very convincing. Why may not animal cells, or the animal body as a whole, build up proteid matter out of simple nitrogenous compounds analogous to the action of plant cells? Loew[1] has indeed experimented in this direction and states that the biuret-free end-products resulting from the proteolysis of ordinary food albumin can be utilized by the animal body for the maintenance of nitrogenous equilibrium, etc., equally well with the common proteid food-stuffs. His conclusions, however, have been called in question by other investigators, notably by Lesser,[2] whose experimental data failed to confirm the above conclusion.

The problem, however, is an exceedingly important one. If the animal body has no power of utilizing the varied nitrogenous compounds of simple constitution formed in the gastro-intestinal tract by the digestive enzymes; if there is a complete lack of ability to construct new proteid matter out of these simple decomposition-products, then surely we must inquire what is the real purpose of their formation. It is true that, with the limitations of our present knowledge, it is difficult to see why, if digestive proteolysis has for its sole object the conversion of the proteid foods into forms suitable for absorption, there should be any considerable breaking-down of proteid beyond the proteose or peptone stage, since the latter bodies would seem to be most easily adaptable for transformation into the proteids of blood lymph and tissue. On the other hand, it is well known that the proteid of the food is possessed of a physiological and chemical nature quite different from that of the proteid in the blood and tissues of the feeding animal, and it is quite conceivable that a synthetical process might be essential — in some degree — for the manufacture of the specific proteids called for by the blood and tissues of that particular species or individual. The question is one that demands careful consideration and thorough investigation, for it touches upon a chapter in nutrition on which we have at present very little satisfactory or convincing knowledge.

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[1] *Ueber Eiweissynthese im Thierkörper, Archiv für exper. Pharmakol. u. Pathol.*, Band 48, p. 303.

[2] *Ueber Stoffwechselversuche mit den Endprodukten peptischer und tryptischer Eiweissverdauung, Zeitschr. für Biologie*, Band 45, p. 497.

In this connection we may call attention to another problem, somewhat far-reaching, but suggested by one of the preceding paragraphs, viz., the possible physiological action of the many katabolites, or decomposition-products resulting from tissue-changes throughout the animal body. In vegetable tissues, many of the nitrogenous products common to these structures are endowed with marked physiological power, as witness the vegetable alkaloids and the non-nitrogenous bodies like salicin, digitalin, picrotoxin, etc. Years ago, physiologists recognized that some of these nitrogenous bodies present in animal tissues did have a distinctly toxic action when introduced directly into the circulation, and hence they were frequently called animal alkaloids, but our knowledge upon these points is exceedingly obscure and indefinite. When we take into consideration the large number of nitrogenous products formed and present in the various tissues and organs of the body, products of proteolysis and of tissue-changes; when we consider how these products circulate through the organism, in blood and lymph; how they come in more or less immediate contact with the different cells of the body prior to their decomposition or elimination, we cannot avoid being impressed with the part they may play in stimulating and modifying tissue or other changes.

The significance of this suggestion is made all the more potent by the knowledge recently acquired concerning several of the internal secretions of the body and the powerful physiological influence exerted by their components. Where can be found a more active physiological agent than the blood-pressure-raising constituent of the adrenals, the epinephrin? Where is there a more active agent in modifying the nutritional processes of the body than the iodine-containing constituent of the thyroid, the iodothyrin? These may truly be counted as representing a type of substances manufactured or secreted primarily for the physiological effect they are capable of exerting; but what about the host of other substances present in the body, many of them simple products of katabolism? May they not have some marked physiological property that if known would serve as a sufficient excuse for their formation? Or, may they not possess some hidden or obscure property which if once understood would make clear a secondary or subsidiary function of no small import for the maintenance of physiological equilibrium, or for the welfare of the body? Many suggestions and some facts present themselves illustrating how direct and indirect influences may be exerted, all pointing toward the harmonious action and interdependence in function of many of the substances formed in the body. Some, however, undoubtedly have more or less of a toxic action, especially when formed in excessive or undue amounts. Thus, the alloxuric bases seemingly cause fever when injected into the circulation or taken *per os*,[1] and according to the recent observations of Mandel[2] there is a very striking relationship between the quantity of alloxuric bases eliminated in the urine and the temperature of the body in cases of aseptic fevers, indicating that these substances, with possibly other incomplete products of tissue-metabolism, are important factors in the production of febrile temperature. We may confidently expect that a thorough study of the physiological action of all the varied katabolic products formed in the body will result in a decided expansion of our knowledge regarding the part these substances may play in normal and abnormal metabolism, and in nutrition in general.

Just here, reference may be made to the many problems in the broad field of nutrition that confront the physiological chemist of the present day. The maintenance of life on a sound physiological basis is one of the practical problems in physiological chemistry, and its solution is not yet attained. We need fuller knowledge regarding the part played by the different nitrogenous food-stuffs, the relative physiological value of animal and vegetable proteid, the relative value of fats and carbohydrates as nutrients aside from their different calorific power, and, by no means least, a fuller and more accurate knowledge of the true physiological needs of the body for proteid foods. Our present dietetic standards are absolutely false and valueless. Our present conception of the physiological needs of the body is altogether faulty and distorted. Our ideas of the rate and extent of proteid metabolism necessary for the maintenance of health and strength are crude and inexact. We place the nitrogen requirement of the healthy man at an absurdly high level, apparently because observation has shown that man is disposed to consume an equivalent in proteid food per day. We need to ascertain by scientific experiment how far such standards are justified; to determine by definite analysis the amounts of nitrogen actually required to maintain nitrogen equilibrium and keep up bodily and mental vigor. Upon the physiological chemist of the present day rests the responsibility for the establishment of nutritive standards that will endure the test of scientific criticism, that will harmonize with daily experience, and that will prove to be physiologically correct.

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[1] See Burian and Schur, *Archiv für die gesammte Physiologie*, Band 87, p. 239.

[2] *The Alloxuric Bases in Aseptic Fevers*, *Amer. Journal of Physiology*, vol. 10, p. 452.

Further, we need to know more concerning the relative decomposition within the body of the truly organized proteid matter of the tissues, and of the albuminous food-stuffs which, having been digested and absorbed, are in a sense a part of the tissues, but not thoroughly or completely incorporated as an integral part of the living cells. Does the urea of the daily excretion come primarily from the breaking-down of the organized proteid, or does it come preferably from the disintegration of the circulating proteid? We recall the famous experiments of Schöndorff, in which blood was made to circulate through the muscles and liver of well-nourished and fasting dogs, with the result that the urea of the blood was increased only when the blood circulated through the tissues of a well-nourished animal. It made no difference with the result whether the blood employed was from a well-fed or a fasting animal; the essential factor was the condition of the muscle tissue through which the blood was made to flow. Schöndorff drew the natural conclusion that the extent of proteid metabolism was dependent upon the nutritive condition of the cells of the tissue, upon the mass of the living cell-material, *i. e.*, upon the amount of morphotic proteid present, and that the proteid content of the intermediary fluids, as blood or lymph, was of no moment in determining the rate of urea formation.

We may well doubt, however, if all the urea formed daily under ordinary conditions of life comes solely from the breaking-down of the truly organized or morphotic proteid. It is more than probable that the urea has at least a twofold origin, and, if so, it is an important matter to be able to discriminate between that which comes from the breaking-down of the unorganized albumen, and that which is derived from the organized tissues. Unquestionably, the decomposition of organized proteid, the morphotic part of the living protoplasm, is quite different from that of the unorganized pabulum of the cell and surrounding media. Quite possibly, the influences controlling the two lines of metabolism are different; perhaps, there are even different kinds of nerve control.

Equally important is it for the physiologist to know more fully regarding the sources of the carbonic acid resulting from oxidation in the body. What proportion of the ever-varying output of this gaseous product of metabolism comes from the oxidation of organized tissue-material, and what from the oxidation of circulating carbohydrate and fat and unorganized material in general? We have learned, for example, that the excretion of carbonic acid runs more or less closely parallel with the degree of muscular activity, and we should possess the means of discriminating between the output from true tissue-oxidation and that which is derived from extracellular sources. A study of the excretion of carbonic acid by fasting individuals, under different conditions of life and activity, would be helpful in throwing light upon this question, and also in giving us a clearer idea of the minimal requirements of the body for non-nitrogenous foods to make good the loss of energy in heat-liberation, muscular work, etc. By such a study we might hope for added light upon that much-discussed problem, the source of the energy of muscular contraction. While most physiologists are certainly agreed that this energy comes preferably from the oxidation of non-nitrogenous matter, there remain many obscure points upon which we need enlightenment.

We likewise need fuller and more exact knowledge of the ways in which uric acid originates in the body, especially regarding its relationship to intracellular decomposition. Our present understanding of the twofold origin of this substance — endogenous and exogenous — is most helpful in making clear many formerly obscure points connected with the formation of this substance from the different classes of food-stuffs. To-day, however, we understand quite clearly the genetic relationship between the free and combined purin bases and uric acid, but we are still uncertain whether this substance is formed to some extent synthetically and whether when once formed it is all eliminated unchanged or undergoes oxidation, in part, into less harmful substances. In other words, we do not yet know how far the uric acid which is contained in the daily urine is a measure of the *production* of uric acid for the twenty-four hours. Uric acid and the alloxuric bases are such important substances, in their influence upon health and the general nutritive condition of the body, that it is extremely important for us to know more concerning their origin and their ultimate fate in the body. We may likewise inquire where uric acid is formed. Does it originate entirely in the liver, or are there other depots where it is produced and collected?

Turning our attention now in another direction, we may revert to the relationship between stereochemical configuration and physiological action as a fruitful subject for investigation. Many interesting facts have already been gleaned, and certain general rules or laws have been formulated, connecting given lines of physiological action with a definite chemical structure. Thus, it is well understood to-day, for example, that all substances which contain a nitro or nitroso group united with or bound to oxygen have the effect of dilating blood-vessels, while, on the other hand, substances which contain the same nitro or nitroso group joined to carbon have a quite different physiological action, being mostly blood-poisons. Further, nitrils, R. CN, tend to produce coma, while isonitrils, R. N=C, are much more toxic and tend to produce paralysis of the respiratory centre.[1] In other words, it is clearly manifest that certain definite groupings within the molecule are the cause of the physiological action of the molecule. At the same time, it is also known that in order to have the physiological action of a substance manifest, not only must it contain the necessary group or groupings, but there must likewise be present a second group which has the power of combining with and holding fast to the tissue upon which the physiological action manifests itself. Slight chemical alteration of a substance may, therefore, interfere with or nullify its ordinary physiological action without necessarily altering the physiologically active groups; but by simply changing these other groups through which the molecule ordinarily attaches itself, so that the latter can no longer adhere to the cell-substance or tissue-protoplasm, there occurs a consequent loss of physiological action.

Another fact clearly understood is that two substances having the same nucleus and like side-chains, with an entirely similar grouping, may still be physiologically unlike, owing to a different arrangement in space. This is well illustrated by the dextro- and lævo-rotary tartaric acids, one of which is readily utilized by *Penicillium glaucum* as nutriment, while the other cannot be so consumed. Many other illustrations might be cited, especially with various types of organic poisons, all tending to show that physiological action is dependent upon the *arrangement* of the atoms or radicles in space, as well as upon the *nature* of the atoms or radicles. With these facts before us, we see many lines of inquiry presenting themselves, many problems demanding solution, with reference both to pharmacology and physiology.

Confining our attention more especially to physiological matters, we are certainly justified in considering the application of these principles to many of the substances conspicuous in the processes of the body. The work and suggestions of Pasteur and Emil Fischer have indicated certain possibilities regarding the nature and action of enzymes, not to be overlooked. Stereochemical configuration may be just as much responsible for enzyme action, for proteolysis, amylolysis, etc., as any other feature of the active molecule, and how far other lines of physiological action may be due to chemical structure and the configuration of the molecule, who can say? One's thoughts naturally turn to the living muscle plasma and the chemical changes that follow or accompany the advent of rigor mortis; to the circulating blood and lymph, and the transformations that occur when these fluids are withdrawn from the protecting influence of the endothelial lining of the living vessels; to the axis cylinder of the nerve-fibres and the changes that occur when the fibres are severed from their connection with the ganglionic cells. These and many other suggestions arise, all calling for a further study of the chemical constitution and stereochemical configuration of the molecules involved, since in the knowledge thus gained may be found the solution of many physiological processes now shrouded in mystery.

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[1] See Fränkel *Ergebnisse der Physiologie, Dritter Jahrgang. Biochemie*, p. 291.

The reference just made to nerve-fibres and ganglionic cells suggests another problem in physiological chemistry, solution of which has long been deferred, viz., the exact chemical nature of nerve-tissue, and the character of the changes involved in the passage of a stimulus or nervous impulse through a nerve to its ending in the muscle or secreting cell. Further, what is the real purpose of the complex myelin surrounding the axis cylinder of medullated nerves, and the corresponding substance imbedded in the gray matter of the brain and cord? These are problems that have long waited solution, and yet they are vital to any clear understanding of the nutritive or other changes that take place in nerve-tissue, either in rest or in activity. Nerve-tissue is strikingly peculiar in its large content of phosphorized bodies of the lecithin type, cerebrosides and cholesterins. These substances, complex in nature and of large molecular structure, are all alike in having the physical properties of fats. Further, lecithin and the cerebrosides all contain fatty acid radicles in large amount, and in addition lecithin contains the radicle of glycero-phosphoric acid. Moreover, the cerebrosides contain a carbohydrate group yielding galactose on decomposition, so it is plain to see that the bodies which give character to the myelin material are highly nutritive substances with high calorific power. These facts might readily be taken as indicating that the function of the myelin is to nourish the more important axis cylinder, to furnish the necessary pabulum for growth and repair as well as to meet the daily demand for energy-yielding material.

While we may speculate, however, as to the part these peculiar substances play in the life of nerve-tissue, we really possess very little positive knowledge of their true purpose. Indeed, we do not know how these bodies actually exist in the living tissue, as is well evidenced by the utter lack of agreement among physiological chemists as to the entity of the so-called protagon. Whether this phosphorized substance, studied by so many investigators, exists as such in the living tissue, or whether it is simply an intimate mixture of lecithin, cerebrin, and one or more other substances, is not yet settled to the satisfaction of all concerned. Further, it is not at all impossible that the cerebrosides, as well as lecithin and possibly cholesterin, may exist in the living tissue combined with some one or more of the proteids present there. Our lack of knowledge is deplorable, and yet, in the words of Sir Michael Foster, this is one of the "master tissues" of the body. Surely, considering the preëminent position and controlling influence of this tissue, we may look for a speedy clearing away of the darkness that enshrouds our understanding of the exact chemical composition of nerve-tissue, and especially of the way these peculiar substances of the myelin material exist in the living tissue.

Again, we may ask ourselves what is the nature of the chemical changes that take place in nerve-tissue; in the ganglionic cells of the gray matter and in the axis cylinder of the nerve-fibres. When a muscle contracts there is a measurable chemical decomposition. The energy of muscular contraction comes from the breaking-down of non-nitrogenous components of the muscle, and perhaps in some measure from the decomposition of nitrogenous constituents. Further, there is a liberation of heat, a development of lactic acids, etc. When a stimulus is applied to a nerve, on the other hand, no such manifestations of chemical action are apparent. The muscle to which the nerve is attached contracts, the secreting cell pours forth the product of its activity, etc., but there is no noticeable change in the nerve itself, no recognizable liberation of heat, no change of reaction, no output of carbonic acid, that can be detected. Are we to conclude, then, that the axis cylinder of the nerve-fibre acts simply as a conducting agent without itself undergoing any change? Is it to be compared to an electric wire, with the surrounding myelin material, the substance of Schwan, serving as a convenient insulating or protective medium? If we are to accept this view, what are we to say regarding the non-medullated fibres? Do not they need an insulating material likewise? We can argue that the myelin substance is especially adapted for the nourishment of the nerve, that its high potential value renders it peculiarly suitable as a concentrated nutriment, and that its intimate contact with the neuraxis and with the ganglionic cells of gray matter proclaims its probable use in this direction. Moreover, if we follow this line of argument still further, we may be led to believe that the stimulation of a nerve, its power of conductivity, etc., are associated with chemical decompositions along its axis as marked in their way as those that occur in a contracting muscle-fibre. Truly, we have here a multitude of questions, for which at present no satisfactory answers are to be found. The problems are on the surface awaiting solution.

Finally, emphasis must be laid upon a series of problems in physiological chemistry, true solution of which will do much to explain natural and artificial immunity, the action of toxins and antitoxins, the bactericidal action of blood-sera, the effect of oxidizing enzymes, of animal and vegetable origin, upon toxins of various kinds, etc. Ehrlich's theories regarding the protection furnished by antitoxic and bactericidal sera, so elaborately devised, constitute a working hypothesis of great value, but we need much additional knowledge concerning the nature and action of the so-called complements and anticomplements, of amboceptors, of haptophor groups, of agglutinins, of precipitins, and of hemolysis. The physiological chemist studies with care the important and suggestive work being carried forward by the many brilliant investigators in pathology and bacteriology, with the feeling, however, that the true explanations for most of the phenomena in question are chemical, and that the actions and interactions involved are chemical ones, to be eventually made clear by a fuller chemical knowledge of the toxic and antitoxic substances themselves and of their alteration and combination under different physiological conditions.

The well-known natural immunity possessed by some animals toward certain diseases, together with the difficulty experienced by most micro-organisms in developing in the healthy body,—a difficulty which at once disappears when from any cause the tissues of the body lose their original vitality and vigor,—all point to the presence in the healthy body of certain general or specific substances which are directly deleterious to the micro-organisms. Such substances are obviously bactericidal, and it is equally plain that in the bodies of many species of animals there are specific antisubstances present which are lacking in other species, thereby explaining the natural immunity of the former towards certain diseases. As is well known, blood-serum possesses, as a rule, a bactericidal power upon most micro-organisms, and we have every reason to believe in the existence of specific substances in the serum which exert some influence upon the growth and development of micro-organisms, and also upon the toxic products they tend to elaborate. These protective substances — the alexins of Buchner — appear to be proteid in nature, resembling globulins, since they are precipitated from serum by the action of certain strong solutions of alkali salts, as sodium sulphate. We know, however, very little regarding their chemical nature aside from the fact that they are obviously very complex, although perhaps even this point is not quite certain. These protective substances are presumably elaborated by the leucocytes of the blood and lymph, cells rich in nuclein and nucleoproteid material. Doubtless, also, some of the gland-cells in the body have a corresponding action; statements which, if true, tend to emphasize the possible proteid nature of the protective substances.

While in a general way we may say that the natural immunity to certain bacteria possessed by some animals is due in large measure to an inhibition of the growth of the micro-organism, it must also be remembered that there is in many species a distinct immunity to the action of the poison which the specific micro-organism produces. This immunity depends either upon a destruction of the poison as by oxidation, upon a combination between the poison and some constituents of the active protoplasmic cells of the body, thereby rendering the poison inactive, or, lastly, upon some action of the specific protoplasmic cells of the body usually affected by the poison, by which the latter is unable to combine with the cells upon which it ordinarily acts. All these suggestions, however, imply chemical reactions of some kind, and obviously should be understood for a betterment of our knowledge upon this important matter.

Again, the specific immunity which shows itself after exposure to a given disease, so that a second infection becomes practically impossible, can be explained satisfactorily only on chemical grounds, viz., by the presence in the blood and lymph of certain protective or immunizing substances which presumably originate through chemical changes in the blood-serum, under the influence of the bacteria causing the disease. These are chemical substances, formed through chemical decompositions or alterations of normal constituents of the blood, and obviously we need to know more of their exact nature.

Following Ehrlich's views, specific antitoxins, bactericidal sera, etc., result from the overproduction of molecules in cells which are sensitive to the action of toxins and other bacterial products. Antitoxins so formed unite with toxins, and the so-called complementary bodies and the bactericidal anti-bodies combine with the bacterial cells, thus affording protection. These processes of alteration and combination, however, are presumably all chemical, involving either alteration of chemical structure, or direct combination of bodies chemically the opposite of each other. Further, the so-called haptophor groups of the toxin molecule are probably represented in fact by chemical groups or radicles, which owe their power of combination with corresponding groups of other cells to chemical affinity. Again, the complementary body, normally present in all healthy blood-sera and which is needed along with the specific anti-body for the destruction of bacterial cells, must owe its activity to the power of chemical combination. Hence, we have presented to us at every turn the question of the chemical nature of these various substances, toxin and antitoxin, complement, receptor, haptophor, etc., which are of such vital importance in the production and maintenance of immunity and protection. Surely this is one of the most important problems of the present day in the domain of physiological chemistry, and calls for both patience and skill of the highest order in its solution.

SHORT PAPER

Mr. Edward Mallinckrodt, Jr., of St. Louis, Missouri, read a paper to this Section on "The Diet, Physical Condition, and Mental Performance of Certain Students in Harvard University."

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DEPARTMENT XI—ASTRONOMY

DEPARTMENT XI—ASTRONOMY

(*Hall* 8, *September* 20, 4.15 *p. m.*)

CHAIRMAN: PROFESSOR GEORGE C. COMSTOCK, Director of the University Observatory, Madison, Wisconsin.

SPEAKERS: PROFESSOR LEWIS BOSS, Director of Dudley Observatory, Albany, New York.

PROFESSOR EDWARD C. PICKERING, Director of Harvard University Observatory.

THE Chairman of the Department of Astronomy was Professor George C. Comstock, Director of the University Observatory at Madison, Wisconsin, who opened the proceedings of the Department with the following remarks:

"We who are American astronomers have been wont to meet under other auspices to mark the progress of our science or to plan new campaigns for its advancement beyond the existing bounds of knowledge, and upon such occasions it has not been an unknown practice among us to appeal to the social instincts common to civilized men of every vocation and to embrace the opportunities thus presented for the development of personal friendships and the formation of a professional *esprit de corps*. With such memories in mind your Chairman cannot to-day address himself to the declared purposes of this Congress and to its somewhat unusual accessories without first giving in the name of all American astronomers, absent as well as present, a cordial greeting to our distinguished colleagues from beyond the sea, who are to-day with us as members of the Congress.

"The administrative body charged with the organization of these congresses has planned them along unique lines, with emphasis placed in special manner upon the unity of knowledge, the interrelations of those several provinces of learning now grown so extensive that no man may, with reason, aspire to thorough acquaintance with more than one or two. That such relations exist, and that they are of fundamental importance to science as well as to philosophy, none can doubt. That they can be profitably presented at an assemblage of international congresses and be there amplified and emphasized with needful cogency and clearness is in part our function to determine, and the major burden of the task must rest with those gentlemen who have been especially invited to present to the Congress papers along the lines thus indicated."

FUNDAMENTAL CONCEPTIONS AND METHODS IN ASTRONOMICAL SCIENCE

BY LEWIS BOSS

[**Lewis Boss,** Director of Dudley Observatory, Albany, N. Y. Professor of Astronomy, Union University, Albany, N. Y. **b.** Providence, R. I., October 26, 1846. A. B. Dartmouth, 1870; A.M. *ibid.* 1877; LL.D. Union College, 1902. Chief Civilian Astronomer, United States Northern Boundary Commission, 1872–76; Superintendent of Weights and Measures, State of New York, 1883. Member of National Academy of Sciences; Foreign Associate of Royal Astronomical Society, London; British Association for the Advancement of Science; Astronomische Gesellschaft. **Author of** *Declinations of* 500 *Stars; Appendix H, Report of the U. S. Northern Boundary Commission; Catalogue of* 8241 *Stars; Positions and Motions of* 627 *Standard Stars;* and various memoirs and shorter articles.]

Astronomical research has put the world in possession of a wide range of specific knowledge. This concerns a class of phenomena outside the ordinary field of human experience. Astronomy seems to be fairly entitled to another kind of recognition. It was the pioneer in scientific method. It was the first to appreciate fully the logic of mathematical analysis, and to stimulate its development. In pursuit of its characteristic aim to compare hypotheses with observed facts, it brought into prominence this intellectual habit, subsequently employed in the development of all branches of exact science. This aspect of astronomy, as an intellectual pursuit, may properly claim our special attention at the present time.

The full and distinct conception of the material universe as a mechanism operating under the dominion of natural laws that are simple and inflexible in their application is the product of later scientific induction. From the first the investigating astronomer must have apprehended some glimmerings of such a conception. This impelled him to submit this idea to the test of exact examination. But before attempting to trace the development and consequence of this impulse let us notice some of the circumstances that environ the work of the practical investigator in astronomy.

In dealing with the celestial bodies we are hampered not only by the fact of the great distances at which they are situated but also by the fact that there is nothing in terrestrial experience that offers an adequate analogy to some of the phenomena that are observed. The handicap of distance has been somewhat reduced through the invention of the telescope. But the original unfamiliarity of conditions involved in the idea of bodies moving along closed orbits in space, without visible attachment to any support, produced a mental shock requiring time for adaptation of the mind to a set of new conceptions. Even now the physical conditions that we observe in celestial bodies, extremely unlike the earth in structure and temperature, present a most serious obstacle to the application of correct principles of reasoning.

Notwithstanding the strangeness of the phenomena with which they have had to deal, it is worthy of remark that practical investigators in astronomy have indulged sparingly in speculations about things which cannot be firmly apprehended through our perceptions, or which have no adequate analogies in human experience. They have shown little inclination to dwell on such questions as that of the probable limits of the universe; whether the planets are inhabited; what gravitation really is; whether there are burned-out suns in space and few or many of them; what is implied in the motions of the stars, as to the past and future of the visible universe; and many other points of a similar nature. These are matters of high philosophic interest and legitimate subjects for speculation; but they are not yet fully ripe for treatment according to the logical methods of scientific research; and therefore the astronomer usually refers to them in his private, rather than in his professional capacity.

The charm of astronomical work seems partly to reside in the fact that the senses, aided or unaided by mechanical appliances, are not directly sufficient to provide needful material of investigation without important aid in the art of interpretation. That the hypotheses and theories to be inferred from observations are not immediately suggested by customary modes of thought is another attractive feature of the work.

In the astronomy of motion the question at issue is not always whether a certain thing can be seen, or not seen; it more often turns on probability of evidence. A small planet appears exactly like the stars and is primarily distinguished from them only by its motion. The object suspected to be a planet either distinctly appears to move relatively to surrounding stars, or it does not so appear. Decision relative to that fact determines the question, which is not strictly quantitative. On the other hand, does the annual motion of the earth in its orbit cause an apparent shifting from side to side of nearer stars relatively to those more distant? It should do this if the earth really moves and the stars are not so distant as to render this apparent shifting, or annual parallax, inappreciable. The largest annual parallax thus far detected for any star is less than one second; and in only a few instances does it amount to one tenth of this quantity. To measure the tenth of a second on the sky, with absolute certainty, in a single set of operations on any one night, is undoubtedly beyond the present possibilities of any means now employed. The question then arises whether, by multiplying observations and varying the circumstances of measurement, the casual and systematic errors of observation can be so far eliminated through compensation of positive and negative errors that the small quantity actually sought will emerge in the final average. This will be a question of mathematical probability; and it is not always easy to determine what the exact measure of this probability is. Yet, if we could not measure the parallaxes of the stars, astronomy would lack one very important link in the chain of evidence by which the reality of the earth's motion is firmly demonstrated. Furthermore, if the parallaxes of the stars, and consequently their distances, could not be measured with a fair approximation to the real quantities, then a most important element in the foundation of stellar astronomy would be wanting. Consequently astronomers are obliged to measure these quantities as well as they can, and they must push their methods and efforts to extremity.

In short, astronomers are continually obliged to measure and reason about quantities which cannot be distinctly perceived in the telescope. Accordingly, from the necessities of the case, there has arisen in modern astronomy what is almost a distinct science — that of measurement — which absorbs a very important part of the total energy expended in astronomical research. This is carried so far, in some instances, that the astronomer sometimes seems, in a measure, to lose sight of the natural phenomenon to be observed and to be quite wholly absorbed in the means by which he observes it.

The nature and necessities of modern scientific research have brought about what, for the want of a better term, might be called the astronomy of the unseen. The companion of Algol has never been seen and never can be seen, yet it is known to exist and even its dimensions and mass are known with a fair degree of probability. So far as this idea concerns apparent motion on the face of the sky, its means of perception are found in multiplication of measurements; in variation of the methods and circumstances of those measurements; and in interpretation of results. One phase of these processes is well illustrated in researches upon the solar parallax. For its determination there are at least four distinct types of investigation, each involving the application of more than one method. The discordance in angle between the results from each of these four sources of determination is too small to be perceived by the aid of the most powerful telescope. In many other instances, as in the variation of the earth's axis of rotation from its axis of figure, astronomy successfully reasons about quantities which are actually too small to be perceived immediately through any aids of the senses that are available. The logic of this achievement rests upon the validity of the practical application of the mathematical theory of probabilities to the treatment of residual phenomena. The outcome is that the objective reality of things which we cannot see may be as firmly established as that of things which we can see. The entire process is essentially an intellectual one in which the testimony of the senses plays only an indirect part.

The more recent developments in all physical science illustrate this tendency to extend the field of knowledge beyond that which can be directly apprehended by sense-perception. This kind of investigation undoubtedly results in a peculiar intellectual satisfaction; it extends indefinitely the territorial domain of the mind; and it arouses the consciousness that man possesses intellectual powers of a capacity to learn the things of the external world to which no one can set definite limits.

An increasingly troublesome feature of astronomical research is found in the unwieldy nature of some of the problems which it presents. The work of developing and applying the theory of a single planet is very great in itself; but the nature of the problem now requires for it its sufficient treatment that the entire family of eight major planets shall be considered together. The full mathematical development of the lunar theory could scarcely be compassed in the professional life of one astronomer. It is still more difficult to see how the problem of structure and motion in the stellar universe is to be effectively handled in the future without systematic organization of the forces concerned and without controlled division of labor. These facts appear to suggest that the science of astronomy differs somewhat from the generality of other branches of exact science in this respect: that in working out some of its single and really integral problems an unusually extensive combination of effort is required in order to arrive at the result. This detracts from the personal glory of the individual who is often able to investigate only one section of an essentially integral problem. But this fact does not appear to deter the investigator in all cases, nor to lessen his enthusiasm.

The single investigator in astronomy is not permitted to devise and select crucial experiments (perhaps only one) by means of which an hypothesis can be sustained, or overthrown, at once. Any astronomical hypothesis which is at all of a fundamental character must usually be discussed through induction from a great multitude of observed facts, provided by numerous observers, in relation to whom the relative value of their testimony must of itself be partly a matter of induction.

It may be worth while to consider for a moment the nature of the scientific truth which the astronomer endeavors to ascertain. There seems to be no occasion to enter upon a philosophical disquisition concerning the real essence of truth and the possibility of objective reality. The astronomer proceeds exactly as if the objective world were real; but he recognizes that the truths which he is able to ascertain concerning it are of a relative character. What appears to be most feasible and fruitful in astronomical research is the attempt to coördinate diverse facts under a general formula, rule, or conception, by means of which we can apprehend all the facts as parts of a single entity. The value of this process of coördination is realized in two ways. First, it is the means of referring otherwise disconnected phenomena to a common origin. This is accomplished by means of a formula which serves to connect them as parts of a greater whole. Such a formula can then be grasped and brought into one field of mental view. Secondly, this coördination may identify some fact, or principle, in nature that not only serves to connect a group of observed phenomena but itself also represents a real fact in nature. The history of astronomy demonstrates that the investigator is not deterred from an effort to gain possession of the element of value first mentioned by any failure to grasp the second element. This is not because the discovery of truths in nature, that have a real existence, is not regarded as the most valuable reward of research, but because experience has shown that the invention of a satisfactory formulation of observed phenomena ultimately leads to the discovery of things which may be regarded as objectively real. In this sense we may say that every successful representation of observed facts through the adoption of a formulated conception results in scientific truth. Examination of the manner in which astronomical research has dealt with celestial motion will illustrate this idea.

Until recent years the investigation of apparent celestial motion has been by far the most important occupation of the astronomer. Bessel defined it as essentially constituting the science of astronomy.

We see the celestial motions as they are projected on the apparent surface of the celestial sphere. We cannot vary our point of view at will in order to see these motions in space of three dimensions. From our immediate perceptions we are wholly unable to form any reliable opinion as to what they are really like, or as to the relative distances separating the earth from celestial objects. The distances must be derived from induction along with the other circumstances defining the motions. Furthermore, the observer himself is in motion. His motion is complicated. Enumerating only the most important, we have, first, the motion of diurnal rotation; secondly, the annual revolution of the earth around the sun as a centre; thirdly, a rapid translatory motion through space of the earth along with the sun. All these motions produce apparent motions of the celestial bodies on the face of the sky; and these motions must be disentangled from those which properly belong to those bodies. The first investigators were in a specially unfavorable situation for successful research. They were not even aware of the motions of the earth and sun. All experience supported the impression of stability for the earth. It was actually necessary to overthrow the direct testimony of the senses before the system of celestial motions could be conceived as it actually exists. Herein astronomy ultimately accomplished a great service to the development of knowledge in demonstrating the important function of the intellectual process through which the direct impressions of perceptions should be interpreted.

Hipparchus, the first great investigator in astronomy, saw that the solution of the problem of planetary motions must be approached through the invention of simple geometrical conceptions by the mathematical consequences of which the observed motions could be represented. He found that the apparent motion of the sun in a plane inclined to the celestial equator could be represented as arising from a uniform circular motion of the sun around a centre outside the earth. He deduced the nominal orbit of the sun in this way and verified the result by showing that this hypothesis represented his observed positions of the sun, as he saw it on the sky, within the possible errors of his observations. This is modern scientific investigation complete at every point. There is every reason to believe that Hipparchus consciously employed his invention as an hypothesis without insisting strongly on its objective reality. His discovery is in the nature of an intellectual truth which was the seed from which subsequent knowledge of the solar system developed.

Acting on the idea suggested in the work of Hipparchus, Ptolemy extended the hypothesis of uniform circular motion to include motion in an epicycle; that is to say, he conceived a planet to be revolving uniformly on the circumference of a circle, the centre of which was also revolving uniformly on the circumference of another circle. Though he added other mechanism to his scheme, this of uniform motion in eccentrics and epicycles was the fundamental notion. We know from Ptolemy's account of his labors, that he regarded his hypothesis mainly as a computing device, — a geometrical conception which could be successfully applied in the representation of apparent planetary motions, as the astronomers of that age saw them; he was perfectly aware of the fact that the sun could be employed as the centre of reference for planetary motion. But that idea was repugnant to human experience; since it involved the consequence that the massive earth must turn upside down every twenty-four hours; and that this giant body of matter, toward the centre of which all bodies were believed to tend, must become tributary to the sun, then supposed to be a body of fire, at that time classified as the lightest of elements. The astonishing facts of the planetary system already known at that time invested them with a degree of mystery which rendered it next to impossible to regard the substance of which they are composed, or the means by which their actual motions are maintained, as having any credible analogies in terrestrial experience.

In view of all the circumstances, it seems reasonable to agree with the view of Delambre that Ptolemy, in the light of his time, would not have been justified in introducing into his work an hypothesis which then seemed contrary to all human experience. His work remained a pure geometric conception designed to serve the practical end of grouping under one idea the representation of observed celestial motions. From this procedure continued, the truth would be sure to emerge.

At its inception the invention of the heliocentric hypothesis by Copernicus was virtually no more than a development and improvement upon the fundamental conception of Ptolemy. The principle of uniform circular motion was retained. The removal of the centre of reference from the earth to the sun was the characteristic feature of this hypothesis, and this was a distinct improvement in the geometric ideal. From the standpoint of the philosopher of that time it could be regarded as no more than this. In defending his new system Copernicus was not able to advance a single reason more convincing than that due to the simplification of planetary computations thus brought about. The telescopic discoveries of Galileo were yet to be made. Nothing was then known of the laws of motion, or of gravitation. The annual apparent motion of stars upon the sky due to reflex effects of the motion of the earth was then unknown and could not have been ascertained by any means of measurement then available. Yet all these sources of proof, and more, are now needed for the establishment of the theory of the earth's motion on a really sound philosophic basis.

We may, then, fairly denominate the invention of Copernicus as a step in astronomical development not greatly different in its inner philosophical quality from the steps previously introduced by Hipparchus and Ptolemy. It resulted in improved representation of planetary motion, and brought the astronomer one long stage nearer his goal, — the search for a demonstrable, objective reality as the basis of planetary motion.

The new system, besides admitting of greater simplicity and perfection in the computations, lent an appearance of reality to representation of the relative distances of planets from the earth in successive intervals of time. This was of capital advantage to Kepler in his research. The excellent observations of Tycho Brahe had convinced Tycho and Kepler that the existing hypothesis of planetary motion, as resulting from compounded uniform circular motions, was no longer tenable even as a device of computation. Yet it should be observed that the existing planetary tables available for the criticism of Kepler served him a most useful purpose in his approximations toward the true elliptical forms of planetary orbits and the equable description of areas. The desire to picture celestial motions as they actually take place in space, and to account in that way for the apparent trace of those motions on the sky, as actually observed, appears to have been more marked in the work of Kepler than in that of any investigator up to that time. But beyond vague conjectures, Kepler did not try to form any theory to coördinate under a more general concept, the facts, or laws, of planetary motion that he had discovered. This task was reserved for Newton.

It is well known that surmises in relation to a hypothetical attraction emanating from the sun and acting on the planets were more or less vaguely entertained by Kepler, Huyghens, Hooke, Halley, and others in the seventeenth century. They had even conjectured that this attraction might vary inversely as the square of the distance. Hitherto, astronomy had been a formal science, — an attempt merely to define the motions which actually take place. But here we see evidence of a desire to refer these motions to some antecedent cause, — a veritable physical origin of them. In fact, the most significant feature of Newton's work is in his discovery that the law of planetary attraction is none other than the terrestrial attraction that acts on bodies at the surface of the earth.

But as soon as the theory of universal gravitation could be regarded as sufficiently established to warrant extensive labor in its application, the normal course of astronomical research was resumed. The application of the principle of gravitation to represent the deviations of the orbits of the planets from the exact elliptical form, the numerous inequalities in the motion of the moon, the phenomena of planetary satellites, the polar flattening of the earth and planets, was similar in spirit to the attempt to represent the motions of the planets through the geometrical conception of compounded, uniform, circular motion. But it is also true that a greatly increased interest attached to this succession of researches, — an interest of broad philosophical scope. This interest is at least twofold.

In the first place, the representation of the apparent celestial motions through the application of gravitational theorems not only possesses the advantage of coördinating observations in the best possible manner, — a thing which previously constituted the whole business of astronomy, — but all this work also tends to the firm establishment of a fundamental postulate by means of which not only the motion of one planet, but also all the motions (strictly speaking, the accelerations) of all the planets, satellites, comets, and meteors, could be interpreted and referred to one antecedent cause.

In the second place, the working out of the consequences of the formula of gravitation emphasized a criterion that is extremely important in estimating weight of evidence in relation to any synthesis concerning natural phenomena. The truth of a theory is measured not simply by success in representing the facts on which it is based. The real test comes in the representation of facts not considered in the original establishment of the theory. Especially satisfactory is it, if the new facts, used as a test, differ in their special nature from those relied on in the original construction of the theory.

Thus the theory of gravitation not only accounted for known inequalities in the motion of the moon, but it was also the means of pointing out the probable existence of other inequalities that were subsequently verified by observation. Not only were the perturbations of existing planets explained, but a hitherto unknown planet was detected through its perturbations exerted on a known planet, and was subsequently found to be actually visible in the sky. The polar compression of the earth was predicted as a consequence of gravitation before measurement was employed to verify the fact. Not only was the precession of the equinox shown to be a consequence of this theory, but the analogous nutation of the earth's axis was discovered and formulated from that theory in advance of the ability of observers to detect the minute apparent motions of the stars which are traceable to this cause. This list might be indefinitely extended, but it seems already sufficient to point out the most valuable element in verification of natural law.

The astronomy of the stars is now in much the same relative situation as that occupied by planetary astronomy two thousand years ago. The problems that confront it must be worked out by the historic method of temporarily assuming simple geometrical conceptions that may be suspected to underlie and connect the diverse facts of observation. The Greek school of astronomy was preceded by a long period in which classification of observed phenomena marked the limit of attainment. The circles of reference for the sphere were invented and the facts of diurnal rotation noted; the path of the zodiac was marked out; the recurrence of eclipses was studied; and the length of the year was approximately determined. Thus a large stock of conventional ideas was accumulated; and these proved useful to the more exact and ingoing research upon which the Greek school of astronomers entered.

A similar accumulation of classified facts and conventional ideas is going on now in the interest of stellar astronomy. First, and most important of all, we are in possession of the results of a very large expenditure of skill and energy in accumulating observed positions of stars at various epochs from 1755 to the present time. As the time seems to be drawing near when parts of the stellar problem may be accessible to actual research with a good hope of results, both the skill and energy devoted to observations of stellar positions, from which the facts of stellar motion can be derived, is on the increase. The effects of certain stellar motions which are merely apparent have been formulated, and the greater part of these effects can now be disentangled from the observations. Statistical researches concerning the distribution of stars suggest valuable hypotheses to be subsequently tested through investigation founded on the observed motions of the stars. The problem of finding a fixed line of reference in direction that can be identically recovered at any future time, so far as this can be accomplished under its necessary limitations, has received a marked degree of attention, and will receive much more.

In fact, the beginnings of actual research upon the problem itself have been made in the discovery of the sun's motion of translation in space. This discovery, with the operations that have established it, is roughly analogous to the work of Hipparchus in determining a nominal orbit of the sun. The solar motion appears likely to furnish us with the base-line needed in stellar investigation; and this naturally becomes an object of close attention until the outline of facts-regarding it shall be well determined. The sun is laying down this base-line at an annual rate of distance which is probably from four to six times as great as that which separates the earth from the sun. It is evident that the length of this base-line rapidly accumulates with time. Already a sufficient length of it has been paid out, so that we see the nearer stars from a distinctly different point of view. We are even now able to determine the relative mean distances from us of stars classified in groups. It is even now almost possible to execute the triangulation from this base by means of which in connection with spectroscopic measures of radial motion, we may determine the distances of individual stars. The facility with which this can be done will increase in a ratio more rapid than that of the increased lapse of time. Within a century from now we may anticipate that astronomers will begin to see the stars in space of three dimensions, and that this ability to see them thus will, thereafter, rapidly become more clear and undoubted.

There is now at the disposal of research in stellar astronomy an accumulation of carefully measured positions of stars at desired epochs in the past much larger than might have been provided on account of anticipated needs in this special line. The requirements of geodesy and of the astronomy of planets and comets are largely responsible for this. Instead of a supply of observations growing out of the previously declared needs of the stellar problem, we find interest in that problem fostered and stimulated by the opportunity for investigation afforded by observations mainly provided for another purpose. Astronomy is filled with illustrations of the fact that its progress is frequently due to a natural development, rather than to deliberate plans in advance. Thus Bradley, in the eighteenth century, set out to detect the minute apparent motions of stars due to the already predicted effect of nutation. While engaged in this investigation he discovered the effect of aberration; and these two discoveries further led to a complete revolution in the standards of accuracy which it became feasible to prescribe for astronomical measurements. Again, Herschel, systematically measuring the relative direction of one star from another very near it for the purpose of detecting, if possible, any appreciable effect of annual parallax due to the differing distances of the two stars, discovered that many of the so-called double stars really constitute physical systems, since known as binary stars.

From incidents like these has grown a dictum that every recorded measurement of the position of a moving celestial body is important irrespective of any immediate utility of it that may be apparent at the time it is made. It is an inspiring thought that every such measurement records a fact unique in the history of nature. If the opportunity to measure be neglected the omission can never be supplied. Can it be said that any measurement of the position of a moving body is superfluous? Should response to a special need constitute the only recognized motive in making such measurements? The forces at the disposal of astronomical research are finite. The demands of the hour are always great. Masses of measurements for the uses of posterity must usually contain an element of uncertainty as to exactly what may really be needed. Uncertainty as to future improvements in the art of measurement may be such as to raise the question whether present standards are not liable to become obsolete. Therefore, the accumulation of measurements upon the celestial bodies chiefly for the use of posterity is not to be commended without limitations. It would seem that a large element of comparatively immediate utility should inhere in every such undertaking; or it should become very clear that posterity will certainly need, and will be able to profit by, the observations we now make for them.

These limitations do not seem to operate against the extensive scheme of accurate observation upon all stars to the eleventh magnitude, known as the Astrographic Chart. This is an undertaking which, while it has its present uses, will probably offer a much larger measure of utility to astronomers of future generations engaged in stellar research. The first and most important requirement in the solution of this problem is that astronomy shall be in possession of measurements upon as many stars as possible, repeated at various epochs over the longest possible interval. In this way the unavoidable systematic errors of the measurements may be rendered relatively less obnoxious to the total of observed stellar motion, especially where that motion is very small. It seems probable that the measurements from the Astrographic photographs mark very nearly a practical limit of accuracy for operations on a large scale. Accordingly we may be quite certain that no astronomical work of the present generation will be better appreciated by astronomers of later ages than that upon the Astrographic Chart, which will convey to them the first accurately observed positions of the vast majority of stars that it contains.

In this undertaking we have a most striking illustration of an altruistic spirit toward posterity which must be regarded as the finest attribute of a civilization having at heart the collective interests of the race.

In the struggle to secure the extraordinary degree of real accuracy required in the effective treatment of important problems in modern astronomy, the scrutiny to which the fallibility of the senses is subjected is a notable feature. Scarcely a measurement can now be made concerning which a doubt is not interposed as to the possible effect of personal idiosyncracies of the observer, or as to some unrecognized effect due to the instrument employed. Accordingly there has grown up during the last few years the habit of special research with a view either to the determination of the effect of these peculiarities, or to devising the means whereby they may be eliminated.

In modern astronomy even the direct testimony of vision is subjected to a similar doubt. Thus we have the testimony of various observers that they have seen on the surface of Mars certain very faint markings which they call canals. At the same time other investigators declare that these markings may be merely illusions due to optical and mental strain; and they produce ingenious experiments to prove their contention. These facts, and many others that might be cited, illustrate the critical tendencies of modern astronomy which are now somewhat more accentuated than they were in former times.

The present course of astronomy strongly tends toward future development of the power to apprehend and reason about quantities that are too small for direct perception. If this be so, the essentially intellectual character of the processes employed will become still more evident than it has been in the past of astronomical investigation. This modern development that is working in all branches of exact science appears to be of more than temporary significance in the history of the race. Its effect is to enlarge the domain of human experience by discoveries in territory before unknown. This is equivalent to enlarging the world in which we live and adding to its variety. The same significance should attach to this work that would be ascribed to the discovery of a new continent. In effect these quasi-supersensual discoveries introduce us into regions of knowledge which are absolutely new to human experience; and when the number of those who are able to enjoy an intellectual tour in these regions shall become a relatively numerous element of population, this species of additions to knowledge will become still more highly appreciated.

In recent times we have become distinctly conscious that astronomy has enlarged its field of investigation to cover a class of researches not immediately reducible to the study of apparent motion. The development of the spectroscope has brought this about. Some

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might contend that spectroscopic researches upon the physical constitution of the sun and stars are as distinct from mechanical investigations which are concerned with these bodies as units in motion, irrespective of their internal structure, as the science of chemistry is distinct from that of mechanics. A brief examination, however, will show that the respective fields of the two departments of astronomy are so interwoven that they cannot readily be divorced. Through the application of a principle of spectral analysis the conclusions of the older astronomy are not only supplemented, but they are logically strengthened in their standing as concepts having objective reality. For example, the theory that the earth moves in an orbit about the sun, quite recently in the world's history a mere hypothesis, requiring a veritable mountain of demonstration before mankind was really justified in its acceptance, receives support from direct observations of the motion of the earth in its orbit made evident in the investigation of stellar spectra. It may be conceded that, from the ordinary point of view, this added evidence was not actually required. But, from the standpoint of the philosopher, this new evidence cannot be regarded as superfluous. Likewise, the measurement of the motions of planets in the line of sight similarly confirms the theory of the solar system. Measurement of stellar motions in the line of sight, by means of the spectroscope, connects the field of astrophysics indissolubly with the astronomy that deals with thwart motions as seen upon the face of the sky. The two lines of research offer concurrent testimony that is of extremely great philosophic importance in arriving at conceptions regarding motion in the stellar universe. But the logical connection of the two branches of astronomical activity has a more profoundly philosophical basis.

The discovery of the law of gravitation introduced a virtually new object of inquiry to the attention of astronomers. To what extent can likeness be traced in physical laws and circumstances that generally prevail among the celestial bodies, including the earth as one of them? The rotation of the sun on its axis, the revolution of satellites about the planets, and a few other disconnected facts of this kind, had already suggested thoughts as to such likeness. But the discovery of gravitation gave an immense stimulus to this idea. The mere fact that every particle of matter in the solar system attracts every other swept away nearly the last vestige of speculations which predicated an essentially peculiar difference between terrestrial and non-terrestrial matter as to fundamental qualities. Matter in Saturn was found to be heavy in the same sense that matter on the earth is heavy. Not so long ago comets were generally supposed to be emanations of some mysterious substance, — celestial will-o'-the wisps. That idea is now relegated to the limbo of forgotten things; and the essential likeness of comets to other celestial bodies, in substance and in obedience to gravitation, is now generally understood. Later on, discoveries concerning the revolutions of the components of binary stars in elliptical orbits extended the hypothesis of likeness throughout the stellar universe. The discovery that the sun, like all the stars, is moving through space approximately in a straight line, at a velocity comparable with the velocities of motion among the stars, irresistibly led to the conclusion that, from the viewpoint of any star, the sun would be seen as a small star differing in no discoverable respect from others of the same brightness. These were a few of the points suggesting essential unity of law and matter throughout the universe. Such ideas are removed by a great gulf from those which prevailed at the beginning of the Christian Era.

The introduction of researches in astrophysics raised this line of induction to a new plane of logical perfection. It proved that the physical condition of the stars, broadly considered, resembles that of the sun. It showed that the radiance of self-luminous bodies is attributable to the same cause which is needed for a similar effect at the surface of the earth. Moreover, it showed that there is remarkable likeness in the chemical constitution of various bodies throughout the visible universe, — specifically, that hydrogen, iron, and other elements are quite universally constituents of the most widely separated of the celestial bodies. The chemical elements existing in the sun were found to be very nearly identical with those found on the earth; and what is more remarkable, the progress of chemical investigation of terrestrial elements served to increase the evidence of apparent likeness in chemical constitution between the sun and the earth.

Thus, from a speculation, carefully guarded with limitations in its expression, the idea of essential likeness of natural phenomena in their operations throughout the universe was developed into a theorem which it would now seem childish to doubt. This conclusion is the joint product of the two branches of astronomical science. In this special line astrophysics, the newer branch, has borne a very important part; and it would seem that, in a future of great promise, its share must be still more conspicuous.

Summing up, now, the results of human experience in the history of astronomical investigation, we trace the influence of two intellectual conceptions which appear to have inspired all that has been done. The first is, that all the observed phenomena of motion can be referred to fundamentally simple geometrical ideals as to their real origin. The second is that the essential qualities of matter and the operations of nature throughout the universe are everywhere virtually the same.

THE LIGHT OF THE STARS

BY EDWARD CHARLES PICKERING

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If an intelligent observer should see the stars for the first time, two of their properties would impress him as subjects for careful study, — first, the irrelative positions, and secondly, the irrelative brightness. From the first of these has arisen the astronomy of position, or astrometry. This is sometimes called the Old Astronomy, since until within the last twenty years the astronomers of the world, with few exceptions, devoted their attention almost entirely to it. To the measure of the light should be added the study of the color of the stars (still in its infancy), and the study of their composition, by means of the spectroscope. In this way a young giant has been reared, which has almost dwarfed its older brothers. The science of astrophysics, or the New Astronomy, has thus been developed, which during the last few years has rejuvenated the science and given to it, by its brilliant discoveries, a public interest which could not otherwise have been awakened. The application to stellar astronomy of the daguerreotype in 1850, of the photograph in 1857, and of the dry plate in 1882, has opened new fields in almost every department of this science. In some, as in stellar spectroscopy, it has almost completely replaced visual observations.

One department of the New Astronomy, the relative brightness of the stars, is as old as, or older than, the Old Astronomy. An astronomer even now might do useful work in this department without any instruments whatever. Hipparchus is known to have made a catalogue of the stars about 150 B. C. Ptolemy, in 138 A. D., issued that great work, the *Almagest,* which for fourteen hundred years constituted the principal and almost the sole authority in astronomy. It contained a catalogue of 1028 stars, perhaps based on that of Hipparchus. Ptolemy used a scale of stellar magnitudes which has continued in use to the present day. He called the brightest stars in the sky the first magnitude, the faintest visible to the naked eye, the sixth. More strictly, he used the first six letters of the Greek alphabet for this purpose. But he went a step further, and subdivided these classes. If a star seemed bright for its class, he added the letter μ (mu), standing for μείζων (meizon), large or bright, if the star was faint he added ε (epsilon), standing for ἐλάσσων (elasson), small or faint. These estimates were presumably carefully made, and if we had them now, they would be of the greatest value in determining the secular changes, if any, in the light of the stars. The earliest copy we have of the *Almagest* is no. 2389 of the collection in the Bibliothèque Nationale of Paris. It is a beautiful manuscript, written in the uncial characters of the ninth century. A few years ago it could be seen by any one in one of the show-cases of the library. There are many later manuscripts and printed editions which have been compared by various students. The errors in these various copies are so numerous that there is an uncertainty in the position, magnitude, or identification of about two thirds of the stars. A most important revision was made by the Persian astronomer, Abd-al-rahman al-Sufi, who re-observed Ptolemy's stars, A. D. 964, and noted the cases in which he found a difference. The careful study and translation of this work from Arabic into French by Schjellerup has rendered it readily accessible to modern readers.

No important addition to our knowledge of the light of the stars was made until the time of Sir William Herschel, the greatest of modern observers. He found that when two stars were nearly equal, the difference could be estimated very accurately. He designated these intervals by points of punctuation, a period denoting equality, a comma a very small interval, and a dash a larger interval. In 1796 to 1799, he published in the *Philosophical Transactions* four catalogues covering two thirds of the portion of the sky visible in England. Nearly a century later, it was my great good fortune, when visiting his grandson, to discover in the family library the two catalogues required to complete this work, and which had not been known to exist. These two catalogues are still unpublished. Meanwhile, little or no use had been made of the four published catalogues which, while comparing one star with another, furnished no means of reducing all to one system of magnitudes. The Harvard measures permitted me to do this for all six catalogues, and thus enabled me to publish magnitudes for 2785 stars observed a century ago, with an accuracy nearly comparable with the best work of the present time. For nearly half a century no great advance was made, and no astronomer was wise enough to see how valuable a work he could do by merely repeating the observations of Herschel. Had this work been extended to the southern stars, and repeated every ten years, our knowledge of the constancy of the light of the stars would have been greatly increased. In 1844, Argelander proposed, in studying variable stars, to estimate small intervals, modifying the method of Herschel by using numbers instead of points of punctuation, and thus developed the method known by his name. This is now the best method of determining the light of the stars, when only the naked eye or a telescope is available, and much valuable work might be done by applying it to the fainter stars, and especially to clusters.

Meanwhile photometric measures of the stars according to various methods had been undertaken. In 1856, Pogson showed that the scale of magnitudes of Ptolemy, which is still in use, could be nearly represented by assuming the unit to be the constant ratio, 2.512, whose logarithm is 0.4. This has been generally adopted as the basis of the standard photometric scale. The photometer devised by Zöllner has been more widely used than any other. In this instrument, an artificial star is reduced any desired amount, by polarized light, until it appears to equal the real star, both being seen side by side in the telescope. Work with this instrument has attained its greatest perfection at the Potsdam Observatory, where measures of the light of the northern stars, whose magnitude is 7.5 and brighter, have been in progress since 1886. The resulting magnitudes have been published for 12,046 stars, included in declination between −2° and +60°. The accidental errors are extremely small, but as the results of different catalogues differ systematically from one another, we cannot be sure which is right and what is the real accuracy attained in each case. In 1885, the *Uranometria Oxoniensis* was published. It gives the magnitudes of 2784 northern stars, north of declination −10°. This work is a remarkable one, especially as its author, Professor Pritchard, began his astronomical career at the age of sixty-three. The method he employed was that of reducing the light of the stars by means of a wedge of shade glass until they became invisible, and then determining the brightness from the position of the wedge. A careful and laborious investigation, extending over many years, has been carried on by Mr. H. M. Parkhurst, using a modification of this method.

For several years before the Oxford and Potsdam measures described above were undertaken, photometric observations were in progress at Harvard. In 1877, a large number of comparisons of adjacent stars were made with a polarizing photometer. Two images of each star were formed with a double-image prism, and the relative brightness was varied by turning a Nicol prism until the ordinary image of one star appeared equal to the extraordinary image of the other. Several important sources of error were detected, which, once known, were easily eliminated. A bright star will greatly affect the apparent brightness of an adjacent faint one, the error often exceeding a magnitude. Systematic errors amounting to several tenths of a magnitude depend upon the relative positions of the images compared. They are perhaps due to the varying sensitiveness of the different parts of the retina. This photometer has many important advantages. However bad the images may be, they are always exactly alike, and may, therefore, be compared with accuracy. Both stars are affected equally by passing clouds, so that this photometer may be used whenever the stars are visible, and at times when other photometric work is impossible. The diminution in light also follows a simple geometrical law, and is readily computed with great accuracy. There is no unknown constant to be determined, as in the Pritchard, and nearly all other photometers. The principal objections to this instrument are, first, that stars cannot be compared unless they are near together, and, secondly, that faint stars cannot be measured, since one half of the light is lost by polarization. The principal uses so far made of this form of photometer are in comparing the components of double stars, and in a long series of observations of the eclipses of Jupiter's satellites, which now extends over a quarter of a century and includes 768 eclipses. Instead of observing the time of disappearance, a series of measurements is made, which gives a light-curve for each eclipse. Much important work might yet be done with this form of photometer, in measuring the components of doubles and of clusters, and determining the light-curves of variables which have a moderately bright star near them.

An important improvement was made in this form of photometer in 1892, by which stars as much as half a degree apart could be compared. The cones of light of two such stars are brought together by achromatic prisms, so that they can be compared as in the preceding instrument. As there is no part of the sky in which a suitable comparison star cannot be found within this distance, any star may be measured with this instrument. In the hands of Professor Wendell this photometer has given results of remarkable precision. The average deviation of the result of a set of sixteen settings is about three hundredths of a magnitude. Light-curves of variables can therefore be determined with great precision, and suspected variables can be divided into those that are certainly variable, and those whose changes are probably less than a tenth of a magnitude.

Another change in this instrument produced the meridian photometer. Instead of using the two cones from one object-glass, two object-glasses were used, mirrors being placed in front of each. In this way, stars however distant can be compared. In theory, this instrument leaves but little to be desired. Almost every source of error that can be suggested can be eliminated by proper reversions. As constructed, the telescope is placed horizontally, pointing east or west. One mirror reflects a star near the pole into the field, the other, a star upon the meridian. A slight motion of the mirror permits stars to be observed for several minutes before or after culmination. The first meridian photometer had objectives of only two inches aperture. With this instrument, 94,476 measures were made of 4260 stars, during the years 1879 to 1882. All stars were included of the sixth magnitude and brighter, and north of declination −30°.

The second instrument had objectives of four inches aperture, and permitted stars as faint as the tenth magnitude to be measured. With this instrument, during the years 1882 to 1888, 267,092 measures were made of 20,982 stars, including all the catalogue stars and all the stars of the ninth magnitude and brighter, in zones twenty minutes wide, and at intervals of five degrees, from the north pole to declination −20°. In 1889, the instrument was sent to South America, where 98,744 measures were made of 7922 southern stars, extending the two preceding researches to the south pole. On the return of the instrument to Cambridge 473,216 measures were made of 29,587 stars, including all those of the magnitude 7.5 and brighter, north of declination −30°. This work occupied the years 1891 to 1898. The instrument was again sent to Peru in 1899, and 50,816 measures were made of 5332 stars, including all those of the seventh magnitude and brighter, south of declination −30°. The latest research has been the measurement of a series of stars of about the fifth magnitude, one in each of a series of regions ten degrees square. Each of these stars is measured with the greatest care on ten nights. This work has been completed and published for stars north of declination −30°, 59,428 measures having been made of 839 stars. In this count, numerous other stars have been included. Similar measures are now in progress of the southern stars, this being the third time the meridian photometer has been sent to South America. The total number of measurements exceeds a million, and the number of stars is about sixty thousand. About sixty stars can be identified with care, and each measured four times with this instrument in an hour. The probable error of a set of four settings is ±0.08.

The principal objection to the instrument just described is the great loss of light. To measure very faint stars, another type of photometer has been devised. A 12-inch telescope has been mounted horizontally, like the meridian photometer, and an artificial star reflected into the field. The light of this star is reduced by a wedge of shade glass, until it appears equal to the star to be measured. Four hundred thousand measures have been made with this instrument during the last five years. The principal research has been the measurement of all the stars in the Bonn *Durchmusterung*, which are contained in zones ten minutes wide and at intervals of five degrees, from the north pole to declination −20°. Large numbers of stars of the tenth and eleventh magnitudes are thus furnished as standards of light. As the light of the object observed is unobstructed, any star however faint, if visible in the telescope, may be measured. Accordingly, many stars of the twelfth and thirteenth magnitudes have been selected and measured, thus furnishing faint standards. Sequences of standard stars have been selected from coarse clusters, thus permitting estimates or measures of these bodies to be reduced to a uniform photometric scale. An investigation of great value has been carried out successfully at the Georgetown College Observatory, by the Rev. J. G. Hagen, S. J. All the stars of the thirteenth magnitude and brighter have been catalogued and chartered, in a series of regions, each one degree square, surrounding variable stars of long period. Besides measuring the positions, he has determined the relative brightness of these stars. A sequence has then been selected from each of these regions, and measured at Harvard with the 12-inch meridian photometer, thus permitting all to be reduced to a uniform scale. As the photometer was first constructed, stars brighter than the seventh magnitude could not be measured, since they were brighter than the artificial star, and could not be rendered equal to it. This difficulty was remedied by inserting a series of shades, the densest of which reduced the light by ten magnitudes. By this method, the range of the photometer may be increased indefinitely. Sirius and stars of the twelfth magnitude have been satisfactorily measured in succession. A further modification of the instrument permitted surfaces to be compared. The light of the sky at night and in the daytime, during twilight, at different distances from the moon, and different portions of the disk of the latter, have thus been compared. Measures extending over seventeen magnitudes, with an average deviation of about three hundredths of a magnitude, were obtained in this way. One light was thus compared with another six million times as bright as itself. A slight modification would permit the intrinsic brightness of the different portions of the sun's disk to be compared with that of the faintest nebulæ visible. By these instruments, the scale of photometric magnitudes has been carried as far as the thirteenth magnitude. To provide standards for fainter stars, a small appropriation was made by the Rumford Committee of the American Academy. Coöperation was secured among the Directors of the Yerkes, Lick, McCormick, Halsted, and Harvard Observatories. Similar photometers were constructed for all, in which an artificial star was reduced any desired amount by a photographic wedge. Telescopes of 40, 36, 26, 23, and 15 inches aperture, including the two largest refractors in the world, were thus used in the same way on the same research. The standards have all been selected, and nearly all of the measurements have been made. This furnishes a striking illustration of the advantages of coöperation, and combined organization. When these observations are reduced, we shall have standards of magnitude according to a uniform scale for all stars from the brightest to the faintest visible in the largest telescopes at present in use. The 60-inch reflector of the late A. A. Common has recently been secured by the Harvard Observatory. It is hoped that still fainter stars may be measured with this instrument.

We have as yet only considered the total light of a star, so far as it affects the eye. But this light consists of rays of many different wave-lengths. In red stars, one color predominates, in blue, another. The true method is to compare the light of a given wave-length in different stars, and then to determine the relative intensity of the rays of different wave-lengths in different stars, or at least in stars whose spectra are of different types. This is the only true method, and fortunately spectrum photography permits it to be done. The Draper Catalogue gives the class of spectrum of 10,351 stars, and the relative brightness of the light whose wave-length is 430 is determined for each. In 1891, measures were published of the relative light of rays of various wave-lengths, for a number of stars whose spectra were of the first, second, and third types.

A much simpler but less satisfactory method is to measure the total light in a photographic image. As in the case of eye-photometry, this method is open to the objection that rays of different colors are combined. Blue stars will appear relatively brighter, and red stars relatively fainter, in the photograph than to the eye. This, however, is an advantage rather than an objection, since it appears to furnish the best practical measure of the color of the stars. Relative photographic magnitudes can be obtained in a variety of ways, and the real difficulty is to reduce them to an absolute scale of magnitudes. But for this, photographic might supersede photometric magnitudes. In other respects, photography possesses all the advantages for this work that it has for other purposes, and many photometric problems are within the reach of photography, which seem hopeless by visual methods. In 1857, Professor George P. Bond, the father of stellar photography, showed that the relative light of the stars could be determined from the diameter of their photographic images. This is the method that has been generally adopted elsewhere in determining photographic magnitudes, although with results that are far from satisfactory. It is singular that although this method originated at Harvard, it is almost the only one not in use here, while a great variety of other methods have been applied to many thousands of stars, during the last eighteen years. Relative measures are obtained very satisfactorily by applying the Herschel-Argelander method to photographic images, and if these could be reduced to absolute magnitudes, it would leave but little to be desired. In the attempt to determine absolute magnitudes a variety of methods has been employed. The simplest is to form a scale by photographing a series of images, using different exposures. The image of any star may be compared directly with such a scale. To avoid the uncertain correction due to the time of exposure, different apertures may be used instead of different exposures. Another method is to attach a small prism to the objective. The image of every bright star is then accompanied by a second image a few minutes of arc distant from it, and fainter by a constant amount, as five magnitudes. Trails may be measured more accurately than circular images, and trails of stars near the pole have varying velocities, which may then be compared with one another by means of a scale. Again, images out of focus may be compared with great accuracy and rapidity by means of a photographic wedge. These comparisons promise to furnish excellent magnitudes, if they can only be reduced to the photometric scale. A catalogue giving the photographic magnitudes of 1131 stars within two degrees of the equator, and determined from their trails, was published in 1889. Great care was taken to eliminate errors due to right ascension, so that standards in remote portions of the sky are comparable. A similar work on polar stars at upper and lower culmination determined the photographic absorption of the atmosphere, which is nearly twice as great as the visual absorption. A catalogue of forty thousand stars of the tenth magnitude, one in each square degree, has been undertaken, and the measures are nearly complete for the portion of the sky extending from the equator to declination + 30°. These stars are compared, by means of a scale, with the prismatic companions of adjacent bright stars. Two measures have been made of images out of focus of 8489 stars, including all of those north of declination − 20°, and brighter than the seventh magnitude. This work is being continued to the south pole. The most important completed catalogue of photographic magnitudes is the *Cape Photographic Durchmusterung*, the monumental work of Gill and Kapteyn. 454,875 stars south of declination − 19° are included in this work. Unfortunately, the difficulty mentioned above, of reducing the magnitudes to an absolute system, has not been wholly overcome, but the work is published in a form which will permit this to be done later, if a method of reduction can be discovered. The extension of this great work to the north pole is one of the greatest needs of astronomy at the present time.

The map and catalogue of the Astrophotographic Congress, the most extensive research ever undertaken by astronomers, will not be discussed here, as it will doubtless be described by others better able than I to explain its merits. If completed, and if the difficulty of reducing the measures of brightness to a standard scale can be overcome, it will furnish the photographic magnitudes, as well as the positions, of two million stars. Time does not permit the consideration here of certain other investigations of photographic magnitudes, such as those made at Groningen. They generally relate to a comparatively small number of stars.

The suggestion that the intensity of a photographic star-image be measured by the amount of heat it cuts off from a thermopile, deserves careful study. It should give a great increase in precision, and would obviate dependence on that tool of many defects, the human eye. No use seems to have been made, so far, of this method.

The next question to be considered is, what use should be made of these various measures of the light of the stars? The most obvious application of them is to variable stars. While the greater portion of the stars undergo no changes in light that are perceptible, several hundred have been found whose light changes. A natural classification seems to be that proposed by the writer in 1880. A few stars appear suddenly, and are called new stars, or novæ. They form Class I. Class II consists of stars which vary by a large amount during periods of several months. They are known as variable stars of long period. Class III contains stars whose variations are small and irregular. Class IV contains the variable stars of short period, and Class V the Algol variables, which are usually of full brightness, but at regular intervals grow faint, owing to the interposition of a dark companion. Twenty years ago, when photography was first applied to the discovery of variable stars, only about two hundred and fifty of these objects were known. Since then, three remarkable discoveries have been made, by means of which their number has been greatly increased. The first was by Mrs. Fleming, who, in studying the photographs of the Henry Draper Memorial, found that the stars of the third type, in which the hydrogen lines are bright, are variables of long period. From this property she has discovered 128 new variables, and has also shown how they may be classified from their spectra. The differences between the first, second, and third types of spectra are not so great as those between the spectra of different variables of long period. The second discovery is that of Professor Bailey, who found that certain globular clusters contain large numbers of variable stars of short period. He has discovered 509 new variables, 396 of them in four clusters. The third discovery, made by Professor Wolf of Heidelberg, that variables occur in large nebulæ, has led to his discovery of 65 variables. By similar work, Miss Leavitt has found 295 new variables. The total number of variable stars discovered by photography during the last fifteen years is probably five times the entire number found visually up to the present time. Hundreds of thousands of photometric measures will be required to determine the light-curves, periods, and laws regulating the changes these objects undergo.

A far more comprehensive problem, and perhaps the greatest in astronomy, is that of the distribution of the stars, and the constitution of the stellar universe. No one can look at the heavens, and see such clusters as the Pleiades, Hyades, and Coma Berenices, without being convinced that the distribution is not due to chance. This view is strengthened by the clusters and doubles seen in even a small telescope. We also see at once that the stars must be of different sizes, and that the faint stars are not necessarily the most distant. If the number of stars were infinite, and distributed according to the laws of chance throughout infinite and empty space, the background of the sky would be as bright as the surface of the sun. This is far from being the case. While we can thus draw general conclusions, but little definite information can be obtained, without accurate quantitative measures, and this is one of the greatest objects of stellar photometry. If we consider two spheres, with the sun as the common centre, and one having ten times the radius of the other, the volume of the first will be one thousand times as great as that of the second. It will, therefore, contain a thousand times as many stars. But the most distant stars in the first sphere would be ten times as far off as those in the second sphere, and accordingly if equally bright would appear to have only one one-hundredth part of the apparent brightness. Expressed in stellar magnitudes, they would be five magnitudes fainter. In reality, the total number of stars of the fifth magnitude and brighter is about 1500, of the tenth magnitude, 373,000, instead of 1,500,000, as we should expect. An absorbing medium in space, which would dim the light of the more distant stars, is a possible explanation, but this hypothesis does not agree with the actual figures. An examination of the number of adjacent stars shows that it is far in excess of what would be expected if the stars were distributed by chance. Of the three thousand double stars in the Mensuræ Micrometricæ, the number of stars optically double, or of those which happen to be in line, according to the theory of probabilities, is only about forty. This fact should be recognized in any conclusions regarding the motions of the fixed stars, based upon measures of their position with regard to adjacent bright stars.

We have here neglected all conclusions based upon the difference in composition of different stars. Photographs of their spectra furnish the material for studying this problem in detail. About half of the stars have spectra in which the broad hydrogen lines are the distinguishing feature. They are of the first type, and belong to Class A of the classification of the Henry Draper Memorial. The Milky Way consists so completely of such stars, that if they were removed, it would not be visible. The Orion stars, forming Class B, a subdivision of the first type in which the lines of helium are present, are still more markedly concentrated in the Milky Way. A large part of the other stars, forming one third of the whole, have spectra closely resembling that of the sun. They are of the second type, and form classes G and K. These stars are distributed nearly uniformly in all parts of the sky. Class M, the third type, follows the same law. Class F, whose spectrum is intermediate between classes A and G, follows the same law of distribution as classes G and K, but differs from them, if at all, in the opposite direction from Class A. There therefore seem to be actually fewer of these stars in the Milky Way than outside of it. One class of stars, the fifth type, Class O, has a very remarkable spectrum and distribution. A large part of the light is monochromatic. Of the ninety-six stars of this type so far discovered, twenty-one are in the large Magellanic Cloud, one in the Small Magellanic Cloud, and the remainder follow the central line of the Milky Way so closely, that the average distance from it is only two degrees. All of these stars, with the exception of sixteen, have been found by means of the Henry Draper Memorial.

It will be seen from the above discussion, that stellar photometry in its broadest sense furnishes the means of attacking, and perhaps of solving, the greatest problem presented to the mind of man, the structure and constitution of the stellar universe, of which the solar system itself is but a minute and insignificant molecule.

SECTION A — ASTROMETRY

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(*Hall* 9, *September* 21, 10 *a. m.*)

CHAIRMAN: PROFESSOR ORMUND STONE, University of Virginia.
SPEAKERS: DR. OSKAR BACKLUND, Director of the Observatory, Pulkowa, Russia.
PROFESSOR JOHN C. KAPTEYN. University of Groningen, Holland.
SECRETARY: PROFESSOR W. S. EICHELBERGER, U. S. Naval Observatory.

THE DEVELOPMENT OF CELESTIAL MECHANICS DURING THE NINETEENTH CENTURY

BY OSKAR BACKLUND

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THE development of celestial mechanics during the nineteenth century is such a comprehensive theme that a fundamental treatment of it within the limits of an address of half an hour or so cannot be thought of. I am therefore limited to the presentation of the principal phases of the subject, and of course in doing so, by reason of the necessary arbitrariness of choice, may not meet the approval of this distinguished assembly.

I will first consider the development of celestial mechanics in so far as it concerns the motions of the planets.

The nineteenth century received a great inheritance from the eighteenth. With the five undying names of Euler, Clairaut, d'Alembert, Lagrange, and Laplace are linked theoretical discoveries which upon the basis of Newton's law explained all the motions of the planets, satellites, and comets in so far as they were furnished by the observations of that time. Was this inheritance so utilized during the past century, that at the end of the same the results of observation may be considered explained by theory? The following discussion will give the answer.

Laplace's *Mécanique Céleste* gives as it were a summing-up of the development of celestial mechanics during the eighteenth century and furnishes at the same time the starting-point for the researches of the nineteenth. We recapitulate, therefore, some of the principal points of the same in order that the discussion may be more easily understood. The coördinates and the elements of the planets and satellites were expressed in series containing: (1) periodic terms (sines and cosines of multiples of the mean and true longitudes); (2) non-periodic terms involving powers of the time, *i. e.*, the so-called secular terms; (3) semi-secular terms, that is, products of the time or the angle into sine and cosine functions; the development being made in powers of the eccentricities and inclinations considered as small quantities. The appearance of the time or the angle explicitly, outside the sine and cosine functions, was considered, at least in part, both by Laplace and Lagrange as the result of incomplete operations. But on the other hand, from the standpoint of astronomy, it was considered entirely useless to complicate the expressions by introducing trigonometrical series in place of angles. The constants of integration, *i. e.*, the elements, were determined numerically for each of the planets then known, and the numerical values of the coefficients of the individual terms of the series were derived therefrom. It was then sufficient in most cases to consider only the lowest powers of the eccentricities in order to obtain the places of the planets with an accuracy corresponding with that of the observations. As a result astronomers were enabled to explain all the observed inequalities; for example, the great inequality in the motions of Jupiter and Saturn, the inequality in the motion of Jupiter's satellites discovered by Wargentin, etc. With the aid of his epoch-making theory of the variation of constants, Lagrange proved the famous theory that the expression for the major axis contains only periodic terms, when powers of the mass higher than the first are neglected. Both Lagrange and Laplace had found that in the first approximation the eccentricities and inclinations may be considered as long-period functions of the time. Through the researches of Laplace the theory of the moon, the motion of the earth about its centre of gravity, and the theory of the figures of the planets, were so developed as completely to satisfy the corresponding observations.

These brief statements of some of the principal points may be sufficient.

The nineteenth century was introduced by two researches of Poisson of remarkable value to celestial mechanics. One was the extension of Lagrange's theory in regard to the major axis to the second power of the mass; the other and far more important was his classic theory of the motion of the earth about its centre of gravity, which was built up by the aid of the method of the variation of constants. There was then for a time a pause in the development of the theories of the major planets.

The transition to the new century was, however, marked by the beginning of a series of discoveries which are to-day still being carried forward, and which in one direction have exerted an important influence in the development of celestial mechanics. I refer to the discovery of the small planets. Laying aside the numerical calculation of special perturbations, which has been developed to a high degree of refinement, the perturbation problem will be considered here in the sense in which it was brought over from the preceding century. The interpolation formulæ which the special perturbations offer can permit only an extremely incomplete insight into the nature of the motion. To be sure, the general perturbation formulæ in the form given by Laplace are also to be considered merely as interpolation formulæ, since they hold for only a limited time, practically nothing being known in regard to the absolute convergence of the series of secular terms. We shall first of all follow the important investigations which have been made for the purpose of representing the motion of a small planet by means of general perturbation formulæ in the sense spoken of.

In addition to the *Theoria Motus Corporum Coelestium*, which, for apparent reasons, does not here come under consideration, Gauss busied himself with the theory of the minor planets, by making extended investigations on the perturbations of Pallas. He did not bring his work to a conclusion, and thus it has remained without further significance. The prize problem given by the Paris Academy in 1804, and repeated in following years, led in 1812 to the memoirs of Burkhart and Binet which, however, inspired no further contributions to the solution of the problem of the perturbations of the small planets. Meanwhile an interesting comet was discovered in 1818 by Pons of Marseilles, whose orbit was computed by Encke, and which was on that account called Encke's comet. The aphelion of the comet lies within the orbit of Jupiter, the eccentricity is far greater than that of any of the hitherto known planetary orbits, and the inclination amounts to 12°. If the formulæ could be found which represent the motion of this comet, the question in reference to the small planets would also be solved. It was, however, not merely from this point of view that Hansen set himself the problem of obtaining such formulæ. He doubted, in fact, the correctness of the comet's acceleration found by Encke, and hoped by means of general formulæ to be able to settle that question. On the basis of the differential equations given in the *Fundamenta Theoria Orbitis quam perlustrat Luna*, Hansen developed formulæ for the perturbations of the logarithm of the radius vector, of the time, and of the sine of the latitude. The essential difference from Laplace's theory consists in the fact that Hansen employs arguments containing multiples of the eccentric anomaly of the comet, of the mean anomaly of the disturbing body. To a given finite power of the ratio of the two semi-major axes there thus belongs a double series, which, with reference to the disturbing body, is an infinite series of powers of the eccentricity, but with reference to the disturbed body is a finite series. This theory Hansen published shortly after 1830 under the title of *Störungen in Ellipsen von grosser Excentricität*, and at the same time made an attempt to obtain the general perturbations of Encke's comet produced by Saturn. Although the computations were not brought to a definite close, still it cannot be doubted that his method is useful for this case. As the perturbations by Jupiter are far more important, both with reference to Encke's comet and to the small planets, and cannot be obtained by this method, Hansen's work cannot be considered as satisfactory, but rather as a failure, at least with reference to Encke's comet. He, therefore, attacked the problem from an entirely different standpoint, and devised the so-called partition method, which he published in his Paris prize memoir, together with an application to the perturbations produced on Encke's comet by the planet Jupiter. This example was also not carried to an end, evidently for the simple reason that this was practically impossible, and thus we see that this method also was unable to solve the problem.

After his unsuccessful effort to obtain general perturbations for such eccentric orbits as that of Encke's comet, Hansen turned his attention especially to the small planets, and by a further development of the method given in his first memoir succeeded in giving formulæ by means of which he was enabled to represent the motion of the planet Egeria, at least for the time embraced by the observations at his disposal. Unfortunately in this, as in the case of so many other small planets, theory and observation deviated more and more the more distant the latter lay from the epoch of the former, so that after about fifty years, his tables no longer satisfactorily represent the observations. Later several of Hansen's pupils, Lesser, Blecker, and others, computed the general perturbations of some of the small planets. The most prominent of Hansen's pupils, Gyldén, again took up Hansen's partition method and substituted the mean anomaly of the planet and the partial anomaly of the comet by means of elliptic integrals, and thus obtained a much greater convergence in the development of the perturbative function. In the determination of the constants of integration, the old difficulties reappeared, so that taken as a whole no success appears to have been gained. Gyldén sought further, by means of a skillful combination of Hansen's partition method with a special development of series, to obtain a simpler method for the computation of the perturbations.

of the small planets. Meanwhile all the calculations made in this department of celestial mechanics soon showed that the path laid out by Hansen does not lead to the object desired. Above all, without an immense expenditure of time and labor no trustworthy results can be obtained for planets that occur in the neighborhood of the so-called gaps, for which the terms of long period cannot be accurately determined; and besides, in this case, the convergences of the secular terms are much slower. All attempts in this direction lead only to the result that at best we may obtain in this way approximate perturbation formulæ which for a considerable time will guarantee the rediscovery of the planet, without claiming to represent the observations.

The circumstance that a large portion of the small planets occur in the neighborhood of the so-called gaps, thus causing such an increase in the perturbations that after a relatively short time these can no longer be considered as small quantities, led Gyldén to state the question in the following manner: "Will it be possible to determine the elements as absolute constants, and to so determine the terms of long periods (thus avoiding completely the introduction of the time explicitly) that the intermediate orbit thus obtained shall remain included within definite limits, and only differ from the real orbit by quantities of the order of the masses of the planets?" This question includes the question of stability, and the principal problem thus consists in proving the convergence of the long-period series. Gyldén believed that he could establish the convergence by means of what he called the horistic method. Poincaré, however, disputes the correctness of this method. On this assumption Gyldén's theory would be merely an hypothesis. Even if the method is correct, it is applicable only with reference to a limited number of small planets, as it is based upon the development in powers of the eccentricities and inclinations of the disturbing and disturbed planets. Here we stand, so far as this question is concerned, at the end of the nineteenth century. Upon the problem presented at the beginning of the century much skill and labor has been spent; a satisfactory solution has not, however, been reached.

If now we turn to the larger planets, a more gratifying picture presents itself. Here we find at the end of a century a work well completed; taking it all in all, theory has in this case mastered quite satisfactorily the century's immensely rich and abundant observations.

Not only the number of observations made during the first half of the century, but still more their epoch-making precision, which is linked with the names of Bessel and Struve, soon showed that the numerical formulæ of Laplace were not sufficient to satisfy the increasingly accurate observations. In individual cases, it is true, the existing tables of motion were replaced by more accurate ones, as, for example, Hansen's and Olufson's tables of the sun, etc., and Hansen began a new theory of the motions of Venus and Saturn, which he published in his Berlin prize memoirs. Nothing, however, forming a congruous whole was at this time accomplished. The uncertainty of the astronomical constants, the inconsistency between the different determinations of them, the need of a more accurate knowledge of the masses of the large planets for the investigation of the motions of the small planets and comets, the especially unsatisfactory theory of the planet Uranus, all these urged investigators to a thorough revision of the theory of the large planets. At this time, at the opportune moment, appeared the astronomical giant, Leverrier. He was already known to the astronomical world by his work on the secular variations of the orbits of the inner planets, published in the year 1839, when by his wonderful investigations on the motion of the planet Uranus he not only established the existence of an outer planet, but also gave its position so accurately that it was only necessary to direct the telescope to this point of the heavens in order to find it. In connection with the discovery of the planet Neptune, which furnishes one of the most brilliant chapters of the century in celestial mechanics, justice demands that we also mention with equal praise the name of Adams.

Shortly after the discovery of Neptune, Leverrier began the colossal work of the revision of the planetary system, which he was enabled to bring to a conclusion. Leverrier planned his work clearly and systematically, and clearly and systematically carried it out. Lagrange's method of the variation of constants proved its power in the most splendid manner. Mathematician and astronomer, Leverrier gave in a peculiarly harmonious combination only the necessary formulæ and these in the simplest manner, and so arranged the astronomical material as most completely to suit his problem. I am convinced that, no matter how many new revisions may be necessary, Leverrier's *Annales de l'Observatoire de Paris* will never be forgotten.

The theory of the inner planets was completed at the end of the sixties, whence new and much-needed values of the masses of the planets Mercury and Venus, as well as of the solar parallax, were obtained. As a result of this investigation it was discovered that the motion of the line of apsides of the planet Mercury was not represented satisfactorily by the theory, whence Leverrier assumed the existence of an unknown intra-Mercurial planet as the cause of the perturbations.

What disappointment Leverrier met at that time is well known, but it is also well known that nothing could turn him from his devotion to the great problem, nothing could bend the force of his great genius. Even after his flight from Paris a lead pencil and a copy of logarithm tables furnished a sufficient means for carrying on the theory of Jupiter. The work was completed not long before his death, and science possessed a theory of the motion of that great planet carried out in a remarkably homogeneous manner; even the theory of Saturn, after a few additional computations by Gaillot, could be considered satisfactory.

In spite of the advances which Leverrier's work shows, astronomy needed another giant to reach the standpoint which it has gained during the last century; the name of this giant is Newcomb. A colossal conscious force, the most comprehensive theoretic knowledge, an acquaintance with observing material and its significance extending to the smallest details, were necessary conditions for the undertaking, immediately after Leverrier, of a revision of the planetary theory. During the last half of the century a mass of observations, rich in quantity and quality, had been gathered, which Leverrier had been unable to use; moreover, additional determinations of certain astronomical constants gave values, which, in consideration of the great accuracy now demanded, it was necessary to take account of in place of those employed by Leverrier. Newcomb's great aim was to obtain a system of astronomical constants and elements of motion which should be as unified as possible, and should correspond with the progress made in the art of observing. The theories of the planets Neptune and Uranus which Newcomb published about 1870, but above all his *Catalogue of Fundamental Stars*, seemed to be precursors of the *Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac.* The catalogue mentioned is especially important, since it forms, in a certain sense, an epoch in the systematic treatment of observations, and the preparation of them for the service of theory. What Newcomb did for the right ascensions, Boss has done for the declinations. In the *Astronomical Papers* we find, then, *summa scientia astronomica,* in all questions that refer to the solution of the problem under discussion. The developments of the theories of the planets, while in agreement with the general fundamental principles of celestial mechanics, are especially adapted to the individual cases, varying in method as the problems demand, and are always so explained as to keep clearly in view the object to be attained. In this work, beside the name of Newcomb shines that of the great mathematician, Hill, who has made available for the advancement of astronomical research the almost forgotten treasures of the immortal Gauss, and was the first to apply successfully Hansen's method to the computation of the mutual perturbations of Jupiter and Saturn. The astronomical papers are thus valuable, not only on account of the results themselves, but also by reason of the methods by means of which these were attained; that is, in other words, these papers have brought celestial mechanics, both in form and content, to a definitely higher plane. Newcomb's little book, *Astronomical Constants*, gives in concise and clear form a bird's-eye view of the results of this work. Newcomb has succeeded in attaining essentially what he started to attain; he has contributed to science a homogeneous system of the fundamental constants of astronomy; to his energy, almost bordering on the wonderful, we are indebted for the realization of the most valuable results at present attainable from modern observations. This view was expressed by the Paris Congress of 1896, when it accepted Newcomb's system almost unchanged; and if it were to assemble again to-day it would certainly correct a small error which it committed. The determination of the precession constant by means of the stars, which remained to be accomplished, as well as the formation of the fundamental catalogue of stars, was delegated to him and was practically accepted in advance, an evidence of the unlimited trust in the authority of Newcomb.

Among the improved values of the masses which result from Newcomb's theory, I should like to call especial attention to the mass of the planet Mercury, which is 30 to 40 per cent smaller than that obtained by Leverrier. On account of the smallness of the coefficient of the mass in the equations of condition, this is very difficult to obtain. Now, however, it has been obtained in another manner and independently, whence it arises that it may be considered as correct within its probable error; this proves again the rigor with which the calculations in the *Astronomical Papers* have been carried out. The motion of the line of apsides of the planet Mercury, not explained by theory, which was discovered by Leverrier, is confirmed by Newcomb. The explanation of this motion will have to wait for further astronomical discovery.

One of the most beautiful discoveries of the century was that of the satellites of Mars. The new problem in celestial mechanics arising therefrom was solved by the discoverer. The fifth satellite of Jupiter, which was added to science by the distinguished observer, Professor Barnard, has added another very important theoretical problem. If we glance now over what has been presented, it cannot be denied that celestial mechanics, during the past century, especially with reference to the motions of the major planets, has essentially kept pace with the results of observation, and that as a whole it satisfies the enormously improved methods of observing. The last thirty years of the past century belong, in this respect, to America, and I believe that every European astronomer will agree with me that they are also the most important.

The progress made in the field of lunar theory has not been mentioned. To do this, however, I should have to explain the works of Poisson, Plana, Hansen, Delaunay, Newcomb, Adams, Tisserand, Hill, and many others, which would require an address at least as extended as the present. I limit myself, therefore, to reminding you that the lunar theory offers yet unsolved problems to the theoretical astronomer, in spite of the splendid results of the savants named, and in spite of the fact that Newcomb has succeeded in improving Hansen's Lunar Tables, and that at present these represent the observations well.

I have purposely omitted the names of Jacobi and Hamilton, so well known in celestial mechanics, whose theories have received further development and application from Delaunay, Tisserand, and Hill, and have served Poincaré as a starting-point for his remarkable theories, beginning with his beautiful Prize Memoir and continuing through the *Méthodes Nouvelles de la Mécanique Céleste*, etc. I do not feel justified in expressing myself with reference to the value and meaning of this last work, for the simple reason that I am not mathematically competent to do so. That Hill's and Poincaré's theories introduce a new epoch, whose fruits the twentieth century will harvest, there seems to exist no doubt.

The nineteenth century has added a new chapter in celestial mechanics, the theory of meteors and comets in their relation to one another. We owe to the clever researches of Schiaparelli, Newcomb, Bredichin, and others the remarkable insight obtained into the motion of these small bodies, which remain individually invisible, except when they penetrate our atmosphere and blaze up, or become visible by being crowded together in the form of comets.

STATISTICAL METHODS IN STE

BY JACOBUS CORNELIUS KAPTEYN

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THE remark has been made several times lately: "The nineteenth century has brought the problem presented by the motions in our solar system to a certain issue. It will be the task of the twentieth to attack the problem of the arrangement and motions in the stellar universe." Science has put astronomers in the possession of new weapons eminently suitable for the purpose:

Photography which dreads no numbers.

Spectroscopy which does not care for distance.

Is it possible with their aid even now to make some plan of campaign?

That is:

Can any way be suggested for the solution of what a famous astronomer recently very conveniently called *the sidereal problem?*

You will forgive me if, in trying to answer this broad question, I wholly restrict myself to presenting my own views on the matter. I am sure that nobody can appreciate more highly than I do, what, to mention only a few of the most recent investigations, such men as Schiaparelli, Newcomb, Seliger, Kobold, Easton, are doing, but want of time utterly prevents me from discussing their methods in so far as they differ from my own.

From an astrometrical point of view the problem in its simplest form comes to this: to determine for every individual star its *position, velocity,* and *mass.*

For practical reasons we may add: its total quantity of light, which in what follows we will call its *luminosity,* though we thus encroach somewhat on the domain of astrophysics. If we assume, as there is ample reason to do, that Newton's law holds good for the whole of the stellar universe, then these data determine the past, the present, and the future arrangement of the stars in space.

We may safely assume that the problem will never be completely solved in this form; for the mass of data, even if it could ever be obtained, would be so overwhelming, that it would defy any detailed mathematical treatment.

But what we may hope to attain and for which I think we possess even now data, no doubt extremely incomplete but still by no means contemptible, is the recognition of the general character of the distribution of these elements, from which we may get an insight into the general plan of the system, and in due time some glimpses of its history past and future.

Just as the physicist investigating the small world of the molecules of a gas cannot hope to follow any one particular molecule in its motion, but is still enabled to draw important conclusions as soon as he has determined the mean of the velocities of all the molecules and the frequency of determined deviations of the individual velocities from this mean, so in the greater world of the stars our main hope will be in the determination of *means* and of *frequencies*.

What is the mean mass of the stars?

How many of them have double, treble — half, a third — that mean mass; in other words, what is the frequency of a given mass?

Are this mean and this frequency the same for the different portions of the stellar world? If not, how do they vary?

In the same way:

What is the mean luminosity of the stars? What the frequency of given multiples of that mean? How do these quantities vary with the position in space?

And again:

What is the mean distance of determined groups of stars and what the frequency of distances different from this mean? Knowing which elements, we shall know the number of stars per unit of volume, that is, the star-density for this group.

Is this density the same at different distances from the sun? Is it the same in and out of the Milky Way?

And once more:

What is the mean velocity of the stars; what the frequency of a determined amount of velocity, and how do these quantities vary with the position?

The knowledge of all these elements will not only give us a general insight into the structure of the stellar system, but also of its change in a relatively short time. Even if, in the course of time, our knowledge becomes sufficiently complete, it will yield a notion about the attractive forces at work in the system, from which again a conclusion will be possible in regard to its more remote past and future.

Looking more closely at the difficulties of the problem, we find that, as far as the masses are concerned, they lie in the fact that, up to the present, but very few traces of a mutual attraction of the stars have been found. But already spectroscopy has made a splendid beginning. It has brought to light the fact that a large proportion of the stars are binaries. Campbell estimates the proportion at no less than a fifth of the whole. Of these the motions in the visual line can be studied. They will furnish us in time, not generally with individual masses, but with a thorough knowledge, both of the mean masses and of the frequencies of determined deviations from these means, at least for the stars of this class.

The remaining elements are:

The three coördinates and the three components of the velocity.

Of the coördinates two are known for a great number of stars, and there is nothing in the way of a more complete knowledge where such may be wanted.

Of the third coördinate, the distance, we know exceedingly little.

Of the three components of the velocity we know, or may soon hope to know, for great numbers of stars:

By spectroscopic observation, the absolute value of one of the components;

By the classical astronomical observations ancient and modern, the two others expressed in arc.

Here, too, our data, to be complete, require the knowledge of the distances. So at the bottom of all lies the difficulty of the determination of the distances.

This has long been felt, and endeavors have not been wanting to remedy the defect.

By these endeavors it has become evident that, save in exceptional cases, these determinations are beyond our power.

Astronomers looked out, therefore, for such exceptional cases, and nearly exclusively concentrated their effort on these. There will be, however, little to encourage us to go on in this way, as soon as the parallaxes of a few hundreds of the most promising objects (stars of excessive brightness or proper motion) shall have been satisfactorily determined. Not only will the most exceptional and really promising objects be exhausted to a great extent, but we must not forget, moreover, that the knowledge of the distance of such selected objects will directly but little further our insight into the general structure of the system. Just because they are *selected* objects they will not be representative of the whole. What other way remains open?

The difficulty depends evidently on the smallness of the parallaxes, in other words, on the smallness of the diameter of the earth's orbit as a base. We are thus necessarily driven to employ the only greater base available, viz. the path traversed by the solar system in its motion through space.

The distance traversed by the sun since Bradley's time is already well over 300 times the diameter of the earth's orbit. The parallactic motion corresponding thereto, where not foreshortened, must amount to over 7″ for the mean of the stars of the sixth magnitude, to about 2″ for those of the ninth.

These are quantities which we may hope to measure with some precision.

It is true that, for the elucidation of most questions, we require accurate proper motions for multitudes of stars not observed by Bradley or other early astronomers, but the difficulty is not a formidable one. As de Sitter and myself tried to demonstrate elsewhere,[1] photography enables us to obtain, in a dozen years, proper motions of as many stars, down to the faintest we can photograph, as will be required for our purposes. The precision need no way be inferior to that of the bulk of the Bradley stars.

It is also true that a still greater base-line would be acceptable; but we may provisionally be content. The base as it is will enable us to reach many conclusions, and even while we struggle on with our problem the base is lengthening out, the precision of the observations is increasing. We may look forward to enormously improved data by the time we have exhausted the treasures virtually contained in the data already now available or obtainable in a short time.

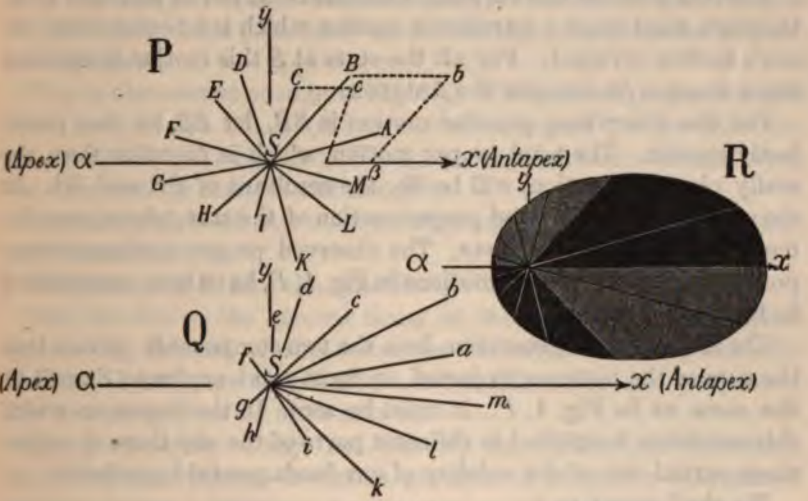

FIG. 1

Of course the difficulty of using the parallactic motion as a measure of distance is, that, for individual stars, we do not know what part of the observed motion is parallactic, that is, due to the solar motion, what part is peculiar to the star. The two become separable only for numerous groups of stars, and then only by the introduction of some hypothesis.

The most plausible thing to do seems to be to adopt as such the hypothesis already in general use in the derivation of the precession and the sun's motion in space: viz., "The peculiar motions of the stars are directed at random, that is, they show no preference for any particular direction." I shall further on refer to this hypothesis as the fundamental hypothesis.

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[1] See *Publications of the Astronomical Laboratory at Groningen*, no. 14, preface.

I shall presently enter into a discussion of it.

If, for the moment, we adopt it, we see at once that we can get the mean parallactic motion of any large group of stars free from any admixture of the peculiar motion.

In Fig. 1, *P*, I have schematically represented the peculiar motion of a number of stars crowded together near the point *S* of the sphere. They must show no preference for any direction. As a consequence, the sum of the projections on any line, counted positive one way and negative in the opposite direction, must be zero.

Now the peculiar motion cannot be observed, for, in addition to it, the stars must have a parallactic motion which is no other than the sun's motion reversed. For all the stars at *S* this motion is directed along the line Sx towards the Antapex.

For the star whose peculiar motion is SB, let $S\beta$ be the parallactic motion. The total proper motion, which is no other than the really observed motion, will be Sb, the resultant of SB and $S\beta$. In the same way the observed proper motion of the star, whose peculiar motion is SC will be Sc, etc. The observed proper motions corresponding with the peculiar motions in Fig. 1, *P*, have been represented in Fig. 1, *Q*.

The mode of their generation from the two components proves that the sum of the motions projected on Sy at right angles to Sx will be the same as in Fig. 1, *P*. It must be zero. In the degree to which this condition is satisfied in different parts of the sky there is a precious partial test of the validity of our fundamental hypothesis.

We shall revert to it.

On the other hand, the sum of the observed motions projected on Sx will be the sum of the total parallactic and the projected peculiar motions. The latter being zero, for the same reason as before, we see that we get the sum of the parallactic motions, consequently the mean parallactic motion of the group free from the peculiar motions. This mean parallactic motion at once yields the mean parallax of the group.

Adopting Campbell's velocity of the solar system, we have but to divide by 4.20 multiplied by the sine of the angular distance of the group from the Apex.

In applying this method, however, we shall always have to bear in mind that it rests on the supposition embodied in our fundamental hypothesis, and that it cannot be used therefore for groups in which the proper motions must evidently favor some particular direction.

So we can safely apply it to stars of any one particular magnitude.

For in the magnitude of a star can be no reason for a predilection of the direction of its motion. It is erroneous, however, to apply it to stars of which the total proper motion has any particular value.

For there are three reasons which will make the apparent proper motions of a star considerable.

(*a*) Considerable linear velocity;

(*b*) Small distance from the solar system;

(*c*) Near coincidence of the direction of the parallactic and peculiar proper motion, whereby the effect of the two is added.

Therefore if, for instance, we select stars of very considerable proper motions, we are certain to give a certain amount of preference:

To stars of great linear velocity and to stars of small distance from the sun, but also to stars the peculiar motion of which favors the direction of the parallactic motion, that is, the direction toward the Antapex.

This is also seen at once from Fig. 1, *Q*. The greatest proper motions there are: *Sa*, *Sb*, *Sc*, *Sm*, *Sl* — corresponding to the peculiar motions *SA*, *SB*, *SC*, *SM*, *SL*, in Fig. 1, *P*, which motions evidently favor the direction *Sx* towards the Antapex.

It is the neglect of this consideration which has led several astronomers into error.

We have now to face the question:

Can we derive the general traits of the structure of the stellar system by the aid of the distances derived from the parallactic motion. I am convinced that we can, and it is the real purpose of this lecture to show in what manner.

The most direct, though not the only way, I think, is that which begins by determining the mean parallax of stars of a determined magnitude and a determined amount of proper motion.

Though, as I explained just now, we cannot derive the parallax of stars of a determined proper motion from the parallactic motion, we can still gain our end somewhat indirectly, but pretty satisfactorily, if to the data furnished by that motion we add what we know by direct determination of parallax.

The results thus obtained were embodied in a simple formula, in a paper published a few years ago.

For the sake of brevity I will call the parallax of any star computed by this formula from its magnitude and proper motion its mean or theoretical parallax.

If for any individual star the true parallax were equal to this theoretical one, we should of course know at once the distribution in space of all the stars of which we know the apparent magnitude and the proper motion.

It need not be said that they will be generally unequal.

Without knowing the individual parallaxes we may still find out the real distribution, however, if we can make out which fraction of the stars have a parallax exceeding its theoretical value two, three, four . . . times, for which fraction this parallax is only half, a third, etc.

We thus have to see whether it be not possible to find out this law of the frequencies.

Theoretically nothing is easier than to derive it from the data furnished by the stars of which the parallax has been measured. For these objects we know the true as well as the theoretical parallax, and we may thus determine at once the frequency of any deviation of the two.

We thus see that there is nothing to prevent us from obtaining ultimately a thorough knowledge of the law in question.

For the present, however, existing materials are quite insufficient for such a thorough determination, and we must provisionally have recourse to a less fundamental course.

The question is analogous to the other, with which every astronomer is familiar: What is the frequency with which errors of a given amount will occur in a determined series of observations? Everybody knows that, by admitting certain plausible hypotheses, which may be supposed to be approximately satisfied in most cases, the question is reducible to the finding of a single number, to that of the probable error, for instance.

Something of the same sort may be done here. True, the conditions, supposed to be satisfied for the distribution of the errors of observation, are certainly not satisfied for the deviations of the true parallaxes from their theoretical value. For if, for instance, the theoretical parallax is 0″ 01, it is evident that a deviation of 0″ 02, very well possible in one direction, is impossible in the opposite one. Positive and negative deviations of the same amount are certainly not equally probable.

We may, however, introduce other conditions, which are certainly satisfied at the limits and which for the rest may be deemed plausible. These will lead to a law of frequency, different from that of the errors of observation, but like that law only dependent on one or only a few constant parameters. I have chosen a form with a single constant.[1]

We may take such a course with the more confidence the smaller the deviations are. For, as these deviations decrease, our independence from the form of the assumed law increases. In our case I find that 70 per cent of all the stars have their true parallax included between 0.4 and 1.6 times their theoretical parallax.

Still of course it cannot be maintained that the frequency law

[1] See *Publications of the Astronomical Laboratory, at Groningen*, no. 8.

thus provisionally adopted represents the facts of nature accurately. We have not sufficient data for testing the matter. But we may safely assume that, the constant parameter being determined from the observations themselves, we must get enormously nearer to the truth than by adopting for the parallax of any star its theoretical value. It appears that even from existing determinations of parallax this one parameter is already obtainable with some precision.

In a few years more extensive results of observation will surely enable us to get a better approximation, and we may well hope that the time is not far distant when accumulated data will furnish such knowledge of this frequency-law as leaves little to be desired.

Having once found the law of the frequencies, we may determine the true frequency of the distances with the same ease as we determine the frequency of determined errors of observations as soon as the probable error is known. The following example will illustrate the whole process.

From the observations of Bradley, which embrace about two thirds of the whole sky, we learn that somewhat less than 10 per cent of the stars of the sixth magnitude have centennial motions ranging from 4″ to 5″. The total number of the sixth magnitude stars in the whole sky is 4730.

We conclude that in the whole sky there must be almost 10 per cent of 4730, in fact, 461 stars of the sixth magnitude having proper motions ranging from 4″ to 5″.

Our formula gives almost exactly 0″01 for the theoretical parallax of the stars of this magnitude and proper motion.

With these data our frequency-law leads at once to the following distribution of the true parallaxes:

461 stars; mag $=6$; $\mu=0''045$
mean $\pi=0''0102$

Limits of π		*Fraction of the whole*	*Number*
0″0000 and	0″0010	0.001	0
.0010	.0016	.004	2
.0016	.0025	.028	13
.0025	.0040	.097	45
.0040	.0063	.209	96
.0063	.0100	.275	127
.0100	.0158	.226	104
.0158	.0251	.116	54
.0251	.0398	.036	16^5
.0398	.0631	.007	3
0.0631		.001	0^5
Totals		1.000	461

For the limits of parallax I chose numbers increasing in the constant ratio of 1 to 1.585. They are such that, if all the stars had the same luminosity their apparent brightness would diminish by just one magnitude as we pass from one shell to the next more distant one.

Some of the earlier investigators have started from the hypothesis of equal luminosity of all the stars. In their theory, if the first shell contains the stars of the first magnitude, all the stars of the second magnitude will be contained in the second shell, those of the third in the third, and so on.

If we treat the stars of the other magnitudes and proper motions in the same way, we shall get what has been represented in Fig. 2.

We there see that of all the stars of the sixth magnitude, 614 will finally find their place in the 4th shell ($\pi = 0''016$ to $\pi = 0''025$), 833 in the fifth, 901 in the sixth, 771 in the seventh, etc.

It will be remarked how widely this arrangement differs from what it is in the just quoted theory which places all the stars of any one apparent magnitude in the same shell.

All the stars of the same apparent magnitude have been put down in the same sector; we have of course to imagine them distributed through the whole of the shell, mixed with those of other apparent magnitudes, the numbers of which have been inscribed in other sectors.

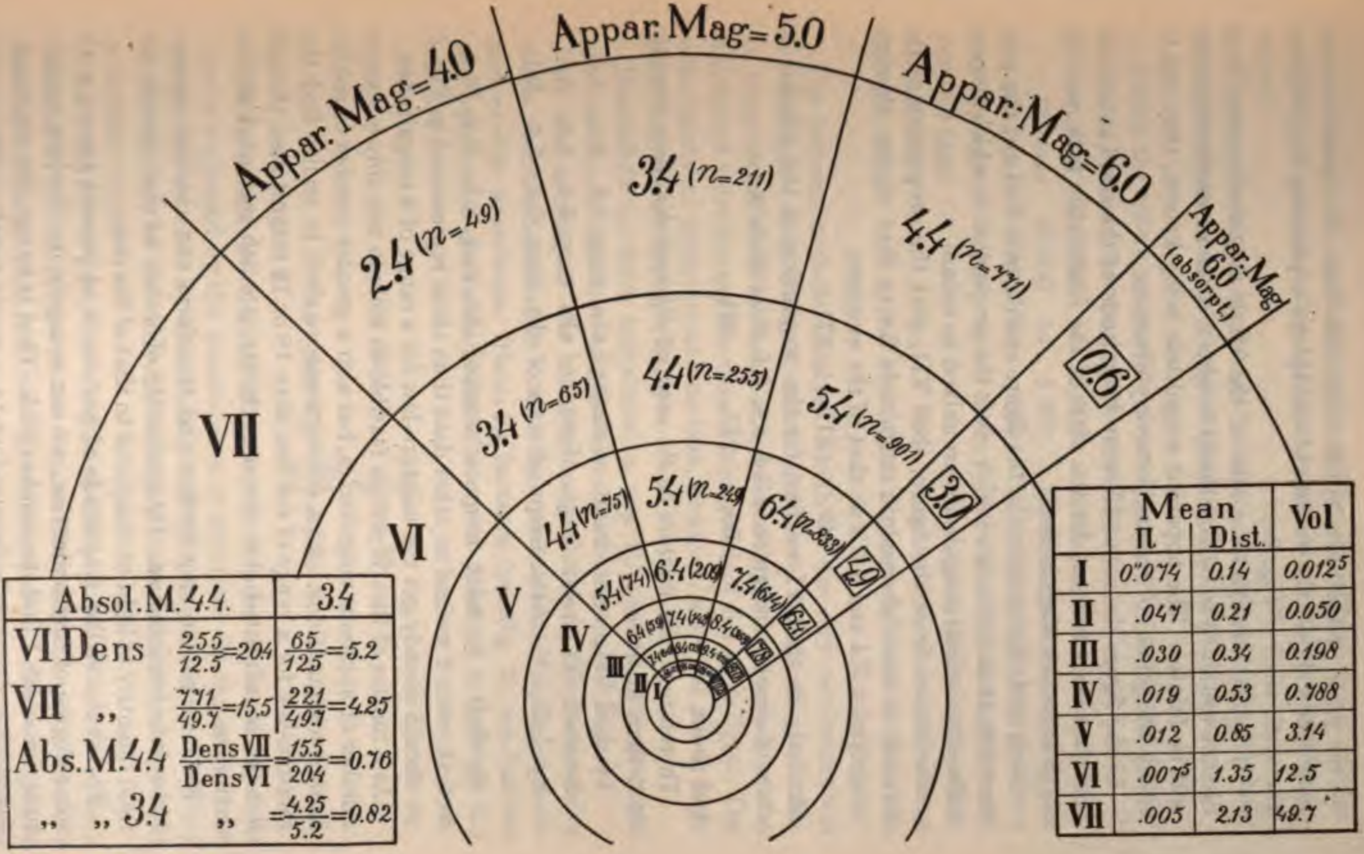
No more than seven shells could well be shown in the figure. In the actual computations their number is of course increased and the stars of the second, third . . . seventh, eighth, ninth magnitude were duly taken into account.

The complete figure would thus show the distribution in space of the stars of any apparent magnitude between the second and the ninth.

In principle no other hypothesis was introduced beyond our fundamental hypothesis, which supplied the mean parallaxes. It is true that the frequency-law introduced is still somewhat unsatisfactory and will remain so for some time to come. The objection, however, is not fundamental. As remarked already there is nothing to hinder us from finding out its true form as soon as we have a sufficient number of accurate parallaxes at our disposal.

In order to derive further results we shall now introduce a new hypothesis, viz., that light suffers no absorption in passing through space. We shall presently have to discuss in how far the results are changed if this hypothesis is dropped.

Admitting that space is perfectly transparent we can at once derive the absolute magnitude for every star in our figure. Absolute magnitude of a star we shall call the apparent magnitude this star would show, were it placed at unity of distance. I shall here take unity of distance to correspond with a parallax of $0''01$. The stars



Absol. M. 4.4.		3.4
VI Dens	$\frac{255}{12.5}=20.4$	$\frac{65}{12.5}=5.2$
VII ,,	$\frac{771}{49.7}=15.5$	$\frac{221}{49.7}=4.25$
Abs. M. 4.4	$\frac{\text{Dens VII}}{\text{Dens VI}}=\frac{15.5}{20.4}=0.76$	
,, ,, 3.4	,, $=\frac{4.25}{5.2}=0.82$	

	Mean Π.	Mean Dist.	Vol
I	0.″074	0.14	0.012^5
II	.047	0.21	0.050
III	.030	0.34	0.198
IV	.019	0.53	0.788
V	.012	0.85	3.14
VI	$.007^5$	1.35	12.5
VII	.005	2.13	49.7

FIG. 2

of the sixth magnitude, apparently, will thus also have absolute magnitude six on the surface of the sphere separating the fifth and the sixth shell of Fig. 2.

To appear always as a star of the sixth magnitude, stars more distant will have to be absolutely brighter, those at smaller distances will be fainter. The apparent magnitude being known, they can be easily computed, of course, for every distance.

In the figure the mean absolute magnitude of the stars in any one shell has been inserted for the stars of the fourth, the fifth, and the sixth apparent magnitude.

Now, first, the numbers in the figure enable us to find out the mixture-law, that is, the law which gives the proportion in which stars of different absolute magnitude are mixed in nature.

For we see from the figure, shell VII, that the proportion of the number of stars of absolute magnitude 2.4 to that of the absolute magnitudes 3.4 and 4.4 is as that of the numbers

49, 211, 371

Similarly in shell VI we find for the proportion of the number of stars of absolute magnitude 3.4, 4.4, 5.4, the numbers

65, 255, 901

and so on.

Therefore, *if the mixture be the same at different distances from the sun*, then we have

by shell VII, relat. frequ. of stars of abs. mag. 2.4, 3.4, 4.4
by shell VI, relat. frequ. of stars of abs. mag. 3.4, 4.4, 5.4
by shell V, relat. frequ. of stars of abs. mag. 4.4, 5.4, 6.4
. . . .
by shell I, relat. frequ. of stars of abs. mag. 8.4, 9.4, 10.4

so that, even if we had no other data than those represented in Fig. 2, we should already get the mixture-law for a range of 8 magnitudes.

As a matter of fact, it proves feasible to include not only a greater number of apparent magnitudes, but also a greater number of shells, in our computations. As a consequence we shall in reality find the mixture-law for a range of not less than 18 or 19 magnitudes, though it must be admitted that the uncertainty is much increased at the extremes.

We may go one step further and transform our absolute magnitudes into luminosities. By luminosity of a star we shall denote its total quantity of light as compared to that of the sun.

For as the stellar magnitude of our sun is at present known with some degree of approximation, we can compute its absolute magnitude, for which I find the number 10.5. That is to say, the sun transferred to a distance corresponding to a yearly parallax 0″01 would shine with the light of a star of the 10.5 magnitude.

Absolute magnitude 10.5 thus corresponding with unity of luminosity, that is, to the luminosity of the sun, there is no further difficulty in transforming other absolute magnitudes into luminosities.

The result arrived at for the mixture-law by carefully working out these ideas is roughly summarized in the following table:

Within the sphere whose radius corresponds to the mean parallax of the stars of the ninth magnitude there will be:

1 star	100 000	to	10 000	times more luminous than sun
46 stars	10 000	to	1 000	times more luminous than sun
1 300 stars	1 000	to	100	times more luminous than sun
22 000 stars	100	to	10	times more luminous than sun
140 000 stars	10	to	1	times more luminous than sun
430 000 stars	1	to	0.1	times more luminous than sun
650 000 stars	0.1	to	0.01	times more luminous than sun

The increase in these numbers, which for the very luminous stars is extremely rapid, becomes slower and slower for the fainter stars. It even seems as if we have to expect no further increase in the number of stars having less than a hundredth of the sun's light. The uncertainty of the extreme numbers, however, does not allow us to assert anything very positively.

Meanwhile we have introduced a new hypothesis, viz., that the mixture-law is the same at different distances from the sun.

By the overlap of the absolute magnitudes in the consecutive shells, we have, to a certain extent, the means of checking the correctness of this hypothesis. Thus, for instance (see Fig. 2), we find for the proportion of the numbers of stars of absolute magnitude 5.4 and 4.4:

$$\text{by shell V,}\ \frac{249}{75}=3.32$$

$$\text{by shell VI,}\ \frac{901}{255}=3.53$$

The numbers are slightly different; not more so, however, than can be explained by the uncertainties of our data. On the other hand we plainly see that by accumulation of such uncertainties the proof of the identity of the mixture in largely distant shells must become extremely weak.

As a matter of fact, the conditions are not quite so bad as they seem to be by Fig. 2, because we dispose of data for magnitudes other than the fourth, fifth, and sixth there represented. In consequence of this, the overlap of the absolute magnitudes in two consecutive shells is much more considerable, and we have even some overlap of non-adjacent shells. Still the proof of the identity of the mixture at different distances from the sun will remain defective so long as we have not at our disposal a sufficient number of proper motions of stars of very different magnitude.

If we had such data in sufficient number for stars from the third down to, say, the fourteenth magnitude, then there would be an overlap of not less than six magnitudes even for the first and the seventh shell. We should then be fairly able to dispense with the above hypothesis.

As mentioned before, we need by no means despair of obtaining such data in a near future.

The distribution of the different degrees of luminosity in the universe is not the only thing that can be derived from such data as those shown in Fig. 2.

For we can evidently also determine at once the number of stars pro unit of volume, in other words the density, for any absolute magnitude. For this purpose we have only to divide the number of stars in any one shell by the volume of that shell. For the various shells this volume has been inserted in the figure in a separate table. I have given an example of this determination in Fig. 2. It is as follows:

	Absol. mag. 4.4	*Absol. mag.* 3.4.
Shell VI	Density $=\frac{255}{12.5}=20.4$	$\frac{65}{12.5}=5.2$
Shell VII	Density $=\frac{771}{49.7}=15.5$	$\frac{211}{49.7}=4.25$

Whence:

$$\text{Absolute magnitude } 4.4\ \ldots\ \frac{\text{Density VII}}{\text{Density VI}}=\frac{15.5}{20.4}=0.76$$

$$\text{Absolute magnitude } 3.4\ \ldots\ \frac{\text{Density VII}}{\text{Density VI}}=\frac{4.25}{5.2}=0.82$$

Suppose the star-densities thus determined for all the absolute magnitudes entering into our computations. If, as we assumed before, the mixture of the stars of different absolute magnitude is the same throughout the system, then we must find the change in density from shell to shell the same for every absolute magnitude. In our example we find for the proportions of the densities in shell VII and VI, 0.76 for the stars of absolute magnitude 4.4; 0.82 for those of absolute magnitude 3.4.

They are somewhat different, but not more so than can be explained by the defectiveness of our data. I found fairly the same consistency for the whole of the materials.

There thus provisionally is no reason for abandoning the hypothesis of the constancy of the mixture for different distances from the sun, and this being the case, the change in the density from shell to shell found for the stars of any determined absolute magnitude must at the same time represent the change in the total star-density. We thus see that the law of the total densities for various distances from the sun can be found, although, as long so our data do not embrace the whole range of the absolute magnitudes existing in nature, we cannot tell the total number of stars pro unit of volume, that is, we can only find the relative densities, not the absolute ones.

Taking the mean of the two determinations in the preceding example, we may thus assume:

$$\frac{\text{Total star-density in shell VII}}{\text{Total star-density in shell VI}}=0.79$$

From the whole of the available data was derived the number 0.76.

In this way I find, taking as unity of density the density in the neighborhood of the sun:

π	*Corresp. dist.*	*Star-density*
0″00118	8.5	0.162
.00187	5.3	.292
.00296	3.4	.465
.00469	2.1	.684
.00743	1.35	.852
.0118	0.85	.945
.0187	0.53	.984
.0296 to ∞	0.34 to 0.	1.000

This determination is quite provisional because some data have been neglected in its derivation, which must have considerable influence. Still, always granting the validity of our premises, there can be no doubt of the general course of these numbers.

In the mean of the whole sky we find a regular thinning out of the stars as we recede further and further from the sun. The thinning out is hardly perceptible as long as the parallax is upward of 0″01.

We might now look at another face of the question. The use of the method just now explained is not necessarily confined to the sky as a whole, but is applicable as well to separate parts of it.

So, for instance, we may derive the laws of the mixture and that of the densities separately for stars in the Milky Way and for those at considerable distance away from that belt. A few years ago I carried out such an investigation, but for the same reason that then made me refrain from publication, I shall not now communicate results. The reason is that some difficulties became apparent which make the results seem doubtful. It may be sufficient here to have directed the attention to the fact that, granting our fundamental hypothesis, that is, the hypothesis that the real proper motions do not favor any particular direction, we must be able to find out the real arrangement of the stars in the Milky Way and outside that stratum.

Before discussing the difficulty here alluded to, I shall state another difficulty, which, though it will not necessarily hinder us from unraveling the mystery of the structure of the Milky Way, may well lead us to doubt the validity of our conclusions in regard to the change of star-density with increasing distances from the sun. I am speaking of the absorption of light in space.

How fundamental the question of the absorption of light is for the determination of the star-density appears from Fig. 2. The number of stars of any apparent magnitude present in any one shell is known independently of any consideration of absorption of light. Whether there is appreciable absorption or not, there are 771 stars of the sixth apparent magnitude in shell VII. But if there is absorption to the amount of 1.8 magnitudes per unit of distance, as recently proposed by Comstock, then, as the mean distance of shell VII is 2.13, the light of the stars of this shell will be diminished by about $2.13 \times 1.8 = 3.8$ magnitudes.

To appear to us as stars of the sixth magnitude these stars must therefore have, not the absolute magnitude 4.4, which we had to assume for them in a perfectly transparent space, but 3.8 magnitudes brighter. We thus would find these stars to be of absolute magnitude 0.6 in the mean.

For the several shells these new absolute magnitudes, corresponding with apparent magnitude 6, have been inclosed in squares in Fig. 2.

Does it follow that in the same space in which in a transparent universe we should have 771 stars of absolute magnitude 4.4, we must now assume the presence of 771 stars of absolute magnitude 0.6? Not generally; for it must be evident that in the theory which assumes absorption, the thickness of the spherical shells of Fig. 2 does no longer correspond with just one magnitude. It can be easily proved,[1] however, that, if the proportion of the total number of stars of two consecutive absolute magnitudes is a constant, and this condition is approximately satisfied for the all-important magnitudes, then we shall indeed have in shell VII just 771 stars of absolute magnitude 0.6; in shell VI just 901 stars of absolute magnitude 3.0, and so on.

Now, whichever theory we adopt, the stars of absolute magnitude 4.4 will be found to be about 200 or 300 times more numerous than the stars of absolute magnitude 0.6.

For the density of intrinsically equally bright stars, therefore, also for the total star-density in shell VII, we shall thus have to assume a value about 200 or 300 times higher in the theory which assumes absorption than we should in the theory which neglects it.

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[1] See this proof, *Astronomical Journal*, no. 566.

For shells at a smaller distance the difference of the density in the two theories becomes rapidly smaller, to vanish altogether in the immediate vicinity of the sun.

On the other hand, this difference increases with enormous rapidity for greater distances. For a distance of 10 units we are already led to a density of over 20,000,000,000 times that which would be found for a transparent universe.

I think that we are justified in rejecting a value of the absorption leading to such results. With an amount of absorption ten times smaller, however, we find, for distance 10, a star-density nearly equal to that in the vicinity of the sun, a state of things against which there can be no *a priori* objection.

From considerations like the preceding it must be evident that we cannot hope to get a thorough insight into the real structure of the universe without attacking this problem of the absorption of light in space. Not to argue in a vicious circle, the determination of its amount has to be independent of the star-density at different distances from the sun.

I shall not stop to consider a method for this determination, which I have explained elsewhere.[1] It may suffice to say that, here again, we can only arrive at a satisfactory solution when we have at our disposal the necessary data for the proper motions of very faint stars.

In what precedes we have considered how we may hope to find: (1) the arrangement of the stars in space; (2) the frequency of different degrees of luminosity. We thall hardly succeed in deriving in the same way what we may call the law of the velocities, that is, we shall not be able to find the frequency with which determined velocities occur in the stellar world.

This law, however, is readily obtainable by spectroscopic observation. If from the directly observed radial velocities we subtract that part which is due to the motion of the solar system, and which, since Campbell's observations, is known with very respectable precision, we obtain at once the real velocity of the stars in the line of sight. Simple countings will thus furnish us immediately with the frequency of different values of this component. Hence, adopting our fundamental hypothesis, we may readily derive the frequency of the total velocities themselves, by considerations founded on the theory of probabilities.[2] The following table shows what I derived, in a somewhat indirect way, from the results published by Campbell;[3]

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[1] See *Astronomical Journal*, no. 566.

[2] *Publications of the Astronomical Laboratory at Groningen*, no. 5, pp. 11, 12.

[3] *Astronomical Journal*, XVIII, p. 80.

Total velocity	*Frequency*	
0 to 10 kil. pro sec.	0.025	
10 to 20	.145	
20 to 30	.252	
30 to 40	.259	Sun's velocity 20 Kil. pro sec.
40 to 50	.178	
50 to 60	.093	
60 to 70	.035	
70 to 80	.011	
80	.002	
	1.000	

In the same hypothesis we may also derive the law from the astronomical proper motions. Time does not allow, however, to enter into this matter, which I have tried to explain elsewhere.[1]

The time that remains I would rather devote to some considerations about the fundamental hypothesis itself. In what precedes I have sketched what, in my opinion, is a good plan of attack of the sidereal problem. The ease with which such problems as, for instance, the much-debated question about the most general structure of the Milky Way, may be settled makes such a plan very attractive.

The fundamental hypothesis on which the whole investigation rests has already done good service and has led to results which are pretty generally accepted. But still — everybody must feel that here lies the weak point of the method. As far as I know, no proof of its general correctness has as yet been attempted, not even within the limits in which such proof seems feasible without serious difficulty.

That there must be divergences in detail seems extremely probable. That there is a certain *a priori* probability that these divergences may be considerable cannot be denied.

What is more important still:

Every astronomer who has devoted much thought and time to the study of the proper motions must be aware of the fact that there remain not inconsiderable anomalies. They prevent him from accepting our fundamental hypothesis on other terms than as a provisional one, to be used for want of a better.

If we base our study of the structure of the universe on this hypothesis, we must do it on the principle that out of several evils we should choose the least. Conceding all this, does it follow that we have to accept the conclusion of a critic who denied any astronomical interest to any research based on this one hypothesis?

I believe not.

Are the objections sufficient to make us neglect the whole of the data furnished by the proper motions?

[1] *Publications of the Astronomical Laboratory at Groningen*, no. 5.

This would be very serious.

For my part, I take these data to be the most important we have, an importance that will immensely increase as time advances. Moreover, the problem before us is of such difficulty and our means of attack so slender that it seems downright sin to neglect any data at our disposal. Besides, even if we neglect the proper motions and confine our attention to the magnitudes and numbers, we do not escape the necessity of hypotheses not better founded than that of the random distribution of the directions of the motions. It must be owing to this scarcity of data and to the hypotheses introduced that Seeliger has brought out a law of the densities[1] which astronomers will hardly be inclined to accept without strong further confirmation, because it assigns to the sun a very exceptional place in the system.

What then? Must we be content to sit absolutely idle, saying that the time has not come to make a beginning?

What astronomer of the present day will feel inclined to have this view?

I think that we are perfectly justified in starting from an hypothesis which has already won its spurs, which has not been shown to clash with observed facts, and to develop its consequences to the utmost.

If in doing this we continue to be able to represent all the known facts, our confidence in the hypothesis will have been strengthened, and we may use it with a lighter heart in further research.

If, on the other hand, we are thus led into evident contradiction with the observations, the hypothesis will still not have been unproductive. For it will have called our attention to anomalies, the knowledge of which will be helpful in replacing our hypothesis by another which will embrace them. Such anomalies have indeed shown themselves earlier, and to a far greater amount, than I had expected.

In trying to derive the law of the velocities, and again in trying to apply the method separately to the Milky Way and to other regions, anomalies were found which in the end turned out to have a very systematic character. This systematic character is so pronounced that it is in the highest degree surprising that they have escaped notice so long.

Still, as far as I know, such is the fact, with a single exception. To Kobold belongs the honor of having, as early as 1895, called attention to a fact which proves that our fundamental hypothesis must very sensibly deviate from the truth, and which, if he had tried to separate it more effectively from the effect of the solar motion, would probably have led him to the same conclusion which I am now about to submit to you.

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[1] See *Betrachtungen üb. die räuml. Vertheil. des Fixsterne.* Abh. der k. bayer. Abh. der Wiss. II, Cl. XIX, Bd. III, Abth. page 603.

In order to show clearly the anomaly in the distribution of the proper motions here alluded to, it will be necessary to call to mind how this distribution must present itself if our fundamental hypothesis is really satisfied.

For this purpose consider a great number of stars, very near each other on the sphere. For the sake of convenience we shall assume them to be all situated in the same point S (see page 399, Fig. 1, P) of the sphere, though not at the same distance from the solar system.

The peculiar proper motions of these stars shall be distributed somewhat in the manner indicated in Fig. 1, P. Now, as explained before, if we compose the peculiar motions SB, SC, with the parallactic motions which are all directed along Sr, we get the really observed motions Sb, Sc — which have been represented in Fig. 1, Q.

From this it must be evident that, whereas, according to our fundamental hypothesis, the distribution of the peculiar proper motions will be radially symmetrical, this symmetry will be destroyed for the observed proper motions. There will be a strong preference for motions directed towards the Antapex (see Fig. 1, Q). One thing, however, must be clear, and we want no more for what follows; it is: that there will remain a bilateral symmetry, the line of symmetry being evidently the line $a\ Sx$ through the Apex, the star, and the Antapex.

Near to this line, on the Antapex side, the proper motions will be most numerous, and they will be greater in amount.

This evident condition of bilateral symmetry furnishes probably the best means of determining the position of the Apex.

For if, from all our data about proper motions, we determine these lines of symmetry for several points of the sky and prolong them, they must all intersect in two points which are no other than the Apex and Antapex.

In trying to realize this plan we meet with the difficulty that we do not find in reality any such perfect symmetry as our hypothesis demands. For the lines of symmetry we have to substitute lines giving the nearest approach to symmetry. Their position will depend, at least to a certain extent, on what we choose to consider as "the nearest approach to symmetry."

If we call the demanded line of symmetry the axis of the x, the line at right angles thereto the axis of the y, then we may, for instance, define the line of greatest symmetry to be that which makes zero the sum of the y's.

The line of symmetry furnished by this definition, if prolonged, will not pass through a single point; they will all cross a certain more or less extended area, the centre of gravity of which might be taken as the most probable position of the Apex.

The position of the Apex being once determined, if we draw the great circles Apex $-$II; Apex $-$III (see Fig. 3), and take these as the axes of the x for the points II, III, . . . then if our fundamental hypothesis is approximately true, we must find the condition of symmetry $\Sigma y=0$ satisfied, with a certain degree of approximation for all the points II, III . . .

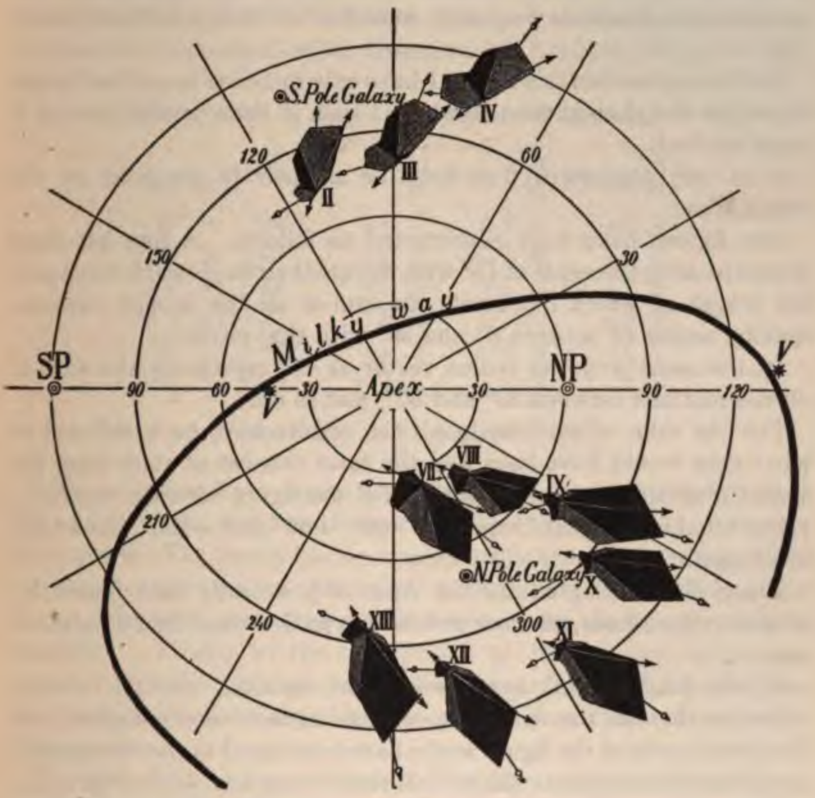

FIG. 3.

Not only that, but we shall have further to expect that any other condition of symmetry will be also approximately fulfilled for every point. Such another condition will be, for instance, that on both sides of the great circles through the Apex the total quantity of proper motion shall be the same; or again that Σx shall be the same on both sides of these circles. How the first of these conditions is satisfied is shown in Fig. 3.

This figure summarizes the more important points in regard to the question in hand, furnished by a treatment of the stars observed completely by Bradley (over 2400 stars). They are distributed over two thirds of the whole of the sky. This surface has been divided up into 28 areas. From the stars contained on each area I have derived the distribution of the proper motions corresponding to the centre of the area. How this was done need not be here explained. The whole of the materials were thus embodied in 28 figures like those of Fig. 3.

Not to overburden this figure, I have only included ten of the figures for which the phenomenon to which I wish to draw your attention is most marked.

It is very suggestive that these lie all near to the poles of the Milky Way.

The figures have been constructed as follows. A line has been drawn making the angle of 15° with the great circle through the Apex, the length of which represents the sum of all the proper motions, making angles of between 0° and 30° with that circle.

In the same way the radius vector at 45° represents the sum of all the motions between 30° and 60°, and so on.

For the sake of uniformity all the results have been reduced to what they would have been had the total number of stars been the same for all the 28 areas. That part of the figure between the radii-vectors making angles of zero and $-60°$, $+60°$ and $+180°$, have been blackened.

The position adopted for the Apex is practically that found by a variety of methods, all more or less akin to that described a moment ago.

If our fundamental hypothesis were satisfied, and if, in consequence thereof, the symmetry of our figures were complete, the blackened parts of the figure would have been equal to the corresponding lighter-tinted parts. (This ideal case is represented in Fig. 1, *R*.)

The real state of things is something quite different, and, what is all-important, we see at once that the divergences are strikingly systematic. The figures at each pole of the Milky Way show them in nearly every particular of the same character. Near the North Pole the blackened parts are invariably much greater; at the South Pole the case is reversed.

At a first glance the difference of the more extensive parts on the side of the Antapex is probably more conspicuous. As a matter of fact, however, the difference between the smaller parts is by no means less important.

For many of you the way in which the second of the above conditions is fulfilled, or rather not fulfilled, will be still more convincing.

For each of the 28 regions the mean value of the x component of the proper motions has been computed, separately for those lying on the two sides of the great circle through the Apex. Let x_R, x_L

denote: mean x of the proper motions to the right, resp. to the left of the great circles through the Apex.

If there were real symmetry, the two ought to show insignificant differences.

The following table shows the actual value of the differences. Mean values were computed for different galactic latitudes by combining the results of regions at equal distance from the Milky Way.

Mean value of $x_R - x_L$ *(centennial motions).*

Apex A	273°6	291	276
Gal. lat. *D*	+29	+34	+19
−40° to −90°	+4″.2	+3″.6	+3″.2
−20 to −39	+2 .2	+1 .6	+2 .4
0 to −19	+1 .8	+1 .2	+1 .4
+ 1 to +20	0 .0	−1 .0	+0 .3
+21 to +40	−2 .4	−2 .5	−1 .7
+41 to +90	−4 .8	−3 .8	−4 .9

Mean absolute value of x=3″7.

This table has been separately derived for stars of Secchi's first and second type, and for those whose spectrum has not yet been determined. The result has been practically the same for all. Also the other component of the proper motions, at right angles to the former, has been investigated. Divergences are shown of a similar character. Finally in the distribution of the *numbers* of proper motions over the four quadrants, the phenomenon is as evident as it is in our table of the x's. About its reality there thus cannot be the slightest doubt.

To what is it to be attributed? The amount of the divergences summarized in Fig. 3 and in our table is so enormous that an explanation of them by an uncertainty of the precession or about the systematic corrections required by Bradley's observations is at once excluded. Besides, for both these elements, the best available values have been used.

More probable seems an error in the adopted position of the Apex. A glance at our Fig. 3 may even perhaps seem to favor such a view, though a closer examination will show that by displacing this point we shall certainly not succeed in making the phenomenon disappear. To show this more convincingly I have repeated the calculations on which our last table rests for two other positions of the Apex, one differing widely in Right-Ascension, the other in Declination. The results are shown in the table. They are practically the same as before.

Or have we to do with a common motion of the whole of the stars which have contributed to any one of our figures? Such an explanation, too, fails. Systematic motion of this kind will make the lines of symmetry diverge from the great circles through the Apex. I therefore investigated what becomes of our results if, for each of our 28 areas, I took for the line of symmetry, not the great circle through the Apex, but the line which, for every particular area, satisfied rigorously the condition $\Sigma y=0$.

Even with regard to these lines the character of the phenomenon as shown by our table is not changed. This proves the inadmissibility of an explanation by local common motion. As, moreover, in this case the adopted position of the Apex plays no part whatever, it proves, even more conclusively than the preceding consideration, that the phenomenon exists independently of errors in the determination of the Apex.

In order to find out, then, what may be the real cause of it, I finally set to work as follows: —

I took in hand first the distribution of the numbers of the proper motions over the angles of position counted from the line towards the Antapex. The results found for all the regions lying nearly at the same or at supplementary distances from the Apex (results which would have been identical, had our fundamental hypothesis been satisfied) were then combined. So, for instance, were the results of 12 such areas as those of Fig. 3, of which the sine of the distance from the Apex lies between 0.90 and 1.00, summarized in a single set of results. This set proved to be all but perfectly symmetrical and duly gave the maximum frequency for the direction towards the Antapex. For these reasons I felt myself justified in provisionally adopting the set as representing the *normal* distribution for the corresponding distance from the Apex. That is, I supposed that this distribution would nearly represent the distribution corresponding to a set of proper motions really fulfilling the fundamental hypothesis, cleared of the inequalities which it is our purpose to find out.

In the possession of this normal distribution we now at once obtain these inequalities separately by simply subtracting the normal number from the corresponding ones found directly from observation for the separate regions.

It thus appeared that these inequalities consist in a manifest excess of proper motions in certain determinate angles of position.

These favored directions have been carefully determined for each of our 28 areas. The greater part of them clearly show two favored directions; for a minority but one of the maxima is well developed.

Entering these favored directions on a globe, it appeared at once that they all converge with considerable approximation towards two points of the sphere. Here we have a clear indication that we have to do with two star-streams parallel to the lines joining our solar system with these two points.

A somewhat different consideration, though also not quite rigorous, may help to make the character of the phenomenon still more evident. The separate figures for the several areas of Fig. 3 show that the symmetry line of the direct motions (calling direct the motions away from the Apex) does not generally coincide with that of the retrograde motions.

The symmetry lines of the direct motions can evidently be determined with considerable precision. It appears that they converge very nearly to a single point of the sphere. This point is some 20 degrees away from the Antapex. The symmetry lines of the retrograde motions cannot be determined with the same accuracy. Still these too converge with some approximation towards a single point, some 75 degrees away from the Apex.

From Fig. 3 may be judged with what approximation the symmetry lines converge to the same points. The open arrowheads on the side of the direct motions all point to absolutely the same point of the sphere. Similarly the arrowheads on the side of the retrograde motions. These arrowheads would completely coincide with the lines of symmetry if these indeed accurately intersected in one point of the sphere.

The divergence is quite small for the direct motions, and satisfactory for the retrograde ones.

We thus in reality have determined the Apex of the solar motion separately from the stars having direct motion and from those having retrograde motion. Instead of finding the same point (or opposite points), we find two points lying about 125° apart.

We will conclude that there are two sets of stars. The motion of the sun relative to the mean (the centre of gravity) of the one set differs from that relative to the other set.

It follows that the one set of the stars must have a systematic motion relative to the other.

Owing to the not rigorous character of the methods, the two points of convergence found in the two ways just described differ not inconsiderably. The mean of the two determinations gives for the position of the one a point 7 degrees south of α Orionis, for the other a point a couple of degrees south of η Sagittarii. We may accept the directions towards these points as a first approximation for the direction of the two star-streams. That they may be found to be still considerably in error matters little.

But it is important to note that the directions are only apparent directions, that is, directions of the motions relative to the solar system.

If it is true that two directions of motion predominate in the stellar world, then, if we refer all our motions to the centre of gravity of the system, these two main directions of motion must be in reality diametrically opposite. For the sake of brevity I shall call the points of the sphere toward which the star-streams seem to be directed the *vertices* of the stellar motion.

The apparent vertices were thus provisionally found to lie south of α Orionis and η Sagittarii. Knowing with some approximation both the velocity of the sun's motion and the mean velocity of the stars, it is easy to derive from the apparent position of the vertices their true position, which must lie at diametrically opposite points of the sphere.

Having once got what I considered to be the clue to the systematic divergences in the proper motions, and having at the same time obtained an approximation for the position of the vertices, I have made a more rigorous solution of the problem.

The existence of two main stream-lines does not imply that the real motions of the stars are all exclusively directed to either of the two vertices; there only is a decided preference for these directions. I have assumed that the frequency of other directions becomes regularly smaller as the angle with the main stream becomes greater, according to the most simple law of which I could think, which makes the change dependent on a single constant.

I have as yet only finished a first approximation to this solution. The result is that one of the vertices lies very near to ξ Orionis. ($\alpha_{1810}=6^{n}\,2^{m}$; $\delta_{1810}=+13^{\circ}5$). The other, diametrically opposite, is not near any bright star. They have been represented by the letter V in Fig. 3. They lie almost exactly in the central line of the Milky Way. Adopting Gould's coördinates of the pole of this belt, I find the galactic latitude to be two degrees. I shall pass over the other quantities involved. I shall only mention that the way in which I conducted the solution points to the conclusion that all the stars without exception belong to one of the two streams.

To my regret I cannot give the detailed comparison of theory and observation, because the detailed determination of the distribution of the proper motions from the data of our solution is such a laborious question that I have not yet made it, and would rather defer it till the real existence of the streams shall have been put beyond reasonable doubt by other observations presently to be considered. I shall only state that by this provisional solution the total amount of dissymmetry for our 28 regions is reduced for the x components as well as for the y components to about a third of the amount they reach in the hypothesis of the random distribution of the directions. Moreover, they have lost their systematic character.

The observations alluded to are those of the radial velocities.

I suspect that the materials for a crucial test of the whole theory by means of these radial velocities are even now on hand in the ledgers of American astronomers. Alas, not yet in published form.

It is this fact which has restrained me till now from publishing anything about these systematic motions, which in the main have been known to me for two years.

If I do not hesitate to publish them now, it is in the hope of eliciting these spectroscopic data, without which a further development of the theory had perhaps better come to a standstill.

If these spectroscopic observations confirm the theory, we may safely go on.

If they do not; they will undoubtedly help to find the true explanation of the dissymmetries summarized in our figures, the real existence of which is out of the question. Further labor devoted to a false theory would be thrown away.

In the mean while it seems well worth the trouble to see what evidence can already be got on the question, even from the scanty materials which have become public property.

Unfortunately we here meet with some difficulties, which singularly diminish the value of any conclusions that might otherwise still have considerable weight.

First, we have to exclude a relatively large number, which, probably or certainly, do not give a fair idea of the whole. As such I consider the stars only observed because of their excessive astronomical proper motion, or selected from a larger list on account of exceptionally large velocity. Further, the Orion stars which seem to be nearly at rest in space; their relation to the system must be somewhat exceptional.

What remains are 78 stars. I have added 46 spectroscopic binaries, though the true velocity of their centres of gravity has been determined only in a few cases. I was mostly compelled to adopt as such the mean of the greatest and smallest of observed velocities.

Small though the collection be, it still offers one formidable difficulty: Great part of it belongs to the very brightest stars in the sky. For these Campbell has discovered the most important fact that they have smaller motions than the mean of the fainter stars.

What may be the cause?

Light will be thrown thereon if more ample data confirm what I found from my scanty store, apparently even more decisively than Campbell's phenomenon, viz., that these stars also lead to a very small velocity of the solar system. For this would make us conclude that the stars nearest to the solar system partly participate in its motion. The conclusion is strengthened by various considerations into which time does not allow me to enter now. On the other hand, there are very serious though perhaps not insuperable objections, which would rather make us seek an explanation in quite another quarter and which at least compel us to wait for further confirmation.

But, whatever may be the cause of the phenomenon, whether it be cosmical or even only instrumental, we must expect that the spectroscopic observations of the bright stars will show the phenomenon of the star-streams less strongly than will the observations of fainter stars; and as our list is made up in great part of such bright objects we must expect to find their influence shown in a somewhat less marked degree than we should be led to imagine from the considerations of this lecture, which are based on the whole of the stars down to the ninth magnitude.

Now this is just what we find to be the case.

I find, arranging in order of the distances from the nearest of the vertices:

Real velocity in the line of sight
(*Kilom. pro sec.*)

Mean dist. from nearest vertex	*Abs. veloc.*	*Number of stars.*	*Theor.*	0.827 × *Theor.*
31°	17.37	(49)	21.75	17.99
59	13.78	(30)	15.98	13.22
79	11.16	(45)	13.15	10.87
Mean	14.25		17.23	14.25
Amplitude	6.21		8.60	

The phenomenon is clearly shown. The observed numbers, however, are only nearly 83 per cent and the observed amplitude but 72 per cent of the theoretical value.

A small but independent contribution is furnished by nine stars, of which the radial velocity has been published on account of its unusually large amount. If our theory is correct, the largest radial motions must be far more numerous near the vertices than at a greater distance.

The nine stars in question are more thickly crowded (if such a word may be used of so small a number) within 43 degrees from the vertices than in the rest of the sky, in the proportion of three to one.

Taking the evidence for what it is worth, we may say that it confirms the theory. The proof is not convincing, however, and I wish to express the hope that those who are in the position to test the whole theory by more extensive and more reliable materials will not neglect to do so.

A few hundreds of stars not pertaining to the Orion stars and fainter than magnitude 3.5 must probably be sufficient for the purpose. Even if the test is fairly stood, we have certainly to see in the present theory no more than a second approximation to the truth. We shall have to develop its utmost consequences.

If we do this the time will probably come when we shall again be led to evident discord with observation. The discordance will point the way to a modified theory, which will be accepted as a third approximation. We must thus come nearer and nearer to the truth.

The theory thus may undergo a series of changes but the principle must remain unaltered: *our distances shall be measured by the parallactic motion.*

Meanwhile, in order to give a solid basis to such theories, we want new data.

In my opinion the most decisive advantage of such theories as the above, defective though they may still be, lies for the present moment in this, that they point out which are the data most wanted for the further development of our knowledge of the structure of the universe. They put definite problems for the solution of which definite data of observation are wanted. The practical astronomer may thus find reliable guidance in the preparation of his working programme.

This is not the place to inquire into what these desiderata may be. Still even this lecture has brought us more than once face to face with difficulties which for their satisfactory solution demand observational data for very faint stars.

Their number is so enormous that their complete observation is out of the question. Happily the purposes of statistical investigation are nearly as well served by specimens so chosen that we may safely admit that they are representative of the whole.

Photography will help enormously in obtaining such specimens.

Specimens giving the number of stars of the several photometrically defined magnitudes.

Specimens giving proper motions.

Specimens giving the class of the spectrum and the radial velocity of stars of as many different magnitudes as are accessible to our observations. Last, not least:

Specimens giving parallaxes.

About these last I may perhaps be permitted to add a few words, bearing on the importance of statistical investigations such as were treated in this lecture.

There is, I think, a very general and very natural feeling that the science of the stars will lack a truly solid basis as long as it is not founded on direct determination of distance.

Mathematically speaking, this may be so.

Practically I think we may be slightly less exacting without serious risk. At all events, we *must* be less exacting if we wish to advance at all.

For the great majority of the stars must certainly have parallaxes far below 0″01. Granting for the moment that we need not despair of measuring such small quantities, even for individual stars, it will certainly only be on the condition that we seek to obtain relative parallaxes. For who will dare to prophesy that we shall be able to measure absolute parallaxes of such an amount in many centuries to come?

Still, it is absolute parallax we want. Therefore the difficulty will remain: how are we to get at the parallax of the stars of comparison?

In my conviction the part to be acted by direct determination of parallax in our investigations about the general structure of the system will be: to furnish the most powerful and most reliable check on the results of the statistical methods.

These methods furnish absolute parallaxes.

For the bulk of the stars the task of the direct determination of parallax will be to decide whether or no they lead to the true differences of parallax.

For them it cannot do more, and the task, as it is, is already by no means a light or an unworthy one.

It becomes enormously more difficult if we take into consideration that very small but interesting part of the stars for which it can do more.

We shall be able to measure directly absolute parallaxes, practically independent of any theory, for the stars nearest to the solar system. Where the parallax exceeds, say, 0″05, the uncertainty of the distance of the stars of comparison will be practically insignificant, if only due care is taken as to the number and choice of these stars.

The number of stars brighter than the tenth magnitude within the distance corresponding with this parallax may be evaluated at some two thousand.

It will be a noble and still not over-heavy task to determine directly the parallax of these stars. This determination would furnish a foundation, independent of any hypothesis, for the astronomy of the regions of the stellar world nearest our terrestrial abode.

The main difficulty, however, lies not in this determination. More difficult it will be to find out *which*, among the million stars brighter than the tenth magnitude, are the two thousand stars to be measured.

I have tried to show elsewhere that even this difficulty, great as it undoubtedly is, may be overcome without overtasking the practical astronomers of the present day. Since that time the kindness of several scientific men has enabled me to ascertain that by using telescopes of longer focus the work may be diminished to a third or less of what it would demand with *carte du ciel* instruments. So in this direction also the future looks hopeful.

At the end of this too long lecture I hope you will agree with the conclusion:

Time has come to undertake a general attack on the mysterious land of the stars. The enterprise is no doubt a very arduous one. It will require unity of scientific effort. Given that unity, however, towards which these congresses must powerfully contribute, hope is brighter now than it was ever before. We may be certain of important results. No doubt, as soon as, with the combined power of scientific men, we penetrate into the promised land, new difficulties will be met, new problems will arise, which will require modifications in the plan of campaign. If it were not so, the struggle would soon lose its fascinating charm.

On the other hand, every inch of firm footing gained will facilitate further operations.

We may be sure that new and unexpected points of view will open on every side. Such is the richness of nature that the experience of Saul, who went out to look for his father's asses and found a kingdom, is rather common in scientific research. Already now that a beginning has been hardly made, there is promise that it will be so in the present case.

For, if it be true that the stellar system, as we know it, consists of two streams coming from widely separate regions of infinite space, and if we find no generic difference between the members of the two, either in their chemical composition or in their motions, then surely we shall have come a step nearer to the full conviction of unity of nature and of its laws throughout the universe.

May these congresses promote in this field of work the same coöperation and emulation that they are sure to bring about in so many other fields.

SHORT PAPERS

Professor R. G. Aitken, of Lick Observatory, read a paper before this Section "On Double Stars," particularly describing the work in progress at the Lick Observatory, and making some suggestions as to the lines of future investigations that promise the largest returns for time and labor invested.

Rear-Admiral C. M. Chester, U. S. N., of the United States Naval Observatory, read a paper upon the work of the Naval Observatory at Tutuila, Samoa, particularly outlining the work assigned to this Observatory in 1896 by the directors of the nautical almanacs of the United States, Great Britain, France, and Germany in the observation of stars to the south of the equator.

Professor A. O. Leuschner, of the University of California, read a paper "On the General Applicability of the Short Method of Determining Orbits from Three Observations."

Professor F. R. Moulton, of the University of Chicago, presented a paper on "The Rôle of Celestial Mechanics in Astronomy." This paper was divided and discussed under five separate heads, as follows:

(1) The Science of Astronomy.
(2) Constituency of Theories.
(3) Indirect Tests of Theories.
(4) Direct Tests by Predictions.
(5) Results Inaccessible to Direct Observation.
(6) Secular Consequences of Minute Influences.
(7) Whence and Whither.

Superintendent O. H. Tittman, of the Coast and Geodetic Survey, read a paper "On the Accuracy Attained in Geodetic Astronomy." The object of the paper was to state some of the results of an inquiry into the present methods of Geodetic Astronomy, the term being here used to include the astronomic observations made for one or more of four purposes: to determine the figure and size of the earth, to fix astronomically certain points on a chart or map in advance of continuous surveys, to fix points in a political boundary, or to determine the variation of latitude.

Professor Ernest W. Brown, of Haverford College, contributed a paper "On the Completion of the Solution of the Main Problem in the New Lunar Theory," in which were given briefly some ideas of the methods used and results obtained during the special study of the subject of the last twelve years.

SECTION B — ASTROPHYSICS

SECTION B—ASTROPHYSICS

(*Hall* 9, *September* 21, 3 *p. m.*)

CHAIRMAN: PROFESSOR GEORGE E. HALE, Director of the Yerkes Observatory.
SPEAKERS: PROFESSOR HERBERT H. TURNER, F. R. S., University of Oxford.
PROFESSOR WILLIAM W. CAMPBELL, Director of the Lick Observatory.
SECRETARY: MR. W. S. ADAMS, Yerkes Observatory.

THE RELATIONS OF PHOTOGRAPHY TO ASTROPHYSICS

BY HERBERT HALL TURNER

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THE European astronomers here present have to thank the organizers of this Congress for much more than their hospitable invitation to attend it, and the opportunities thus afforded of meeting here in St. Louis so many men eminent in their own or other branches of knowledge: over and above this they owe to them opportunities of seeing the great observatories which have developed so rapidly in this country during the last quarter of a century, and of admiring at close view the resources and the work of which the fame had already reached us across the Atlantic. This is not the time or the place for any account of what we have seen and learned; but not to put on record a word or two of appreciation of the great works accomplished, and of that munificence on the part of American citizens which has rendered them possible, would be indeed an omission. We from Europe are, in at least one respect, critics well qualified to judge whether an adequate return is being obtained for endowments such as have recently fallen to the happy lot of American astronomers, for most of us have had some practice in the use of such endowments — *hypothetically*. The constraints of more modest equipments have inevitably suggested plans for work on a larger scale — observatories-in-the-air which our imaginations fill with beautiful and novel apparatus, where the preliminary trials are always successful and no mistakes are made. We come to you accordingly prepared to judge what we see by comparison with a very high standard, and you may well be content with the commendation which we offer unstinted. We rejoice to think that, in the presence of the new and vast possibilities opened up by the gradual accumulation of facts during the last century, by the invention of the spectroscope, and by that of the photographic plate, astronomy should be so fortunate as to receive valuable aid just at a time when it is so urgently needed. It may be well for us to glance for a moment on the other side of the picture, and to wonder what would have been the course of events if this timely aid had not come. How would astrophysics, the newborn child of astronomy, have been nourished? We can scarcely think that it would have been allowed to want for nutriment, but whatever was given to it must inevitably have been withdrawn from the scanty stock of the parent science; either parent or child, if not both, must have shown signs of starvation. This danger is by no means entirely averted even yet; the needs of both, especially of the youthful astrophysics, are increasing daily, as in the case of any other young and healthy organism. The future is not free from anxiety; but that the present is not actually a time of distress is largely due to the generosity displayed towards our science on this side of the Atlantic.

I am tempted to make a remark regarding another science, suggested by the above considerations in conjunction with incidents of travel. No one can cross this great continent and note the extraordinarily rapid spread of civilization, without feeling his interest drawn forcibly to the remnants of the former state of things; to the few remaining native tribes and the monuments of their ancestors scattered through the land. No man of science, whatever his main interest may be, can be insensible to the vital importance of securing permanent records of these vestiges before they inevitably perish. No astronomer who is properly grateful for the endowment of his own science in time of need can fail to hope that the science of anthropology may be equally fortunate at a most critical juncture. I have not the means of knowing whether the vanishing opportunities are being properly cared for: I earnestly hope it may be so; but if it is not, surely this great assembly of men from all sciences and nations could not unite to better purpose than to urge on the American nation the supreme importance of special assistance to anthropology at the present time. We all have needs, even pressing needs, but the pressure is not usually of this kind. The subject-matter of our investigations is not evanescent; we astronomers, for instance, know that if we must perforce put aside a particular investigation for lack of means, fifty years hence a more fortunate successor will find the eternal heavens little changed for the same purpose. But the anthropologist cannot wait; with him it is now or never, and science would be a poor thing indeed if we could not be so unselfish as to recognize his needs as more urgent than our own. Is it too much to hope that, even before we leave this hospitable city, we may have some assurance that full justice shall be done in this matter?

It is a familiar fact that there are epochs in the history of a science when it acquires new vigor; when new branches are put forth and old branches bud afresh or blossom more plenteously. The vivifying cause is generally to be found either in the majestic form of the discovery of a new law of nature, or in the humbler guise of the invention of a new instrument of research. The history of astronomy has been rich in such epochs, notable among them being that when Newton announced to the world the great law of gravitation, and that when Galileo first turned his telescope to the skies.

We have within the last half-century been fortunate enough to include another great epoch in astronomical history, characterized by the birth, almost a twin-birth, of two new scientific weapons — the spectroscope and the sensitive film. It is, of course, somewhat difficult and scarcely necessary to assign an exact date for the origin of either of these; the spectroscope was perhaps first systematically used on the heavenly bodies by Huggins, Rutherfurd, and Secchi in the fifties, but we may trace it back to the early work of Fraunhofer, who described the spectrum of Sirius in 1817, or further back to the experiments of Newton with a prism; and the *dry* plate, which in particular has conferred such benefits on our science, had of course its precursors in the collodion plate or the daguerreotype. But the greater part of the influence on astronomy of both the spectroscope and the photographic method dates from the time when the dry plate was first used successfully, not much more than a quarter of a century ago; and in that quarter of a century there have been compressed new advances in our knowledge which perhaps will compare favorably with the work of any similar period in centuries either past or to come. It is difficult to estimate at their true value historical events in which we play a part, and any review of such a period undertaken now must be necessarily imperfect, for we are advancing so rapidly that our point of view is continually changing. But it is an encouraging thought that obvious difficulties may enhance interest in the attempt and suggest kindly excuses for its shortcomings.

From the embarrassingly large number of possible topics which the period provides, I have selected that of astronomical photography, and I invite your attention to some characteristic features of the photographic method in astronomy, and some reflections thereupon. It is scarcely possible to avoid repeating much that has been said already, but I hope it will be clear that no claim to original-is advanced; in what follows I wish to claim nothing as mine ve its imperfections.

The advantages of the photographic method, which attracted ttention from the first, may be grouped under three heads — its power, its facility, and its accuracy. The lines of demarcation are ill-defined, but the classification will help us a little, and I proceed to consider the groups in this order.

The immense *power* of the photographic method as compared with the eye arises from the two facts that (*a*) by the accumulation of long exposures fainter and fainter objects can be detected, and that (*b*) large regions of the heavens can be recorded at the same exposure. No property of the photographic plate has excited more marvel than the former, — that it can detect objects too faint to be seen even by our largest telescopes; objects of whose very existence we were in ignorance and should have remained in ignorance. Early successes have been followed up by others more striking as years have rolled on, as better instruments have been devised, and the patience of the watchers has proved equal to greater strain. It is here that the change from the "wet" plate to the "dry" has proved most advantageous. The possibilities with the former were limited to the period during which it would remain wet; with the latter, exposures may be continued for hours, days, even years — not, of course, continually in the case of astronomical photography, for the camera must be closed when daylight approaches; but it can be opened again at nightfall and the exposure resumed without fault. In this way objects of extraordinary faintness have been revealed to us. When Nova Persei had flashed into brilliance in 1901, and then slowly faded, long-exposure photographs of its region revealed to us a faint nebulous structure which we could never have seen; they told us that this structure was changing in appearance in a manner which it taxed our ingenuity to explain, and about which speculation is still rife. But a greater triumph was to come; even the spectrum of this faint object has been photographed. When we consider that in the spectrum each point of light in the object is enormously diluted by being spread out into a line, the difficulty of this undertaking seemed almost prohibitive; but it was not sufficient to prevent Mr. Perrine, of the Lick Observatory, from making the attempt, and he was deservedly rewarded by success. I may be wrong in regarding this success as the high-water mark in this direction at the present time, and it will probably be surpassed by some new achievement very shortly; but it will serve to illustrate the power of photography in dealing with faint objects.

But may we here pause for one moment to marvel at the sensitiveness of the human eye, which is such that it is, after all, not left very far behind in the race? The eye, sensitive as it is merely to transient impressions, is no match ultimately for the plate, which can act by accumulation. But with similar instruments the plate must be exposed for minutes or even hours to seize the impression of a faint object which the eye can detect at a glance. There seems to be no reason in the nature of things why the eye should not have been surpassed in a few seconds; and in the future the sensitiveness of plates may be increased so that this will actually be the case, even as in the past there was a time when the sensitiveness was so small that the longest exposure could not compete with the eye. But this time is not yet come, and at the present moment the eye is still in some departments superior to its rival, owing to this very fact, that though it can only see by glances, it can use these glances to good effect. In the study of the planets the more clumsy method of the photographic plate (which, by requiring time for the formation of the image, confuses good moments with bad) renders it almost useless as compared with the eye; and again, we have not as yet used photography for daylight observations of stars.

But there is another direction in which the photographic plate is immensely superior to the eye in power; it can record so much more at once.[1] In the able hands of Prof. Barnard, Dr. Max Wolf, and others, this property of the plate has been used to record the presence in the sky of vast regions of nebulosity such as, we may safely say, the eye would never have satisfactorily portrayed, not altogether because of their faintness (for in one of his papers Professor Barnard tells us that he was actually led to photograph such a region because he had become vaguely conscious of it by eye-observation), but because of their diffusion. It is noteworthy that these beautiful photographs were taken with comparatively humble instruments, and we may be as yet only on the threshold of revelations still to be made in this direction.

Secondly, the photographic method represents a great advance in facility of manipulation. A familiar example may be taken from the domain of planetary discovery. In old time, to recognize a new object among numerous fixed stars, it was necessary either laboriously to map out the whole region, or to learn it by heart, so that it was practically mapped in the brain. Now all this labor is avoided; two photographs of the same region, taken without any strain on the memory or the measuring ability of the observer, can at a glance, by a simple comparison, give the information that a strange object is or is not present, — information formerly obtained at so much cost. Sometimes, indeed, the cost was so great that the information was not obtained at all. For fifteen years Hencke searched without success for a planet, and for nearly forty years after the discovery of the first four small planets in 1807 no further discoveries were made, though hundreds were constantly crossing the sky, and a dozen new planets are now found every year with little trouble.

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[1] This property has been beautifully illustrated by a lecture experiment of Prof. Barnard. He throws on the screen a picture of a large nebula which the photographic plate has no difficulty in portraying all at once; but the picture is in the first instance covered up by a screen, except for a small aperture only, and this aperture, he tells his audience, represents all that can be seen by the eye at one time, using the giant telescope of the Yerkes Observatory. By moving the screen about, different portions of the picture may be viewed successively, as also by moving the telescope about in looking at the sky itself. But what a revelation follows when the screen is removed and the full glory of the nebula is exhibited at a single glance! We can well understand that the true character of these objects was hopelessly misinterpreted by the eye, using the imperfect method of piecemeal observation which alone was formerly possible.

But though this instance of increase in facility is striking, it is far from being the only one or even the most important. Wherever we require a record of any kind, whether it be of the configuration of stars, or of solar spots, or of the surface of the moon, or of a spectrum, the labor of obtaining it has been enormously reduced by the photographic method. Think for a moment of what this means in the last instance only, — think of the labor involved in mapping one single spectrum by eye-observation; of the difficulty of settling by such a method any doubtful question of the identity of certain lines in the spectrum of a star! A few years ago Dr. McClean announced that he had found oxygen in the star β Crucis. Up to that time this element, so familiar to us on this earth, had appeared to belong to us alone in the universe, for in no spectrum had its lines been detected. The proof of its existence in β Crucis depended on the identity of a number of lines in the spectrum with some of those of oxygen; and the measures were sufficiently difficult on a photograph, so that for more than a year the scientific world refused to pronounce a verdict. How long would the case have dragged on if only visual measures had been possible? We may fairly doubt whether a definite conclusion would ever have been reached at all. By the sheer facility of the new method of work we have advanced by leaps and bounds where we could only crawl before.

Thirdly, there has been a great gain in *accuracy* from the introduction of photography; and it is this quality which is above all of value in the science of astronomy.[1] The wonderful exactness of the photographic record may perhaps best be characterized by saying that it has revealed the deficiencies of all our other astronomical apparatus, — object-glasses and prisms, clocks, even the observer himself.

It has almost been forgotten that in the early days the accuracy of a photograph was doubted. Even now it can scarcely be said that we know definitely the stage of refinement at which we must begin to expect irregular displacements of the images from distortion of the photographic film; but we have learned that they do not occur in a gross degree, and that other apparatus must be improved before we need turn our attention seriously to errors arising from such a cause. Consider, for instance, what photography has told us about our optical apparatus, which we regard as having reached a high stage of perfection. We are accustomed to think of properly made optical apparatus as being sufficiently similar in all its parts; it is tacitly assumed in the principle of the heliometer, for example, that one half of the object-glass is sufficiently similar to the other. But a stock adjustment recently adopted in photographing a spectrum for accurate measurement exhibits clearly the errors of this assumption. Photographs are taken of the spectrum through the two halves of the objective; and if they were properly similar the lines in the two halves of the spectrum should fit exactly. A mere glance is usually sufficient to show discordances. It is true that one of the photographs is taken through the thick half of the prism and the other through the thin, so that errors of the prism are included; but these, again, are optical errors. They are, however, not the only sources of error which at present mask photographic imperfections. Glass plates are not flat, and this want of flatness introduces sensible errors. Even with the great improvements in our driving-clocks which were called for immediately photographs were to be taken, — with electrical control and careful watching on the part of the observer, — there is apt to creep in a "driving-error" which gives bright stars a spurious displacement relatively to faint. We must get flatter plates, better driving-clocks, and watch more carefully before we can certainly accuse our photographs of a failure in accuracy. Nevertheless, there are indications that we may be near the limit of accuracy even now. Examination of the réseau lines on various plates appears to show small displacements for which no cause has yet been assigned; and the end of our tether may not be far away. But as yet we have not been pulled up short, and there is hope that the warning may be, as on one or two previous occasions, a false alarm.

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[1] Two things may be measured on a photographic plate — the position of an object, or the density of the image; the former being an indication of its position in the heavens, and the latter of its brightness. With the latter topic I do not propose to deal, for the reason that it is in the hands of a much abler and more experienced exponent; but the former alone will provide enough food for reflection.

Such being the accuracy of the photographic method, it is surprising that it should not as yet have been more fully adopted in that field of work where accuracy is of the greatest importance, — namely, in what is called fundamental work, with the transit-circle or other meridian instruments. The adoption of new methods is always a slow process, and there are at least two classes of difficulties which hinder it. The first class has its origin in the instinctive conservatism of human nature, wherein men of science differ little from their fellows. The second has to do with available capital; and in this respect we are distinctly at a disadvantage compared with other men; for when a new instrument of *general* utility is invented, at once a large amount of capital is invested in working out the details and improving them to the utmost; whereas for a scientific instrument no such funds are available. Think, for instance, of the money spent in perfecting the bicycle, and the time occupied in developing it from the earliest forms to those with which we are now familiar, — from the "bone-shaker" of the sixties, through the high bicycle which we saw twenty years ago, to the modern machine. Think, too, how totally unexpected have been some of the incidents in the history of this machine, — such as the introduction of pneumatic tires, or its use by ladies.[1] In the case of such an instrument, now universally adopted, if rapid development could have been secured by expenditure of money and brains, surely enough of both commodities were forthcoming to attain that end; and yet simplicity and finality have probably not yet been attained in a period of thirty years. When we compare the small amount of money and especially the small number of persons that can be devoted to the perfection of a new scientific method, such as the use of photography in astronomy, it will excite little surprise that progress during the same period of thirty years has been slower. In commerce old machines can be thrown on the scrap-heap when improvements suggest themselves; but who can afford to throw away an old transit-circle? The very fact that it has been in use for many years renders its continued use in each succeeding year the more important from considerations of continuity.

It is doubtless for such reasons as these that little has yet been done in the way of utilizing photography for meridian observation. Although one or two meritorious beginnings have been made, which have sufficed to show that there are no insuperable difficulties in the way, up to the present moment no meridian instrument of repute is in regular work using the photographic method. And this fact cannot, after all, be completely explained by the reasons above mentioned. Opportunities for setting up costly new instruments do not occur frequently in astronomy, but they do occur. In the last decade, for instance, large transit-circles have been set up both at Greenwich and the Cape of Good Hope; but in neither instance has any attempt been made to adopt the photographic method. The Washington Observatory was reconstructed well within the period since the great advantages of photography have been recognized; and yet not even in the United States, the land of enterprise, was a start then made in a direction in which it is certain that we must some day travel. That day has probably been deferred by the stimulation of competing methods which a new one brings with it. When electric light was first introduced into England, the gas companies, stimulated by the stress of competition, adopted a new and improved form of light (the incandescent gas) which put them at a much less serious disadvantage compared with their new rival. So when photography began to show what new accuracy was attainable in measurement of star-positions, it would almost seem as if the devotees of the older visual methods were compelled to improve their apparatus in order not to be left wholly behind in the race. The registering micrometer[1] was produced by Messrs. Repsold, with the astonishing result that the troubles from personal equation, which have so long been a difficulty in all fundamental work, have practically disappeared.

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[1] I have in my possession a copy of a work of reference on cycling, dated no earlier than 1887, in which it is carefully stated as a deliberate conclusion that ladies will never use the machine to any great extent.

This beautiful invention has placed the eye once more in a position actually superior to the photographic plate; for with the eye we can observe stars in daylight, and so secure information of great importance, whereas no photographic method of doing this has, as yet, been devised. And there is also the fact that for faint stars a long exposure would be required for what the eye can accomplish in a few seconds.

Thus in one or two astronomical channels the effects of the rising tide of photography have scarcely yet been felt; but into all the others it has swept with ever-growing force. Looking back over the thirty years of advance, we may be well satisfied. With more funds, and especially with more men, no doubt more could have been done: let us even admit that we might have done better with the same funds and the same limited staff. But on the whole we have been fortunate. At a critical time, when we might have felt the want of larger endowments acutely, the need was almost anticipated by a stream of benefaction. If this stream had its chief source in the United States, its beneficial effects have poured over the whole world; and induced currents have begun to flow elsewhere. We may reflect with thankfulness how much harder our advance might have been but for the noble gifts to the Harvard, the Lick, and the Yerkes observatories: and earnestly hope that the cheerful expectations of a great American astronomer, that these are but the foreshadowing of much larger gifts to science, may be adequately realized.

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[1] We have been accustomed hitherto to determine the position of a star by observing the instant when it crossed a fixed wire; but it has long been known that two different observers record systematically different instants — they have a personal equation. Recently we have learned that this personal equation varies with the brightness of the star observed, and with other circumstances, and to make the proper corrections for it has severely taxed our ingenuity and involved much work. Before the invention of photography, we might well bear this with patience, since it seemed to be inevitable; but the photographic plate, which is free from human errors, offers a way of escape from all troubles — at the expense, no doubt, of some little experimenting, but with every prospect of speedy success. Eye-observation, which had borne this burden so long, must get rid of it if it was to march alongside the untrammeled photographic method; and the surprising thing is that it has actually done so. The adopted device is extremely simple: replace the fixed wire which the star crosses by a wire which moves with the star and registers its own movements. The registering is done automatically; but the motion of the wire is controlled by the observer, and there is still room for a new form of personal equation in this human control. But none manifests itself, probably for the reason that we no longer have two senses concerned, but only one. In recording the instant when a star crosses a wire we employ either the eye and the ear, or the eye and the sense of touch; and personal equation arises from the different coördination of the two senses in different people. But in making the wire follow the star, the eye alone is concerned, and there is no longer any room for difference in "latent period" or other coördination of two senses.

May I now turn to one or two of the problems with which this new development of our work has brought us face to face? They are numerous and serious, and it is impossible to consider many of them, perhaps even the most important of them. One of the most pressing is the problem of rendering generally accessible the vast accumulations of material for study that have been suddenly thrust upon our attention. How are our photographs to be stored, preserved, and published? Even now troubles have gathered, and time will only multiply them. It is many years since Professor Pickering drew attention to the difficulties in storing the photographic plates taken at the Harvard Observatory; when many thousands of photographs have been accumulated, not only the space they occupy, but the actual weight of glass, is an embarrassment. And there seems to be no doubt concerning the duty of accumulation. May I confess an early and mistaken view which I formulated on this matter? I reasoned thus: The proper moment for making use of a photograph taken last night is to-day. It is useless to defer the examination until to-morrow, for there will then be new photographs claiming attention. Hence, it is unscientific to take more photographs than can be dealt with immediately. This seemed to be a plausible argument and to show a way out of the difficulty, for if a photograph had once been adequately examined, it need not be stored so carefully, and there would not in any case be many to store. But Professor Pickering has demonstrated many times over that the view is untenable. By taking photographs almost recklessly, and without any hope of dealing with even a fraction of them, he has created the possibility of tracing the history of celestial events *backwards*. When new objects are discovered he can go to his shelves and tell us how long they were visible previous to discovery: and this information is so valuable that we must certainly arrange our future plans with reference to it. It is quite certain that we must be prepared to deal with enormous accumulations of plates, to store them in proper order, and to catalogue them; and if it has already been found difficult to do this for the collection of a single observatory during twenty years, what can we look for in the centuries to come?

Possibly the second difficulty, that of preservation, may be an antidote to the first. It is by no means certain that our photographs will last long; and if not, there will be a natural limit to the time during which they need be kept. Sir William Crookes has, however, reminded us that by toning them, by substituting sturdy gold for the perishable silver, we may prolong their life indefinitely, though this will, of course, sensibly increase the cost of each plate. As yet I have not heard of any toning process being systematically adopted. Our course is, however, comparatively clear in this direction; it would seem imperative that a selection of the earliest photographs, at any rate, should be carefully toned, so that they may be available for comparison in years as far distant as possible. Although this is a matter of detail, it seems to me to compare in importance with almost any practical question which may claim the attention of astronomers; and if some decision of the kind were the only outcome of this gathering, I think we might be well content with the result.

The question of publication is chiefly one of funds, and is only worthy of special remark because these particular funds are so often forgotten in planning enterprises. I need not labor the point, for the experience of any astronomer will supply him with plenty of instances. The difficulties of publication have much in common with those of storage; they will increase year by year, and even when the money for printing has been found, the storage of publications received from other observatories will itself become an embarrassment. There is, however, one way in which some of the stress may be relieved, namely, by efficient cataloguing. If we have before us a list of all the photographs existing in the world, and know that we can send for a copy of any one of them which may be required, it is no longer necessary to have copies of all. This applies, of course, to other publications as well; and though we may take some time to grow out of the sentimental desire for a complete library, and though the existence of a few such complete institutions may always be desirable, I venture to think that many observatories will ultimately be driven to the plan of acquiring only what is certainly and immediately useful, depending on temporary loans from central institutions for other material.

But there is a class of problems differing totally in character from these practical questions of storage and preservation of plates. A period of suddenly increased activity such as we have been passing through in astronomy is not without important effects on astronomers themselves. The human element in our scientific work is sometimes overlooked, and generally accorded only a subordinate importance; but, coming as I do from an old university devoted to the Humanities, I may be perhaps forgiven for calling attention to a few human considerations. In the first place, I have felt some anxiety lately for that very important body of astronomers who are sometimes called amateurs, though the name is open to criticism, — those whose opportunities for work are restricted to a more or less limited leisure. It is a body which is somewhat sensitive to the feeling that astronomical work has gone beyond them, that in the presence of large instruments and of the special knowledge acquired by those using them, their own efforts and their own humbler instruments are no longer of any value. If I am right in supposing that this feeling has been called into existence lately by the rapid advances made in photography, it is certainly not for the first time. At previous epochs this diffidence has found expression, and has, I am glad to say, been met by careful contradiction; but it is necessary to repeat the expostulation again and again, for the anxiety is apt to crop up with every new development of astronomical activity.

The *early* days of photography were better ones than usual for the amateur; indeed, the introduction of the photographic method is largely due to the work of such men as Rutherfurd and Draper in America, de la Rue and Common in England. But now that we have passed beyond the stage when each new plate taken was a revelation; now that we are tolerably familiar, at any rate, with the main types of possible photographs which can be taken with modest apparatus; more especially now that we have begun to discuss in elaborate detail the measurement of star-positions or of stellar spectra, the old shyness is beginning to crop up again. But it is of the utmost importance that this shyness should be zealously overcome. Perhaps, after all, it is not sufficient to assert that there is still good work for amateurs to do, nor even to mention a few instances of such work urgently required; perhaps it should be made easier for them to follow what is being done. Especially do we want more and better *books*, written by the best men in each subject. The original memoir, though it may be the proper form of publication for the workers themselves, does not satisfy all requirements. There is much to be done in the way of extension and collation before the work can be presented in a form attractive to those who would gladly keep in touch with it if the process could be made a little easier. Huxley was constantly urging upon scientific men that it was not sufficient to attain results; they must also express them in an intelligible and attractive form. Of course it is not easy for the same man to do both. There are few who could have determined, like Schiaparelli, that the period of rotation of the planet Mercury was eighty-eight days instead of one; but there are fewer still who, after making the discovery, could have given the beautiful lecture which he gave before the King of Italy, developing fully in attractive detail the consequences of the discovery; and yet it is probably true that many more could make, at any rate, an attempt in this direction, if adequate opportunity and inducement were provided. Could not a part of the sums available for the endowment of research be devoted to the endowment of text-books? It is, of course, an inducement to write such a book that it is a good thing well done; but in the case of a scientific worker this is scarcely sufficient, because the same could be said of his continuing his particular work. If we ask him to pause, and render the treasures he has collected accessible to others, there must be some additional inducement. Publishers are not able to offer pecuniary encouragement, because books of the type I have in mind would not appeal to a very large public. But why should they not be subsidized? I do not think it need be a very costly business, if the money were placed in the hands of a central body to issue invitations for books to be written. An invitation would be in itself a compliment; and the actual pecuniary value of the inducement would shrink in importance, just as the actual amount of gold in a medal awarded by one of our leading scientific societies is not very seriously regarded. It may be objected that to ask the best men to write text-books is to set them to inferior work, and so to delay true scientific progress; but are we sure that the real march of science is being delayed? There are pauses in a journey which merely waste time; but there are others without which the whole journey may be delayed or prevented, as when a man should neglect to rest and feed the horse which carries him.

But the development of photography has brought with it much more than a recurrence of diffidence in some amateurs; it has foreshadowed a serious rearrangement of astronomical work generally, — a new division of labor and a new system of coöperation. To quote one notable instance: a very small number of observatories could take enough photographs to keep the whole world busy examining or measuring them, and we are already face to face with the question whether this is a desirable arrangement. Let me give a concrete example of this modern situation. In the winter 1900–01 the small planet Eros offered a specially favorable opportunity for determining the solar parallax, and some thousands of photographs were taken at a number of observatories for the purpose. It is not yet very clear how a definitive result will be obtained from the mass of material accumulated, most of which is being dealt with in a very leisurely manner: but a small portion of it has been discussed by Mr. A. R. Hinks, of Cambridge, and one of the many important results obtained by him in a recently published paper (*Mon. Not. R. A. S.*, June, 1904) is this: that the plates taken at the Lick Observatory are susceptible of such accurate measurement, and so numerous, that a determination of the solar parallax from them alone would have a weight nearly equal to that from the whole mass of material. If the Lick plates can be measured and reduced, it will not much matter if all the others are destroyed. Whence we may deduce two conclusions: first, that it is eminently desirable that these beautiful pictures should be measured and reduced as soon as possible; secondly, that we must consider future plans of campaign very carefully if we are to avoid waste of work and discouragement of workers. It is tolerably easy to reach the first precise conclusion; I wish it were easier to arrive at something more definite in regard to the second. It seems clear that we may expect some readjustment of the relations between the better-equipped observatories and those less fortunate, but it is not at all clear what direction that readjustment should take. One possibility is indicated by the instance before us: the discussion of the Lick photographs was not conducted at the Lick Observatory, but at Cambridge; the price paid for the fine climate of Mount Hamilton is the accumulation of work beyond the powers of the staff to deal with, and the new divison of labor *may* be, for the observatories with fine climates and equipment to take the photographs, and astronomers elsewhere to measure and discuss them. Professor Kapteyn has set us a noble and well-known example in this direction, and in view of the pressing need for a study of many photographs already taken, it is to be hoped that his example will be followed, especially in cases similar to his own, where no observatory is in existence. If in such cases the investigator will set up a measuring-machine instead of a telescope, he will deserve the gratitude of the astronomical world.

But the case is not so clear when a telescope is already in existence. Mr. Hinks had a fine telescope at Cambridge, and it required some self-denial on his part to give up observing for a time in order to discuss the Lick photographs and others. If the accumulations already made, and others certain to be made in the future, are to be dealt with, this kind of self-denial must certainly be exercised, but it does not seem quite clear that it should always fall to the lot of those with a modest equipment. Considerations of strict economy might suggest this view, but there is a human side to the argument which is not unimportant. The danger that the minor observatories should feel their work unnecessary is even graver than the similar possibility in the case of amateurs already mentioned, and calls for prompt attention from astronomers generally, if it is to be averted. It is the more serious because of another set of considerations of a quite different kind, viz., the funds available for research show a rather alarming tendency to accumulate in the hands of a few large observatories, leaving many astronomers who could do useful work without the means of doing it. A conspicuous example is afforded by the present state of the work for the Astrographic Chart initiated in Paris seventeen years ago. On the one hand, a few of the large observatories have easily acquired funds not only for taking and measuring the plates and printing the results, but for publishing an expensive set of charts which will be of very little use to any one; on the other hand, some of their colleagues have found the utmost difficulty in getting funds for even taking the plates; others have got so far but cannot proceed to measure them; and very few indeed have yet funds for printing. If there had been a true spirit of coöperation for the general good in this enterprise, surely some of the funds being squandered on the comparatively useless charts would have been devoted to the proper completion of the only part of the scheme which has a chance of fulfillment. I do not mean to imply that this would have been an easy matter to arrange, but it is noteworthy that no attempt in this direction has been made, and that as a consequence a promising scheme is doomed to failure in one important particular. For though the survey of the whole sky to the eleventh magnitude may some day be completed, it will be sadly lacking in homogeneity. Some sections are finished before others are begun, so that in the vital matter of epoch we shall have a scrappy and straggling series instead of a compact whole.

Coöperation in scientific work, the necessity of which is being borne in upon us from all sides, is nevertheless beset with difficulties, and no doubt we shall only reach success through a series of failures, but we shall reach it the more rapidly if we note carefully the weaknesses of successive attempts. In the particular scheme of the Astrographic Chart, I think an error which should be avoided in future was made by those who have access to the chief sources of astronomical endowment. They have made the enterprise doubly difficult for their colleagues: first, by setting a standard of work which was unattainable with limited resources; and, secondly, by depleting the reserves which might have gone to assist the weaker observatories.

It is easier to draw attention to these modern tendencies than to suggest a remedy for them. It may, perhaps, be questioned whether a remedy is either possible or necessary; it may be urged that it is both inevitable and desirable that astronomical observation should gravitate more and more to those well-equipped observatories where it can be best conducted, and that new resources will obtain the greatest results when added to a working capital which is already large. From the purely economical point of view of getting results most rapidly, these conclusions may be true. But if we look at the human side of the question, I hope we shall dissent from them; if we think first of astronomers rather than of the accumulation of astronomical facts, I hope we shall admit that something must be done to check the excessive specialization and the inequalities of opportunity towards which there is a danger of our drifting. We cannot afford the division of astronomers into two types: one isolated in a well-equipped observatory in a fine but rather inaccessible climate, spending his whole time in observing or taking photographs; another in the midst of civilization, enjoying all the advantages of intercourse with other scientific men, but with no telescope worth using, and dependent for his material on the observations made by others. Some division of labor in this way is doubtless advantageous, but we must beware lest the division become too sharply pronounced. Will it be possible to prevent its undue growth by some alternation of duties? Can the hermit observer and the university professor take turn and turn about to the common benefit? The proposal is perhaps a little revolutionary, and has the obvious disadvantages of inconvenience and expense at the epochs of change; but I do not think it should be set aside on these grounds.

I must admit, however, that I am not ready with a panacea. It has been chiefly my object to draw attention to some modern tendencies in astronomical work, hoping that the remedies may be evolved from a general consideration of them. Such questions of the relationship of the worker to his work are even harder to solve than those we meet with in the work itself. But there is at least this excuse for noticing them on an occasion like the present, that they are, to some extent, common to all departments of knowledge, and our difficulties may come to the notice of others who have had occasion to consider them in other connections and may be able to help us. Or, again, we may take the more flattering view that the human problems of astronomy to-day may be those of some other science to-morrow; for astronomy is one of the oldest of the sciences, and has already passed through many stages through which others must pass. In any case, we must deal with these problems in the sight of all men; and of all the consequences entailed by our lately acquired opportunities, none are more interesting and none can be more important to us than those affecting the astronomer himself.

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THE PROBLEMS OF ASTROPHYSICS

BY WILLIAM WALLACE CAMPBELL

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THE investigator in any field of knowledge must, as the price of success, both comprehend the general principles underlying his special problem, and give constant care to its details. Yet it is well, now and then, to leave details behind and consider the bearing of his work upon the science as a whole. Whether our subject is that of determining the accurate positions of the stars, or their radial velocities, the orbits of the planets, or the constitution of the sun, we are making but minor contributions to the solution of the two great problems which at present compose the science of astronomy. These problems, perhaps the most profound in the realm of matter, may be stated thus:

(1) A determination of the structure of the sidereal universe; of the form of that portion of limitless space occupied by the universe; of the general arrangement of the sidereal units in space; and of their motions in accordance with the law of gravitation.

(2) A determination of the constitution of the nebulæ, stars, planets, and other celestial objects; of their physical conditions and relations to each other; of the history of their development, in accordance with the principles of sidereal evolution; and of what the future has in store for them.

The first problem has for its purpose to determine *where* the stars are and whither they are going. It has been ably treated under the head of astrometry.

The second seeks to determine the nature of the heavenly bodies, — *what the stars really are.* This field of inquiry is well named astrophysics.

The motives of these problems are distinct and definite; but, judged by the ultimate bearing of his results, nearly every astronomer is working in both fields. The astrophysicist borrows the tools of the astronomer of position, the latter uses the results of the former, and *vice versa*. Let me give two illustrations. Astrophysics desires to know the relative radiating power of matter in different types of stars, — the Sirian and solar types, for example. The meridian circle [1] and the telescope [2] discovered a companion to Sirius; the micrometer determined the form and position of the orbits; [3] the heliometer observed the star's distance; [4] and the photometer [5] measured the quantity of light received from it. Computations determine from these data that Sirius is but two and one half times as massive as our sun,[6] whereas it radiates twenty-one times as much light; [7] from which it follows that a given quantity of matter in Sirius radiates many times as effectively as the same quantity of solar matter, — a fact of prime importance in the astrophysical study of all Sirian stars. The parallaxes of the stars are needed by the student of stellar evolution as well as by the student of the structure of the heavens.

Again, the measurement of radial velocities of the stars has been left almost completely to those observers who are especially interested in astrophysical problems and methods, yet it is the student of astrometry who is eager to use their results. The overlapping of the two departments of astronomy is but the symbol of progress.

The term astrophysics is of the present generation, but the beginnings of astrophysical inquiry are somewhat older. Theories of planetary evolution by Kant [8] and Laplace; [9] observations of nebulæ and star clusters by the elder Herschel,[10] and his wonderfully sagacious deductions concerning them; various studies of planetary markings and conditions; systematic investigations of the sun-spots, including Schwabe's discovery [11] of their eleven-year period; — these constituted the main body of the science in 1859. But the spirit of inquiry as to the nature of the heavenly bodies was latent in many quarters; and Kirchhoff's immortal discovery of the fundamental principles of spectrum analysis [12] opened a gateway which many were eager to enter. The spectroscope became at once, and has remained, the astrophysicist's principal instrument. However, the spectrum is not his only field, nor the spectroscope his only tool. Radiation in all its aspects, and the instruments for determining its quantity and quality, are the means to the ends in view. And the great generalizations of scientific truth, the doctrines of evolution and of the conservation of energy, for example, have been no less helpful here than elsewhere.

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[1] Bessel, *Astronomische Nachrichten*, nos. 514, 515, 516; and *Monthly Notices, Royal Astronomical Society*, VI, 139.

[2] *Astronomische Nachrichten*, LVII, 131.

[3] Zwiers, *Proceedings*, Amsterdam Academy of Sciences, May 27, 1899.

[4] *Annals* of the Cape Observatory, VIII, part II.

[5] *Annals*, Harvard College Observatory, XIV, part I, 152.

[6] Auwers, *Astronomische Nachrichten*, CXXIX, 232.

[7] Clerke, *Problems in Astrophysics*, 199.

[8] Kant's *Allgemeine Naturgeschichte*.

[9] Laplace's *Exposition du Système du Monde*, II, 295.

[10] Voluminous and frequent papers in *Philosophical Transactions*, from about 1780 to 1820.

[11] *Astronomische Nachrichten*, XXI, 233; and Humboldt's *Cosmos*, part II, p. 401.

[12] *Philosophical Magazine*, XX, 93.

The study of our sun forms the principal basis of astrophysical research. The sun is an ordinary star, comparable in size and condition with millions of other stars, but it is the only one near enough to show a disk. The point-image of a distant star must be studied as an integrated whole; whereas the sun may be observed in considerable geometrical detail. We cannot hope to understand the stars in general until we have first made a thorough study of our own star.

We are unable to study the body of the sun, except by indirect methods. The interior is invisible. The spherical body which we popularly speak of as the sun is hidden from view by the opaque photosphere. This photospheric veil, including the sun-spots; the brilliant faculæ and flocculi, projecting upward from the photosphere; the reversing layer, in effect immediately overlying the photosphere; the chromosphere, a stratum associated with and overlying the reversing layer; the prominences, apparently ejected from the chromosphere; and the corona, extending outward from the sun in all directions to enormous distances; — these superlatively interesting features of the sun constitute the only portions accessible for direct observation; and they are an insignificant part of its mass. They are literally the sun's outcasts. Our knowledge of the sun is based almost exclusively upon a study of these outcasts. Nevertheless, we are able to formulate a fairly simple and satisfactory theory of its constitution.

The materials composing the sun appear to be the same as those forming the earth's crust. Of the eighty known elements, slightly more than half have been observed in the reversing layer and chromosphere, by means of their spectra. The existence of others remains unproved, but there are no reasons to doubt that they too are present. Our most complete study of the sun's composition was made by Rowland,[1] and he has said that, if the earth were heated to the temperature of the sun, the terrestrial and solar spectra would be virtually identical.

The force of gravity at the sun's surface is well known, but the radial pressures at interior points are somewhat uncertain, as they depend upon the unknown law of increasing density with increasing depth. The minimum value of the pressure at the sun's centre is thought to be fully ten thousand million times the pressure of our atmosphere at sea-level.[2] The most probable value of the effective temperature of the sun's radiating surface is 6000° Centigrade,[1] and the minimum value for the centre is perhaps five million degrees. In view of these high temperatures, and the low average density of the sun, the interior must be largely gaseous, and perhaps entirely so; although, under the stupendous pressures, a great central core is probably of a viscous consistency,[2] but ready to assume the usual properties of a gas when the convection currents carry the viscous masses up into regions of lower pressure.

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[1] *Physical Papers*, pp. 521–524.

[2] Arrhenius, *Lehrbuch der Kosmischen Physik*, p. 123.

The surface strata are radiating heat into surrounding space. To maintain the supply, it is imperative that convection currents should carry the cooled masses down into the interior, and bring corresponding hot masses up to the surface. These currents make the sun a very tempestuous body. Further, the outrushing materials must acquire the higher rotational speeds of the surface strata, and the inrushing must lose their tangential momentum; and these can scarcely be ineffective factors in the sun's circulatory system.

The mechanical theory of the maintenance of at least a part of the sun's radiation must be considered as a necessary consequence of the law of gravitation — as unavoidably a consequence of that law as precession is. Helmholtz computed that a contraction of the solar diameter of less than 400 feet per year [3] would suffice to maintain the present rate of flow. Whether this is the sole source of supply is uncertain, and very doubtful. The discovery of sub-atomic forces in uranium, thorium, and radium is of interest in this connection. These radioactive substances have revealed the existence of intense forces within the atom, long dreamed of by students of physics and chemistry, but never before realized. The energy radiated by an atom of these substances is thousands of times greater than that represented by the ordinary chemical transformations of equal masses of any known element. Whether these forces are working within the sun, prolonging its life many fold, and incidentally diminishing the required rate of Helmholtzian contraction, we do not know; but we are not justified in treating gravitation as the sole regulator of radiation.[4] We are encouraged to this view by the fact that the age of the earth, as interpreted by geology and biology, is many times greater than the superior limit set by the gravitational theory.

The dazzlingly brilliant photospheric veil which limits the depth of our solar view is due, with no room for doubt, to the condensation of those metallic vapors which, by radiation to cold space, have cooled below their critical temperatures. These clouds form and float in a great sea of uncondensed vapors, very much as do our terrestrial clouds; but it seems probable that the process of formation is continuous and rapid; and that they are added to from above, or from the interstices, and melt away from below.

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[1] Young, *Popular Astronomy*, XII, 225.

[2] Young, *The Sun*, p. 331.

[3] *Ibid*, p. 315.

[4] Young, *Popular Astronomy*, p. 225. This article, and the volume referred to in footnote no. 2 are the best existing repositories of facts and theories relating to the sun.

The sun-spots are the most extensively studied and the least understood of all solar phenomena. That they are large-scale interruptions in the photosphere, and at the same time the most striking evidence of atmospheric circulation, there can be no doubt. Observations made near the sun's limb, to determine whether the spots are elevations or depressions with reference to the photosphere, seem not to be reliable, perhaps because of abnormal refractions in the strata overlying and surrounding the spots. In the earth's atmosphere, a high barometer is the indication of descending currents, which generate heat by compression and prevent cloud formation. Is not the umbra of a spot an area of high pressure, which forces the solar atmosphere slowly downward, preventing cloud formation in that area, but favoring the growth of brilliant faculæ and flocculi in the regions of uprush surrounding the spot, a theory first suggested by Secchi?

The visible spots are not the sole evidences of circulation. The surface is covered with a network of interstices, or vents between clouds, which probably exercise all the functions of the visible spots, but on a smaller scale.

There is no reason to question the truth of Young's discovery that the Fraunhofer lines originate in the absorption of a reversing layer [1] — a thin stratum of uncondensed vapors lying immediately over and between the photospheric clouds.

The chromospheric stratum, several thousand miles in thickness, includes and extends far above the reversing layers, and contains the lighter gases, such as hydrogen and helium, and the vapors of calcium, sodium, magnesium, and other elements which do not condense under existing temperatures.

The prominences have in general the same composition as the chromosphere. In some the lighter gases, and in others the heavier metallic vapors, predominate. They are portions of the chromosphere projected beyond its usual level by the more violent ascending currents, or perhaps by eruptions of a volcanic character; and these forces are almost certainly augmented by the pressure of the sun's radiation. It is difficult to account for the quiescent, cloud-like prominences in regions far above the chromosphere on any supposition other than that they are in equilibrium under the opposing influences of gravity and radiation pressure.

The nature of the forces which control the general and detailed coronal forms is but little understood. Motion within the corona has never been directly observed. Yet we cannot question that the component particles are driven outward from the sun, and that many of them probably fall back into the sun, either singly or after combining to form larger masses. It is suggested that outbound particles may be started on their way by the violent solar circulation, continued on their journey by radiation pressure, and arranged in the characteristic streamers under the influence of magnetic forces.

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[1] *U. S. Coast Survey Report*, 1870, pp. 141–156.

The light received from the corona is of three kinds:

(1) A small quantity of bright-line radiations from a gas overlying the chromosphere. This gas is unknown to terrestrial chemistry, and astronomers provisionally call it coronium. It is distributed very irregularly over the solar sphere, and shows a decided preference [1] for the sun-spot zones.

(2) The bright-line radiations from coronium are almost a negligible quantity, in comparison with those from the same regions which form a strictly continuous spectrum, and which seem to be due to the incandescence of minute particles heated by the intense thermal radiations from the sun.

(3) A small proportion of the inner, and a large proportion of the outer, coronal light are solar rays reflected and diffracted by the coronal particles.

Arrhenius has recently shown that Abbot's observation [2] of an apparent temperature of the corona nearly equal to that of his observing room is in harmony with the spectrographic evidence of an inner corona composed of incandescent particles. Arrhenius [3] finds that one minute dust particle to each 11 cubic meters of space in the coronal region observed by Abbot, raised to the temperature of 4620° absolute required by Stefan's law, would give a corona of the observed brightness, and of the observed temperature. The bolometric strip measured the resultant temperature of the few highly heated particles and the cold background of space upon which the particles are seen in projection.

Arrhenius further estimates that a corona composed of incandescent dust particles need not have a total mass greater than 25,000,000 tons, to radiate the quantity of light yielded by the brightest corona observed. This is approximately that of a cube of granite only 200 meters on each side; a remarkably small mass for a volume whose linear dimensions are millions of kilometers.

This *résumé* of solar theory necessarily overlooks many unsettled points of great significance. Most important of all, perhaps, is that of the solar constant: does it vary, and in accordance with what law? Why is there a sun-spot period, and why are the large spots grouped within limited zones? Why does the form of the corona vary in a period equal in length to the spot period? Why does the angular speed of rotation increase from the poles to the equator? What is the origin of the faculæ and the flocculi? Why do the Fraunhofer lines show little evidence of high atmospheric pressure? Why are the radiations from calcium, one of the heavy elements, so prominent in the higher chromospheric strata and in the prominences? A great number of such questions are pressing for solution. Under the stimulus of the brilliant researches of our chairman, the reinventor and the leading developer of the spectroheliograph, coöperative plans for solar work on a large scale are now being organized. We should be vitally interested in promoting these plans; for the study of the sun, as the principal foundation of astrophysical research, has been unduly neglected.

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[1] *Astrophysical Journal*, XI, 231.
[2] *Astrophysical Journal*, XII, 71–75.
[3] *Lick Observatory Bulletin*, no. 58.

The celestial bodies develop under conditions over which we have no control. We must observe the facts as they are, at long range, and interpret them in accordance with those principles of physical science which govern what seem to be closely related terrestrial phenomena. A successful study of the development of matter in distant space, under the influence of heat, pressure, electricity, and other forces of nature demands a complete understanding of the action of the same forces upon terrestrial matter. The astrophysicist dwells in the laboratory as well as in the observatory; and laboratory researches must supply the links which connect world-life and star-life.

It has not been possible for laboratory investigators to reproduce stellar phenomena on a scale approaching that occurring in nature, nor to duplicate conditions of temperature and pressure existing within the stars; and these are unfortunate limitations. Nevertheless, many successes have been achieved in this direction. The low-temperature triumphs of Dewar,[1] Olczewski, and others approximate to the conditions of space surrounding the stars. The electric arc and spark appear to reproduce the temperatures of many stellar chromospheres and reversing layers. The electric furnace of Moissan[2] seems to supply temperatures comparable with those of the photosphere, and it promises to throw light upon the processes of cloud formation in the stars. Investigations as to the influence of varying pressures, — from almost perfect vacua up to many atmospheres, — as to the effects of varying electrical conditions and of other factors,[3] have answered many celestial questions, and introduced others equally pressing.

Laboratory observations have established that the spectra of the elements are not the same under all circumstances. We formerly thought it remarkable that nitrogen should have two or three characteristic spectra, or that a metal should have a spark spectrum and an arc spectrum. We are now confronted with the potent fact that an element may have a variety of spectra, depending upon the nature and the intensity of the forces employed in rendering it luminous.[1] But for most cases these involve only moderate variations in the relative intensities of spectral lines. The complications which threaten to result therefrom are more apparent than real. The multiplicity of spectral reactions promises to be a powerful aid to analysis, by supplying a more exact key to the conditions in the celestial light source which produce the observed effects.

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[1] Numerous papers in *Proceedings* of the Royal Society, principally between 1890 and 1900.

[2] Numerous papers in *Comptes Rendus*, principally between 1890 and 1900.

[3] Kayser's *Handbuch der Spectroscopie*, II, 289–337.

For many years following the application of the spectroscope to celestial problems it was supposed that a continuous spectrum must indicate incandescent solid or liquid matter. The situation is not so simple as this. Some gases radiating under high pressures give spectra apparently continuous.

The effect of increasing temperature conditions on certain spectra has long been well known. Certain lines are enhanced in relative brilliancy when we pass from the temperature of the arc to that of the high-tension spark, and *vice versa*;[2] but it seems certain that, within measurable limits, the positions of the lines do not change under this influence.

Humphreys and Mohler[3] have proved that the spectral lines are shifted by pressure; toward the red with increasing pressure in the atmosphere surrounding the arc. It is not difficult to see the bearing of this discovery upon astrophysical inquiry. Some subjects are made more complex; but the hope is held out that eventually we may detect these indications of pressure, differentially, in the brighter stars.

It is also known that the spectra of some elements are altered by the presence of other elements,[4] but the extent and character of the induced changes are little understood. As the chemical elements are never found alone in celestial bodies, the serious consequences of this effect must be evident.

The temperature in glowing Plücker tubes is of great interest, from its bearing upon the probable temperatures of nebulæ, the auroræ, and other bright-line phenomena of a diffuse nature. It is not certain that direct observation by any thermometric device can deal with the problem. The measures thus far attempted have assigned temperatures but a few degrees higher than that of the environment. These indications are probably correct for the average temperature of the contents of the tube, but hardly so for those molecules which are glowing. It has been suggested that perhaps a very small proportion of the molecules receive and carry the discharge: that while the molecules in action may be very hot, the average for all in the tube is very low. It seems reasonable to suppose, also, that the low-temperature indication is due to the fact that the current is actually passing but a small fraction of the time. The effect upon the eye is that of a continuous glow, whereas the thermometer measures the average effect.

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[1] Kayser's *Handbuch der Spectroscopie*, II, 222–286.

[2] Berlin, *Berichte*, 1894, 257–258; *Astronomy and Astro-Physics*, XIII, 660–662; *Astrophysical Journal*, XVII, 270; and many others.

[3] *Astrophysical Journal*, III, 114; IV, 175; VI, 169.

[4] Lewis, *Astrophysical Journal*, X, 137; Nutting, *Bulletin Bureau of Standards*, I, 77.

The influence of a magnetic field [1] upon the character of spectral lines, established in the laboratory by Zeeman, has not yet been observed in celestial spectra, but its detection may be merely a question of the dispersive power available on faint spectra.

It will be perceived that the interpretation of celestial spectra must be made with circumspection. We are not always justified in reaching conclusions upon the spectroscopic evidence alone; general conditions must also be taken into account. For example, shall we say that the temperature of the gaseous nebulæ is very high, because they have bright-line spectra? On the contrary, the difficulty of maintaining a high temperature in a mass so attenuated should be given at least equal weight. The radiating molecules or particles may for the instant be quite hot, but the effective temperature of the whole nebula is probably low.[2]

The experimental verification of radiation pressure by Lebedew [3] and by Nichols and Hull [4] is far-reaching in its consequences. We must take this force into account, as truly and as constantly as we must consider gravitation. Radiation pressure requires us to reconstruct our theories of comets' tails, of the corona, of the zodiacal light, of the auroræ, — in fact, of every phenomenon of nature involving minute particles.[5] And what celestial object does not involve them?

On the other hand, the student of the stars has pointed the way for the laboratory investigator, in many instances. The ultra-violet hydrogen series [6] was photographed by Huggins, in the spectrum of Vega, before it was found in the laboratory; and Pickering has discovered another hydrogen series,[7] in Zeta Puppis, which still awaits terrestrial duplication. The hypothetical element, helium, in the sun, waited a quarter-century for Ramsay's discovery,[8] and the laboratory investigation [9] of its more complete spectrum which followed. Stu-

[1] *Handbuch der Spectroscopie*, II, 613–672.

[2] *Lehrbuch der Kosmischen Physik*, 43.

[3] *Annalen der Physik*, 1901, VI, 433.

[4] *Astrophysical Journal*, XV, 62; XVII, 315; XVII, 352.

[5] Arrhenius, *Physikalische Zeitschrift*, November, 1900.

[6] *Philosophical Transactions*, CLXXI, 669.

[7] *Astrophysical Journal*, V, 92.

[8] *Nature*, LXV, 161.

[9] *Astrophysical Journal*, III, 4.

dents of the solar corona and of the gaseous nebulæ are discussing the properties of the hypothetical elements coronium and nebulium almost as familiarly as if they had actually handled them. Out of some 20,000 absorption lines mapped by Rowland, more than the half are awaiting laboratory identification.

In this connection, the mathematical relations existing between the positions of lines in the spectra of many of the principal elements, discovered by Balmer,[1] Kayser,[2] Runge, and Paschen,[3] have already been of great utility; and they can scarcely fail to illuminate the question of the construction of the atoms involved.

A new era of physical science was inaugurated about eight years ago by the discovery of argon on the one hand, and of the X-rays on the other. The former was followed by the discovery, in quick succession, of several other constituents of the earth's atmosphere which at present demand our attention as to their presence in chromospheric and auroral phenomena. It would be most surprising if the many forms of radiation, including those of the radioactive substances, discovered in the train of the X-rays, should not throw strong light upon the constitution of matter. And how shall we deal intelligently with the forms of matter in other worlds before we understand the constitution of matter upon the earth? The modern theory of electrons, in which material atoms play the subordinate part, and electric charges the principal part, promises to have a wide application to celestial phenomena. Further, the actual transport and interchange of matter in the form of small particles, from one star to another, as urged with great learning and skill by Arrhenius,[4] seems to be a plain and unavoidable consequence of recently established physical facts. Should this theory stand the test of time, its far-reaching consequences would accord it a position of the first rank.

The photographic programme inaugurated with the Crossley Reflector by Keeler comprised 104 negatives of the regions containing the principal nebulæ and star-clusters. These photographs, covering but one six-hundredth part of the entire sky, record 850 [5] nebulæ, of which 746 are new. If this proportion should hold good over the whole sphere, the number discoverable with this instrument, with exposures of ordinary length, would be half a million. This estimate would be too large in case the smaller nebulæ have a tendency to cluster around the prominent nebulæ, which to some extent is probably true. The number of stars visible in our great telescopes is of the order of one hundred millions. The dark or invisible bodies in-

[1] *Annalen der Physik*, xxv, 80.
[2] Berlin *Abhandlungen*, 1890.
[3] *Annalen der Physik*, 1897, LXI, 641.
[4] *Proceedings*, Royal Society, LXXIII, 496.
[5] *Lick Observatory Bulletin*, no. 64.

dicated by several considerations — the planets in the solar system, the spectroscopic binaries, the eclipsing variable stars, and the gravitational power of the universe — should outnumber the bright ones several fold.[1] It is the thesis of astrophysics that all these objects — the nebulæ, the bright stars, and the invisible bodies — are related products of a system of sidereal evolution. The general course of the evolutionary process, as applied to the principal classes of celestial objects, is already known. We are able to group these classes, with little chance of serious error, in the order of their effective ages.

The earliest form of material life known to us is that of the gaseous nebulæ. In accordance with the simplest of physical laws, a nebula must radiate its heat to surrounding space. In accordance with another law, equally simple, it must contract in volume, — toward a centre, or toward several nuclei, — and generate additional heat in the process. Eventually a form of considerable regularity will result. Whether this form is that of a typical planetary nebula, of a spiral nebula, or of some other type, is a matter of detail. It is quite possible that nature uses several moulds in shaping the contracting masses, according as they lie on one side or the other of critical conditions. The variety of existing forms is extensive. One can see very little resemblance in the Trifid Nebula,[2] which is apparently breaking up into irregular masses; the Dumb-Bell Nebula,[3] from whose nearly circular form rings of matter seem to be separating; the great spiral nebulæ;[3] the Ring Nebula in Lyra,[4] with a central star; the compact planetary nebula G. C. 4390,[5] containing a dense, well-defined nucleus; and many others of distinct types.

The condensed globular forms occupying the positions of nebular nuclei have almost reached the first stage of stellar life.

It is not difficult to select a long list of well-known stars which cannot be far removed from nebular conditions. These are the stars containing both the Huggins and the Pickering series of bright hydrogen lines, the bright lines of helium, and a few others not yet identified. Gamma Argus [6] and Zeta Puppis [7] are of this class. Another is DM. +30.°3639,[8] which is actually surrounded with a spherical atmosphere of hydrogen, some five seconds of arc in diameter. A little further removed from the nebular state are the stars containing both bright and dark hydrogen lines; [9] — caught, so to speak, in the act of changing from bright-line to dark-line stars. Gamma

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[1] *Report*, British Association for the Advancement of Science, 1901, 563.
[2] Newcomb, *The Stars*, Frontispiece.
[3] Roberts's *Celestial Photographs*.
[4] *Astrophysical Journal*, x, 193.
[5] *Publications*, Lick Observatory, III, following p. 229.
[6] *Astronomy and Astro-Physics*, XIII, 456.
[7] *Astrophysical Journal*, v, 92.
[8] *Astronomy and Astro-Physics*, XIII, 461.
[9] *Astrophysical Journal*, II, 177.

Cassiopeiae, Pleione, and Mu Centauri are examples. Closely related to the foregoing are the helium stars.[1] Their absorption lines include the Huggins hydrogen series complete, a score or more of the conspicuous helium lines, frequently a few of the Pickering hydrogen series, and usually some inconspicuous metallic lines. Calcium absorption is absent, or scarcely noticeable. The white stars in Orion and the Pleiades are typical of this age.

The causes which produce bright lines in stars [2] are not thoroughly understood; but atmospheres of higher temperatures than their underlying strata, or very extensive simple atmospheres, seem to be demanded. The former condition, on the large scale required, involves some difficulties, and mildly suggests the possibility that external influences may be acting upon the radiating strata of bright-line stars.

The assignment of the foregoing types to an early place in stellar life was first made upon the evidence of the spectroscope. The photographic discovery of nebulous masses in the regions of a large proportion of the bright-line and helium stars affords extremely strong confirmation of their youth. Who that has seen the nebulous background of Orion,[3] or the remnants of nebulosity in which the individual stars of the Pleiades [4] are immersed, can doubt that the stars in these groups are of recent formation?

With the lapse of time, stellar heat radiates into space; and, so far as the individual star is concerned, is lost. On the other hand, the force of gravity in the surface strata increases. The inevitable contraction in volume is accompanied by increasing average temperature. Changes in the spectrum are the necessary consequence. The second hydrogen series vanishes, the ordinary hydrogen absorption is intensified, the helium lines become indistinct, and calcium and iron absorptions begin to assert themselves. Vega and Sirius [5] are conspicuous examples of this period. Increasing age gradually robs the hydrogen lines of their importance, the H and K lines broaden, the metallic lines develop, the bluish-white color fades in the direction of the yellow, and, after passing through types exemplified by many well-known stars, the solar stage is reached.[6] The reversing layer in solar stars represents but four or five hydrogen absorption lines of moderate intensity; the calcium lines are commandingly prominent; and some 20,000 metallic lines are observable. The solar type seems to lie near the summit of stellar life. The average temperature of the mass must be nearly a maximum, for the low density indicates a constitution that is still gaseous.

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[1] Clerke's *Problems in Astrophysics*, p. 189.
[2] Frost's *Scheiner's Astronomical Spectroscopy*, p. 250.
[3] *Harvard Annals*, XXXII, 66, and plate III, fig. 4.
[4] Clerke's *System of the Stars*, p. 224, and frontispiece.
[5] Huggins, *An Atlas of Representative Stellar Spectra*, plates V and VI.
[6] *Ibid.* plate VII.

Passing time brings a lowering of average temperature. The color passes from yellow to the red, in consequence of lower radiating temperatures and increasing general absorption by the atmosphere. The hydrogen lines become indistinct, metallic absorption remains prominent, and broad absorption bands are introduced. In one type, of which Alpha Herculis [1] is an example, these bands are of unknown origin; in another, illustrated by 19 Piscium,[2] they have been definitely identified as of carbon origin. The relation between the two types is not clear. It has even been advocated that the evolutionary process divides shortly after passing the solar stage: that the reddish stars with absorption bands sharply terminated on the violet edges are on one branch, and that the very red stars with absorption bands sharply defined on the red edges are on the other branch. This plan of overcoming a difficulty seems to me to introduce a greater difficulty; and I do not doubt that systematic investigation will supply the connections now missing. That the denser edges of the bands in Type IV [3] Secchi should occupy the same positions as the denser edges of absorption bands in Type III [4] can hardly be without significance; and Keeler's view that the carbon absorption bands in Type IV are matched by carbon radiation, in some stars, at least, of Type III, suggests a most promising line of investigation for powerful instruments.

There is scarcely room for doubt that these types of stars are approaching the last stages of stellar development. Surface temperatures have lowered to the point of permitting more complex chemical combinations than those in the sun. The development of "sun-spots" on a large scale is quite probable, and the first struggles to form a crust may be enacted. Type III includes the several hundred long-period variable stars of the Omicron Ceti [5] class, whose spectra at maximum brilliancy show several bright lines of hydrogen and other elements. The hot gases and vapors seem to be alternately imprisoned and released. It is significant that the dull red stars are all very faint, — there are none brighter than the 5½ magnitude. Their effective radiating power is undoubtedly very low.

The period of development succeeding the red-star age of Type IV has illustrations near at hand, in the planets Jupiter and the earth; invisible save by borrowed light. When the interior heat of a body shall have become impotent, the future promises nothing save the slow leveling influence of its own gravitation and meteorological elements. It is true that a collision may occur to transform a dark body's energy of motion into heat, sufficient to convert it into a glowing nebula, and start it once more over the long path of evolution. This is a beautiful theory, but the facts of observation do not give it satisfactory support. There is little doubt that the principal novæ of recent years have been the results of collisions,[1] either between two massive dark bodies, or between a massive body and an invisible nebula. The suddenness with which intense brilliancy is generated would seem to call for the former, but the latter is much more probable, in view of many facts. The nebular spectra of the novæ are generated in a few months: [2] but in every case thus far observed the bright nebular bands grow faint very rapidly, and in the course of a few years leave a continuous spectrum, — apparently that of an ordinary star. Either the masses involved in the phenomena are extremely small, or the disturbances are but skin-deep. In any case, the novæ afford little evidence as to the complete renebularization of dark bodies.

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[1] Huggins, *An Atlas of Representative Stellar Spectra*, plate XII.

[2] Clerke's *Problems in Astro-Physics*, p. 218.

[3] Frost's *Scheiner*, p. 312.

[4] *Ibid.* p. 300.

[5] Clerke's *Problems in Astro-Physics*, p. 224.

I spoke of the *average* temperature of a developing star as reaching a maximum near the solar stage when the border-line between gaseous and liquid constitution is reached. This refers to the entire mass. The law of surface temperatures is quite a different one. The bright-line and helium stars seem to have hotter surfaces than the solar and red stars. The spectra which we observe are surface phenomena which indicate the temperatures of the radiating and absorbing strata. The maximum intensity of continuous radiations is higher up in the spectrum for the white stars than for the yellow and red, a safe indication of higher temperatures. The lines in white-star spectra are distinctly the enhanced lines thought to be produced by high temperatures. These facts are not inharmonious. Surface temperature is a function of the rapidity with which convection currents can carry heat from the interior to the surface. The comparatively low internal heat of white stars, delivered quickly at the surface by rapidly moving gases, may readily maintain higher atmospheric temperatures than the much hotter interiors of solar stars, whose circulation has the sluggishness of viscosity.

Sir William and Lady Huggins are inclined to assign greater importance to mass and density, as factors in evolution, than to temperatures.[3] Their view is that under the influence of great surface gravity, the generation and radiation of heat is accelerated, and the life of the star is lived more rapidly. They have been led to this view, in part, by the apparent anomaly of double stars, in which the more massive primary is generally yellower than the less massive companion. The subject is one of great difficulty and importance, and, unfortunately, laboratory methods are on too small a scale of mass and pressure to solve the problem.

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[1] *Astronomy and Astro-Physics*, XI, 907.

[2] *Ibid.* XI, 715.

[3] Clerke's *Problems in Astro-Physics*, p. 274.

Up to the year 1800 only twelve variable stars were known. Chandler's catalogue,[1] dated 1888, contains 225 entries. The remarkable progress made by astronomical science in the past fifteen years is fairly indicated by the fact that in this interval the number of known variable stars increased from 225 to more than 1400. To Harvard College Observatory belongs the great credit of discovering nearly 900 of these objects.

In many respects variable stars constitute the most interesting class of objects in the heavens. The tens of millions of ordinary stars are undoubtedly growing older; and the tens of thousands of nebulæ, from which stars will eventually be formed by processes of condensation, are undergoing transformation; but appreciable changes in the ordinary stars and in the nebulæ proceed with extreme deliberation, and no permanent changes have yet been noted. Variable stars, on the contrary, are changing before our eyes; and they repeat their fluctuations continually. They present opportunities for discoveries of the greatest interest in themselves, and of remarkable utility in the study of the problem of stellar evolution.

It is a conservative statement that in nineteen variable stars out of twenty we have little idea as to the causes of variability. The causes of the variations have been determined in the case of Algol[2] and a few others of that class: large dark companions revolve around these stars, and once in every revolution the companions pass between us and the principal stars, thus preventing a portion of their light from reaching us. In Zeta Geminorum[3] and three or four others of its class the spectroscope has shown that massive dark companions are close to, and rapidly revolving around, the principal stars. These invisible companions produce disturbances in the extensive atmospheres of the stars, and cause the observed variations in brightness, but the nature of the disturbances is still a matter of conjecture. Omicron Ceti[4] and other stars of its class have given no evidence of companions. Brightness variations in them seem to be due to internal causes. Perhaps they have reached the age when solid crusts attempt to form on their surfaces, just as one day a crust struggled to form on the liquid earth. A crust formed one month may be melted or sink to a lower level a few months later. Perhaps there are "sun-spots" on these stars, in scale vastly more extensive and in period shorter than those on our sun; but these suggested explanations may be far from the truth.

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[1] *Astronomical Journal*, VIII, 81.

[2] Clerke's *System of the Stars*, p. 128.

[3] *Astrophysical Journal*, XIII, 90.

[4] *Lick Observatory Bulletin*, no. 41.

For more than half a century a great many astronomers have devoted themselves assiduously to making photometric observations of variable stars. There are a dozen observatories, both large and small, which are systematically devoting some of their resources to this work. By common consent of the profession, or by appointment from learned societies, there have for some fifty years been individual astronomers, or committees of astronomers, who systematize results, call attention to the need for observations of certain neglected objects, and in many other ways encourage the photometric study of variable stars. Photometers are inexpensive, the methods are simple, and results have rapidly accumulated.

Observations of variable stars with slit-spectrographs, on the contrary, are surprisingly meagre and fragmentary. Not a single institution, not a single telescope, not a single observer, is working continuously or even extensively on the subject. Yet the method is a very powerful one: the few isolated studies made on variable stars have led to results of remarkable richness. The subject is one of great difficulty. Photographic spectra require much time for accurate measurement and reduction. And, finally, powerful and expensive instruments are demanded.

Harvard College Observatory has been remarkably successful in discovering variable stars by means of peculiarities in their spectra, as well as in classifying them, and in qualitative studies of many spectral details, using objective-prism spectrographs; but it is hoped that slit-spectrographs, attached to powerful telescopes, may soon be devoted systematically to this subject, as it constitutes one of the richest fields now awaiting development.

A century and a half of meridian-circle observations has given to the world, as one of many priceless contributions, a knowledge of the proper motions of several thousand stars. Some of the ablest astronomers have used these results as a basis for determining the most probable elements of the sun's motion,[1] and in studies upon the distribution of the stars in space. Unfortunately, these investigations necessarily involve assumptions as to the unknown distances of the stars.

A few years following the application of the spectroscope to the study of celestial objects, Huggins recognized that the Doppler-Fizeau principle supplied, in theory at least, the long-hoped-for method of measuring the components of stellar motions in the line of sight — their radial velocities; and that the application of this method would enable us to determine both the direction and the speed of the solar motion, entirely independently of the distances of the stars. Efforts to apply this method met with signal failure for twenty years, and doubts even as to ultimate success were quite generally felt and freely expressed. The beginnings of success were made by Huggins[1] and Pickering,[2] in showing that photography reveals, with great clearness, the delicate spectral lines which the eye in purely visual observations is unable to see at all. In 1888, Vogel[3] applied this knowledge in the first photographic attempt to measure radial velocities, and his work inaugurated a new era. His observations, obtained with a small telescope and imperfect spectrograph, were not sufficiently accurate to meet the needs of the principal sidereal problems, but they led to several brilliant discoveries at Potsdam, and were invaluable in marking out the path of progress. It was not until 1896 that the use of a powerful telescope, equipped with an efficient spectrograph, gave results accurate enough to satisfy present requirements.[4] In fact, the accuracy obtained exceeded our most hopeful expectations.

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[1] Clerke's *System of the Stars*, chapter 23.

It is not surprising that thirty years were required to develop successful methods. The work is so delicate that, unless suitable precautions are taken at every point in the process, the errors introduced may readily be larger than the quantities sought for. With the Mills spectrograph, for example, a speed of nine kilometers per second displaces the lines only 0.01 mm. The probable error of a velocity determination for the best stars, such as Polaris,[5] is but one fourth of a kilometer per second, corresponding to a linear displacement of 0.0003 mm., or 0.00001 inch. In view of the newness of the subject, the richness of the field, and the fact that the more active great telescopes are now nearly all applied to this work, I append a list of the improvements which have contributed most powerfully to recent progress:

(1) A realization of the fact that a spectrograph is an instrument complete in itself. The telescope to which it is attached serves only to collect the light and to deliver it properly upon the slit.

(2) The development of a method of reduction which permits the use of all good stellar lines, irrespective of whether they correspond to, or lie between, the comparison lines.[6]

(3) The use of a longer collimator, permitting a wider slit, and requiring larger prisms, with greater resolving power.[7]

(4) The use of simple prisms, of better glass, with better optical surfaces.[8]

(5) Care in collimating, to insure that the star light and comparison light traverse identically the same part of the collimator lens.[9]

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[1] *Atlas of Representative Spectra*, plate II.

[2] *Annals*, Harvard College Observatory, various volumes.

[3] *Publications*, Potsdam Astrophysical Observatory, vol. VII.

[4] *Astrophysical Journal*, VIII, 123.

[5] *Ibid.* XIV, 60.

[6] *Ibid.* VIII, 146.

[7] *Ibid.* VIII, 125

[8] *Astronomy and Astro-Physics*, XII, 45.

[9] *Publications*, Lick Observatory, III, 177.

(6) The adoption of a compact and rigid form of spectrograph mounting designed in accordance with good engineering practice.

(7) The elimination of flexure effects by supporting the spectrograph, in connection with the telescope, in accordance with engineering principles. The conventional spectrograph had been supported entirely at its extreme upper end; the instrument projected out into space, unsupported, boldly inviting flexure under the varying component of gravity.

(8) The use of a constant temperature case around the instrument.[1]

(9) Precautions taken to eliminate many sources of error from the measures of the spectrograms.[2]

Up to December, 1900, — the last month of the departing century, — the speeds of 325 stars had been determined with the Mills spectrograph in the northern two thirds of the sky. Omitting several stars whose lines could not be measured accurately, and some thirty spectrographic and visual binaries for whose centres of mass the velocities were still unknown, 280 stars remained available for deducing the relative motion of our solar system.[3] The observational data were distributed symmetrically in right ascension, and the result for this coördinate of the apex agreed with Newcomb's proper-motion result within a small fraction of a minute of arc. The data were extremely unsymmetrical in declination, as there were few observations between – 15° and – 30° declination, and none whatever south of – 30°. The solution placed the apex 15° south of Newcomb's position. The deduced speed, 20 km. per second, is no doubt close to its true value.

There is a question whether the direction of the solar motion can be determined more accurately from proper motions or from radial velocities, an equal number of stars being available in the two cases; but as to the speed, no doubt of the very marked superiority of the spectrographic method can exist. This, however, is but incidental, for the two methods are in fact mutually helpful and mutually dependent: the motion of every star involves both components.

In this connection two points call for appreciation: First, the motion of the solar system is a purely relative quantity. It refers to the group of stars used in the solution. We could easily select twenty or thirty of these stars whose velocities were such that the deduced motion would be reversed 180° from that given by the entire list of stars. We want to know the solar motion with reference to the entire sidereal system. A satisfactory solution of the problem demands that we use enough stars to be considered as representative of the whole system. Second, the great sidereal problems require that observational data for their solution should cover the whole sky. Until one year ago radial velocity measures were confined to the northern two thirds of the celestial sphere. Further attempts to deduce the solar motion from northern observation alone would not be justified. Observations in the southern third of the sky were needed, not only to represent that large region in the solution, but in order that the unknown systematic errors which affect the northern observations, as well as the southern, might be eliminated, through the symmetrical balancing of the material. Fortunately the energetic and wise policy of the Cape Observatory and the generosity of Mr. D. O. Mills have provided two complete equipments, which are now busily engaged in supplying the southern data required. The Mills spectrograph in the northern hemisphere has secured about three thousand spectrograms of approximately five hundred stars, and the Mills spectrograph in the southern hemisphere has secured four hundred spectrograms of one hundred and twenty-five stars. The number of stars not on the Mills list, and *accurately observed* with other high-dispersion spectrographs, is not known, but it is probably between one hundred and two hundred. We may reasonably expect that, in two or three years, as many as eight hundred well-determined radial velocities may be brought to bear upon pressing sidereal problems.

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[1] *Bulletin Astronomique*, XV, 49; *Astrophysical Journal*, XI, 259, and XV, 172.

[2] *Astrophysical Journal*, VI, 424, and many others.

[3] *Ibid.* XIII, 80.

It is a frequent question: Is the solar system moving in a simple orbit, and will it eventually return to the part of its orbit where it is now? The idea of an affirmative answer to this question is very prevalent in the human mind. It is natural to think that we must be moving on a great curve, perhaps closed like an ellipse, or open like a parabola, the centre of mass of the universe being at the curve's principal focus. The attraction which any individual star is exerting upon us is certainly very slight, owing to its enormous distance; and the combined attractions of all the stars may not be very much greater; for since we are somewhere near the centre of our stellar system, the attractions of the stars in the various directions should nearly neutralize one another. Even though we may be following a definite curve at the present time, there is, in my opinion, little doubt that we should be prevented from continuing upon it indefinitely. In the course of our travels we should be carried, sooner or later, quite close to some individual star whose attraction would be vastly more powerful than that of all the other stars combined. This would draw us from our present curve and cause us to follow a different one. At a later date, our travels would carry us into the sphere of attraction of some other great sun which would send us away in a still different direction. Thus our path should in time be made up of a succession of unrelated curves.

Spectroscopic binary systems, as by-products of radial velocity measurements, are of exceedingly great interest from the light which they cast upon the construction of other systems than ours. When we look at the sky on a clear night, we may be sure that at least one star in six or seven is attended by an invisible companion, comparable in mass with the primary body, the two revolving around their common centre in periods varying from two or three days in many cases, up to three or more years in others. For the triple system of Polaris [1] the long period perhaps exceeds fifteen or twenty years. As the shortest-period visual binary now known, that of δ Equulei, is only 5.8 years,[2] the gap between visual and spectroscopic binaries has been definitely closed.

The companions of binaries discovered by means of the spectrograph have not been observed visually in our powerful telescopes, although they have been carefully searched for. They may be so close to the principal star that, viewed from our distance, the two images cannot be resolved. The separation of the components is probably less than one hundredth of a second of arc for most of the binaries thus far announced. Again, for very few of the systems are the spectra of both components recorded. This does not establish that the companion is a dark body, but only that it is at least one or two photographic magnitudes fainter than the primary. The fourth-magnitude companion of a second-magnitude star would scarcely be able to impress its lines upon the primary's spectrum. The invisible components in many spectroscopic binaries might be conspicuous stars, if they stood alone.

Only those systems have been detected whose periods are relatively short, and for which the variations of radial speed are considerable. The smallest observed variation is that of Polaris — six kilometers per second. Had the variation for Polaris been only one kilometer, it would no doubt have escaped detection. Such a variation could be measured by present instruments and methods; but this range would not have excited the observer's suspicion, and the discovery would have remained for the future. It is probable that there are more systems with variations of speed under six kilometers than there are with larger ones; and all such are awaiting discovery. The velocity of our sun through space varies slightly, because it is attended by companions — very minute ones compared with the invisible bodies discovered in spectroscopic binaries. It is revolving around the centre of mass of itself and its planets and their moons. Its orbit around this centre is small, and the orbital speed very slight. The total range of speed is but three one-hundredths of a kilometer per second. An observer favorably situated in another system, provided with instruments enabling him to measure speeds with absolute accuracy, could detect this variation, and in time say that our sun is attended by planets. At present, terrestrial observers have not the power to measure such minute variations. As the accuracy attainable improves with experience, the proportional number of spectroscopic binaries discovered will undoubtedly be enormously increased. In fact, the star which seems not to be attended by dark companions may be the rare exception. There is the further possibility that the stars attended by massive companions, rather than by small planets, are in a decided majority; suggesting, at least, that our solar system may prove to be an extreme type of system, rather than a common or average type.

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[1] *Astrophysical Journal*, XIV, 60.

[2] *Lick Observatory Bulletin*, no. 84.

Observations of stellar motions in the line of sight enable us to solve many other important auxiliary problems. Only one will be referred to here. The determination of stellar distances is exceedingly important, and correspondingly difficult. We know the fairly accurate distances of a dozen stars, and the roughly approximate distances of two or three dozen others. Radial velocity observations, in combination with proper motions, will enable us to determine the average distances of entire classes of stars. Let us consider the stars of the fifth magnitude, of which there are a thousand or more. They travel in practically all directions. A definite relation will exist between their average proper motion and their average radial motion, within a small limit of error. If meridian observations ascertain that the average annual proper motion of these fifth-magnitude stars is 0.03 seconds of arc, and spectrographic observations determine that their average speed in the line of sight is thirty-five kilometers per second, it is a simple matter to compute what their average distance must be in order to harmonize the two components.

A study of 280 observed stars as to the relation existing between visual magnitude and velocity in space led to interesting results.[1] The average speed of 47 stars brighter than the third magnitude is 26 km.; of 112 stars between the third and fourth magnitude, 32 km.; and of 121 stars fainter than the fourth magnitude, 39 km. The progression in these results is very pronounced, and I think we are justified in drawing the important conclusion that, on the average, the faint stars of the system are moving more rapidly than the bright stars. This interesting indication should be confirmed or disproved by the use of a much greater number of stars.

The proper method of combining radial velocities for statistical purposes is a question of great importance. The method of least-squares is based upon the assumption that the accidental errors of observation follow a certain law, found by experience to be substantially true. This method is not applicable to the combination of radial velocities, unless radial velocities are distributed in accordance with the law of accidental errors. Do stellar velocities whose values are near zero exist in greatest numbers? Or does some moderate speed predominate? The average speed *in space* of the 280 stars observed spectrographically is 34 km. When a much greater number of radial velocities is available, the law of distribution must be investigated, and a safe method of combination be developed.

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[1] *Astrophysical Journal*, XIII, 80.

Other practical questions exist as to the proper weights to assign to results of different degrees of accuracy, when it is desired to combine them statistically. The speeds of the brighter second- and third-type stars can be determined well within a kilometer per second, whereas the speeds of first-type stars, containing only broad and hazy lines, may be in error from five to fifteen kilometers. Again, low dispersion spectrography is developing so rapidly that in a few years the speeds of hundreds of the fainter stars will be known within two kilometers. Shall the weights assigned to individual results be proportional to the inverse squares of their probable errors? I think not. The deduced solar motion, for example, should refer to an observed programme of stars which shall be representative of the entire sidereal system. It must refer to a star with hazy lines, or to a faint star, as truly as to a bright solar-type star. One poorly determined result for velocity, used alone, should have small weight, but a large number of such determinations should be given considerable weight; proper care being taken to avoid systematic error. Prudence would suggest that separate solutions be made, first for the stars whose spectra admit of accurate measurement, and later for those whose spectra contain hazy lines, or which have been observed with low dispersion. From these a guide as to the relative weights to be assigned to the three or more classes of stars in combination may be found.

Radial velocity observers are concerned as to the part played in the results by pressure in the reversing layers of the stars. The differential effects of pressure are too small to detect in stellar spectra by present means, and there is no known method of eliminating them. We have no recourse but to assume that the stellar lines, neglecting the effect of radial motion, are in identically the same position as the solar lines and the laboratory lines of the elements. Whether the lines in the blue stars are produced under lower pressure than those in the sun, and the lines in the red stars under greater pressure than those in the sun, remains unknown, but this is not impossible. The effect of systematic errors in observed speeds from this source, as well as from other sources, would be eliminated from many statistical inquiries by having all parts of the sky represented in the solution.

Errors in the tables of absolute wave-lengths do not enter into radial-velocity results, provided the relative values are correct. In fact, we scarcely need to know the wave-lengths at all, for the determinations of velocity may be put upon a strictly differential basis, and

I incline strongly to the belief that this should be done. Let us consider the case very briefly. Rowland's wave-lengths are based upon spectrograms taken with high dispersion and resolving power. Radial-velocity spectrograms are secured with instruments of much lower power. Close solar and laboratory lines, of different intensities, clearly separated on Rowland's plates, are blended on stellar plates. For this and other reasons, the effective wave-lengths on the two classes of plates are different. The difficulty of assigning correct wave-lengths in the case of plates taken with a single-prism spectrograph is even greater: whole groups of separate lines are blended into one apparent line, and lines actually single are very few indeed. It is necessary to use blends, both in the stellar and comparison spectra. Two methods at least are available to eliminate errors in velocity due to errors in assumed wave-lengths. First: At the conclusion of a long series of observations of stars of the same spectral type, the velocity yielded by each line for each star should be tabulated. If one line gives velocities consistently large or consistently small, the conclusion is that its effective wave-length has been wrongly assumed, and we should be justified in changing it arbitrarily. And so on, for each line employed. This involves the assumption that the comparison bright-lines and the corresponding stellar lines have the same wave-lengths; and all the wave-lengths are reduced to one system, true for the particular spectrograph employed. The method is not entirely free from objection. Second: If the solar spectrum and the comparison spectra are photographed on one and the same plate, under precisely the usual observing conditions, measures of this plate, corrected for the observer's very slight radial velocity with reference to the sun, will form a reduction curve of zero velocity, expressed in terms of micrometer readings. If a spectrogram of star and comparison, made with the same instrument and measured in the same manner, is compared with this reduction curve, measure for measure, the speed of the star will be obtained directly, and irrespective of wave-length values; and many other fruitful sources of systematic error will be eliminated at the same time. Mr. R. H. Curtiss, of Mount Hamilton, formulated a method [1] on this basis last year, and he has applied it to a spectroscopic-binary variable star. The observations were made with a spectrograph whose dispersion is but one fifth, and whose exposure-time for a given star is but one tenth that of the Mills spectrograph. The probable error for a faint star seems to be not more than twice as great as that for a bright star with the Mills spectrograph. The method promises to be of great utility, capable of application to several thousand stars between the fifth and eighth magnitudes.

On account of the large proportion of spectroscopic binaries, stars should not be used statistically until observations covering several years have established the constancy of their motions. To determine the orbits and the speeds of the centres of mass of the binary systems, from twenty-five or more spectrograms each, is a task several fold more extensive than that of measuring the constant speeds of the non-binary stars.

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[1] *Lick Observatory Bulletin*, no. 62.

There remains the question of coöperation, on the part of radial-velocity observers, to avoid useless duplication, and to increase the output of results. Seven leading observatories in the northern hemisphere, and one in the southern, are in this field, presumably with the intention of remaining indefinitely. A second observatory in the southern hemisphere, devoted exclusively to this work, is of an expeditionary character, and its long continuance is problematical. It is fair to the participating observatories to say, judging by results thus far published, that some are still in the period of experiment and development; and, in fact, that all observers are introducing frequent improvements, which lead to greater accuracy. As long as the development of instruments and methods is in rapid progress, formal coöperation is unwise. Premature coöperation leads to confusion. Duplication of observations for the principal stars is as valuable and desirable in radial-velocity measurements as in meridian determinations of stellar positions. But just as soon as the methods assume a reasonably stable form, the entire sky should be apportioned amongst the interested observatories, in accordance with carefully considered plans which shall permit and encourage individual initiative. I have little doubt that this point will be reached by a sufficient number of observatories within two years, and that it would be well to conclude the preliminary organization of coöperative plans within the coming year. Such plans should be formed with severe deliberation, as the labor involved would be commensurate with that devoted to the construction of the Astronomische Gesellschaft zones for the entire sky.

The problems immediately confronting the astrophysicists of the twentieth century are serious ones. They call for our best efforts. The volume of work demanded is stupendous, and the difficulties to be overcome are correspondingly great. Nevertheless, the men and the means will be forthcoming. The mass of solid fact brought within the realm of knowledge by astronomers now living, many of whom are happily with us this week, is sufficient indication that the general solution of the problems of to-day is but a question of time. And we should be equally hopeful as to the problems of the future, for the desire to know the truth about the universe which surrounds us is an enduring element in human nature.

SHORT PAPERS

Professor A. Ricco, Director of the Royal Observatory, Catania, Sicily, presented a paper "On the Distribution of the Solar Phenomena."

Professor I. Hartmann, of the Astrophysical Observatory at Potsdam, Germany, read a paper before the Section on "A new Method of measuring the Stellar Spectra."

Professor E. B. Frost, of Yerkes Observatory, presented a paper giving a comparative study of stellar spectra.

Professor Charles D. Perrine, of Lick Observatory, read a paper on "Some Total Solar Eclipse Problems."

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DEPARTMENT XII

SCIENCES OF THE EARTH

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SCIENCES OF THE EARTH

(*Hall* 3, *September* 20, 11.15 *a. m.*)

CHAIRMAN: DR. G. K. GILBERT, U. S. Geological Survey.
SPEAKERS: PROFESSOR THOMAS C. CHAMBERLIN, University of Chicago.
PROFESSOR WILLIAM M. DAVIS, Harvard University.

THE METHODS OF THE EARTH-SCIENCES

BY THOMAS CHROWDER CHAMBERLIN

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IT is my assigned task to review the methods of the earth-sciences. The technical processes of the constituent sciences are peculiar to each and are inappropriate subjects for discussion before this composite assemblage; but the fundamental methods of intellectual procedure are essentially common to all the earth-sciences, and to these the address will confine itself.

That which passes under the name earth-science is not all *science* in the strict sense of the term. Not a little consists of generalizations from incomplete data, of inferences hung on chains of uncertain logic, of interpretations not beyond question, of hypotheses not fully verified, and of speculation none too substantial. A part of the mass is true science, a part is philosophy, as I would use the term, a part is speculation, and a part is yet unorganized material. However, I like to think of the aggregate, not as an amorphous mixture of science, philosophy, and speculation, but as a rather definite aggregation of these, not wholly unlike the earth itself. The great mass of our subject material may be regarded as a lithosphere of solid facts. Around this gathers an atmosphere of philosophy, rather dense near the contact zone, but thinning away into tenuous speculation in the outer regions. For myself, I like to think of the nucleus as solid and firm throughout, not as a thin fractured crust floating on a fiery liquid of Plutonian suggestiveness. I like to think of the philosophic and speculative atmosphere as no mere gas-zone of shallow depth, as of old, but as an envelope of intense kinetic life wherein the logical molecules touch one another with marvelous frequency, and wherein there is frictional contact with the solid but seemingly inert lithosphere. In the outer tenuous zone, indeed, the molecular flights are freer and the excursions are without assignable limits. I believe an appropriate atmosphere of philosophy is as necessary to the wholesome intellectual life of our sciences as is the earth's physical atmosphere to the life of the planet. None the less, it must ever be our endeavor to reduce speculation to philosophy, and philosophy to science. For the perpetuation of the necessary philosophic atmosphere, we may safely trust to the evolution of new problems concurrently with the solution of the old.

But granting the importance of the philosophic element, we doubtless agree without hesitation that the solid products of accurate and complete observation, natural or experimental, are the bed-rock of our group of sciences. The first great object sought by laudable methods is, therefore, the promotion of the most accurate, searching, exhaustive, and unbiased observation that is possible. One of the earliest efforts in behalf of our sciences, therefore, was naturally directed to the task of promoting the best observational work. It was soon discovered that two chief dangers threatened the worker, — bias and incompleteness. To guard against the first there was evolved

The Method of Colorless Observation

Under its guidance, the observer endeavors to keep his mind scrupulously free from prepossessions and favored views. However tensely he may strain his observing powers to see what is to be seen, he seeks solely a record of facts uncolored by preferences or prejudices. To this end, he restrains himself from theoretical indulgence, and modestly contents himself with being a recorder of nature. He does not presume to be its interpreter and prophet. At length, in the office, he gathers his observations into an assemblage, with such inferences and interpretations as flow from them spontaneously, but even then he guards himself against the prejudices of theoretical indulgence.

Laudable as this method is in its avoidance of partiality, it is none the less seriously defective. No one who goes into the field with a mind merely receptive, or merely alert to see what presents itself, however nerved to a high effort, will return laden with all that might be seen. Only a part of the elements and aspects of complex phenomena present themselves at once to even the best observational minds. Some parts of the complex are necessarily obscure. Some of the most significant elements are liable to be unimpressive. These unobtrusive but yet vital elements will certainly escape observation unless the mind is forced to seek them out, and to seek them out diligently, acutely, and intensely. To make a reasonably complete set of observations, the mind must not only see what spontaneously arrests its attention, but it must immediately draw out from what it observes inferences, interpretations, and hypotheses to promote further observations. It must at once be seen that if a given inference be correct, certain collateral phenomena must accompany it. If another inference be correct, certain other phenomena must accompany it. If still a third interpretation be the true one, yet other phenomena must be present to give proof of it. Once these suggestions have arisen, the observer seeks out the phenomena that discriminate between them, and, under such stimulus, phenomena that would otherwise have wholly escaped attention at once come into view because the eye has now been focused for them. It may be affirmed with great confidence that without the active and instantaneous use of these concurrent processes the observer will rarely, if ever, record the whole of any one set of significant elements, much less the whole of all sets. His record will contain incomplete parts of different sets of significant elements, *but no complete set of any one.* The obscure factors of each set are quite sure to be overlooked and the obtrusive factors of several sets indiscriminately commingled. The method of colorless observation is thus seriously defective in the completeness of its products, while it successfully guards them from bias.

Standing over against it, in strong contrast, is the method which at once endeavors to seek out and put together the phenomena that are thought to be significant. This leads promptly to the construction of a theory or an explanation which soon comes to guide the work and gives rise to

The Method of the Ruling Theory

The chief effort here centres on an elucidation of phenomena, not on an exhaustive determination of the facts. Properly enough the crown of the work in the end, explanation is brought to the forefront and eagerly made the immediate object of endeavor. As soon as a phenomenon is presented, a theory of elucidation is framed. Laudable enough in itself, the theory is liable to be framed before the phenomena are fully and accurately observed. The elucidation is likely to embrace only the more obtrusive phenomena, not the full complement of the obtrusive and the unimpressive. The field is quite likely to present many repetitions of the leading phenomena and a theory framed to fit those that first arrest attention naturally fits the oft-recurrent phenomena of the same class. While there may be really no new evidence, nor any real test, nor any further inquiry into the grounds of the theory, its repeated application with seeming success leads insidiously to the delusion that it has been strengthened by additional investigation. Unconsciously then it begins to direct observation to the facts it so happily elucidates. Unconsciously the facts to which it gives no meaning become less impressive and fall into neglect. Selective observation creeps insidiously in and becomes a persistent habit. Soon also affection is awakened with its blinding influence. The authorship of an original explanation that seems successful easily begets fondness for the intellectual child. This affection adds its alluring influence to the previous tendency toward an unconscious selection. The mind lingers with pleasure upon the facts that fall happily into the embrace of the theory, and feels a natural indifference toward those that assume a refractory or meaningless attitude. Instinctively there is a special searching-out of phenomena that support the theory; unwittingly also there is a pressing of the theory to make it fit the facts and a pressing of the facts to make them fit the theory. When these biasing tendencies set in, the mind soon glides into the partiality of paternalism, and the theory rapidly rises to a position of control. Unless it happens to be the true one, all hope of the best results is gone. The defects of this method are obvious and grave.

It is safe to say, however, that under this method, with all its defects, many facts will be gathered that an observer of colorless attitude would have quite overlooked. The reverse may doubtless also be said. An effort to avoid the dangers at once of the colorless Scylla and the biasing Charybdis gave rise to

The Method of the Working Hypothesis

This may be regarded as the distinctive feature of the methodology of the last century. This differs from the method of the ruling theory in that the working hypothesis is made a means of determining facts, not primarily a thesis to be established. Its chief function is the suggestion and guidance of lines of inquiry; inquiry not for the sake of the hypothesis, but for the sake of the facts and their final elucidation. The hypothesis is a mode rather than an end. Under the ruling theory, the stimulus is directed to the finding of facts for the support of the theory. Under the working hypothesis, the facts are sought for the purpose of ultimate induction and demonstration, the hypothesis being but a means for the more ready development of facts and their relations, particularly *their relations.*

It will be seen that the distinction is somewhat subtle. It is rarely if ever perfectly sustained. A working hypothesis may glide with the utmost ease into a ruling theory. Affection may as easily cling about a beloved intellectual child under the name of a working hypothesis as under any other, and may become a ruling passion. The moral atmosphere associated with the working hypothesis, however, lends some good influence toward the preservation of its integrity. The author of a working hypothesis is not presumed to father or defend it, but merely to use it for what it is worth.

Conscientiously followed, the method of the working hypothesis is an incalculable advance upon the method of the ruling theory, as it is also upon the method of colorless observation; but it also has serious defects. As already implied, it is not an adequate protection against a biased attitude. Even if it avoids this, it tends to narrow the scope of inquiry and direct it solely along the lines of the hypothesis. It undoubtedly gives acuteness, incisiveness, and thoroughness in its own lines, but it inevitably turns inquiry away from other lines. It has dangers, therefore, akin to its predecessor, the ruling theory.

A remedy for these dangers and defects has been sought in

The Method of Multiple Working Hypotheses [1]

This differs from the method of the simple working hypothesis in that it distributes the effort and divides the affections. It is thus in some measure protected against the radical defects of the two previous methods. The effort is to bring up into distinct view every rational explanation of the phenomenon in hand and to develop into working form every tenable hypothesis of its nature, cause, or origin, and to give to each of these a due place in the inquiry. The investigator thus becomes the parent of a family of hypotheses; and by his paternal relations to all is morally forbidden to fasten his affections unduly upon any one. In the very nature of the case, the chief danger that springs from affection is counteracted. Where some of the hypotheses have been already proposed and used, while others are the investigator's own creation, a natural tendency to bias arises, but the right use of the method requires the impartial adoption of all into the working family. The investigator thus at the outset puts himself in cordial sympathy and in the parental relations of adoption, if not of authorship, with every hypothesis that is at all applicable to the case under investigation. Having thus neutralized, so far as may be, the partialities of his emotional nature, he proceeds with a certain natural and enforced erectness of mental attitude to the inquiry, knowing well that some of the family of hypotheses must needs perish in the ordeal of crucial research, but with a reasonable expectation that more than one of them may survive, since it often proves in the end that several agencies were conjoined in the production of the phenomenon. Honors must often be divided between hypotheses. In following a single hypothesis, the mind is biased by the presumptions of the method toward a single explanatory conception. But an adequate explanation often involves the coördination of several causes. This is especially true when the research deals with complicated phenomena such as prevail in the field of the earth-sciences. Not only do several agencies often participate, but their proportions and relative importance vary from instance to instance in the same class of phenomena. The true explanation is therefore necessarily multiple, and often involves an estimate of the measure of participation of each factor. For this the simultaneous use of a full staff of working hypotheses is demanded. The method of the single working hypothesis is here incompetent.

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[1] In this sketch I have drawn freely upon my paper on "The Method of Multiple Working Hypotheses," *Journ. Geol.*, v, 1897.

The reaction of one hypothesis upon another leads to a fuller and sharper recognition of the scope of each. Every added hypothesis is quite sure to call forth into clear recognition neglected aspects of the phenomena. The mutual conflicts of hypotheses whet the discriminative edge of each. The sharp competition of hypotheses provokes keenness in the analytic processes and acuteness in differentiating criteria. Fertility in investigative devices is a natural sequence. If, therefore, an ample group of hypotheses encompass the subject on all sides, the total outcome of observation, of discrimination, and of recognition of significance and relationship is full and rich.

Closely allied to the method of multiple working hypotheses is

The Method of Multiple Series

In many of the more complex problems of the earth-sciences the basal facts are but imperfectly determined, *e. g.*, the rate of rise of internal temperature, the rigidity of the earth's body, the thermal conductivity of the earth's interior, the amount of the earth's shrinkage, the extent of lateral thrust in the formation of folded mountains, and many others, indeed most others. There is need to deal with these problems notwithstanding the imperfection of the basal data, for in many cases these must long remain imperfect. Moreover, there is need to treat these problems tentatively to determine what fundamental facts are really needed, how these can best be secured, and with what precision they must be determined. Preliminary trial may save much tedious and expensive experimentation. It is as foolish to cultivate sterile soil in science as in agriculture, and preliminary tests may show that given soils are necessarily sterile. In many cases, all the needs of the problem may be met by a multiple series of assumptions covering the full range of a probable fact. In most cases it is easy to see that the value of a given fundamental factor cannot range beyond certain extremes on either hand. If a series of values ranging from the one extreme to the other be used simultaneously in the inquiry, the full range of results dependent on this factor may be covered. In some inquiries this serves as well as if the exact truth were known, for whatever the assignable value, certain deductions cannot stand. In other cases it will be shown that a very slight change in the value of the basal factor will wholly change the outcome, and hence that extremely accurate determinations must be made before any trustworthy solution can be reached. Expensive determinations in the first case are folly; very accurate determinations in the second are to be sought at any cost. Conclusions on imperfect data in the one case are perfectly safe; conclusions without precise determinations in the other are folly. It is to be hoped that, with the wider adoption of the method of multiple series, tables of serial determinations covering the data of the more vital phenomena of the earth-sciences will be constructed, as tables of physical constants now are.

The Method of Regenerative Hypotheses

In the method of multiple hypotheses, the members of the group are used simultaneously and are more or less mutually exclusive, or even antagonistic. Supplementary to this method is the use of a *succession* of hypotheses related genetically to one another. In this the results of an inquiry under the first hypothesis give rise to the assumptions of the succeeding hypothesis. The precise conclusions of the first inquiry are not made the assumptions of the second, for the process would then be little more than repetitive, but the revelations and intimations, perhaps the incongruities and incompatibilities, of the first results beget, by their suggestiveness, the basis of the second. The latter is the offspring of the former, but between parent and offspring there is mutation with an evolutionary purpose. A cruder first attempt generates a more highly organized and specialized working scheme fitted to the new state of knowledge developed. The method is specially applicable to elaborate inquiries, particularly those in which the premises are imperfect and a long logical chain is hung upon them. The discussions of our great fundamental conceptions furnish the best examples, chiefly examples of the lack of a systematic regenerative method. Among these, two general classes may be recognized, (1) those of a rather rigorous type, as, for a distinguished example, the researches of George Darwin on tidal reaction and the history of the earth and moon, and (2) those of a looser and sometimes rather metaphysical type, which I shall try to illustrate by the doctrine of determinism. In all cases, *assumptions* are made the basis of the procedure. Absolute premises are not available. Taking its start from these assumptions, the process pursues a long course and at the end conclusions of great import are often drawn. Usually the process rests there, and in this lies a serious shortcoming. It should give rise to a new process of a higher order. Not seldom, a critical study of the results will reveal features that were not recognized nor suspected in the original assumptions, though really there. Out of these revelations should grow new assumptions and a new process. The second conclusion may in like manner betray unsuspected qualities, and these should beget still other assumptions, and so the procedure should continue until the field is exhausted.

To choose a specific illustration is not a little delicate, for to be most familiar it must be of the negative type, but I fear such an illustration is the only way to convey clearly the meaning here intended. I therefore venture to choose one so eminent and so admirable, even with its limitations, that any suggestion of shortcoming will in no wise dim the luster of a great achievement. In the classic investigations of George Darwin on tides and their astronomic consequences, a viscous earth is assumed as the starting-point, with properties such that the tidal protuberance is carried forward by the rotation of the earth to the point which gives the maximum effect on the motions of the earth and moon. These assumptions run potentially through the whole train of brilliant mathematical deduction. At the end of the inquiry, or if not of this particular inquiry, at least of collateral inquiries, the conclusion is reached that the earth is a rigid body comparable to steel. Between such a rigid body and such a viscous earth as was assumed at the outset of the inquiry, there is a seeming incongruity. This, under the regenerative method, suggests a new investigation on the assumption that the earth is a very rigid body, with the further assumption that it has high elasticity of form, such that its protuberance may perhaps not be carried forward to the degree previously postulated. These new assumptions are the more imperative because they are supported by inquiries based on quite independent lines. In framing the new hypothesis an advance in detail and in organization is to be sought on the evolutionary principle already indicated. If the earth as a whole is as rigid as steel, and the outer part is, as we know, formed of rock much less rigid than steel, the interior must be much more rigid than steel, and there must be a differential distribution of rigidity. The new inquiry may then well start with the assumption of increasing rigidity toward the centre. Postulating an earth so constituted, a first step of the regenerated inquiry might well be an effort to learn not only the amount of the tidal protuberance, but also the *position* of the protuberance, since its position is as essential as its amount in influencing the motions of the earth and moon. As a geologist I venture to entertain the belief that exhaustive inquiry on such regenerative lines would bring forth results in harmony with geological evidences, with which the well-known conclusions heretofore reached seem to be at fatal variance.

The earth-sciences are not purely physical sciences. They concern themselves with life and with mentality, as well as with rocks, ocean, and atmosphere. Our group is exceptionally comprehensive in the range of its subjects. Our methods should hence be such as to encompass the whole field. They should give us ultimately a complete working system of thought relative to all the earth is or holds. In some sense the earth-sciences must come to comprehend the essentials of all the sciences. At least as much as any other scientists, we are interested in the fundamental assumptions of all the sciences, and in their consistent application. To touch hastily this broader field, I choose a second illustration of the method of regenerative hypotheses from the relations between the assumptions of science and the conclusions of science.

As our working basis, we assume that our perceptions represent reality, when duly directed and corrected, but that error and illusion lurk on all sides and must be scrupulously avoided. We assume that we are capable of detecting error and of demonstrating truth; and that, as requisite means, we have choice, and some measure of volitional command over ourselves and over nature.

Starting thus with assumptions that embrace choice and the possibility of error, and going out into physical research, most of us have concluded that antecedents are followed rigorously by their consequents. Going out a step further into the chemico-biological field and noting the close interrelations between physical and vital phenomena, many of us have been led to a belief in their ultimate identity. Going out a step further into the mental field, not a few of us have concluded that an unvarying sequence of antecedents and consequents reigns here also. But this seems to contradict the assumptions with which we started. Our primary assumptions embraced choice, volitional control, and the alternative of reaching truth or falling into error according to our self-directed discrimination.

What is to be done in the face of this seeming contradiction? The method of regenerative hypotheses answers that a new set of assumptions begotten of the contradictory conclusion should be made the basis of a new inquiry, and, if possible, of a new working hypothesis. Instead of the usual assumption of choice, and of the possible alternative of reaching truth or falling into error, let the assumption be that all acts of the mind are parts of a rigorous chain of antecedents and consequents. Let it be assumed that no swerving from the predetermined sequences is possible, that every thought and every act follows its antecedents with absolute rigor, no real choice, or volition, or alternative between accuracy and error, being possible.

Let this set of assumptions be tried as a working hypothesis. If investigation be possible under it, let such investigation cover the whole ground of what we call truth and error. Let a distinction be drawn between absolutely predetermined mental actions corresponding to truthfulness on the one hand, and falsity on the other, if this be possible, and out of the former let science be constructed and let it be shown *why it is science*, and let the latter be disposed of in some suitable way. In other words, let the doctrine of determinism *be put into workable form*, and carried into effect in all its applications, with every step true to the primary assumptions. If this can be done successfully, we shall have a wholly new working basis for the production of science, with new criteria of science. If it cannot be done, and the hypothesis of determinism is unworkable, let it be cast aside like any other unworkable hypothesis. Whatever metaphysicians may think of an unworkable scheme, scientific investigators may as well send it to the junk-shop.

Huxley once delivered himself of an able exposition of determinism. It was severely criticised by a fellow countryman who seemed to Huxley to have dealt with him unjustly, and he poured out the vials of his rhetorical wrath upon his critic as only Huxley could. But if determinism be true, I do not see how Huxley's critic could have swerved by a turn of a phrase from what he wrote, and Huxley's wrath was not more consistent than that assigned to Xerxes when he lashed the stormy Hellespont because it thwarted his purpose. But in this I may be wholly wrong. *Let determinism prove itself by giving rise to a complete and systematic working hypothesis.*

Whether this can be done or not, let any other basal assumptions suggested by the inquiry be made the ground of like attempts and be developed into full working hypotheses, if possible, and so continue the effort until the whole field is covered. Let it be seen what can and what cannot be put into the form of a working system.

In this second illustration of the method of regenerative hypotheses, I have touched questions not usually thought to belong to the earth-sciences. It is none the less true that they are basal to the earth-sciences, as they are to all science, and to all true philosophy as well. The earth-sciences are entitled to probe for their own bottom as well as other sciences, or any philosophy, and it is altogether wholesome that they should do so. The most serious source of error in the development of the earth-sciences, in my judgment, is our relative neglect to probe fundamental conceptions and to recognize the extent to which they influence the most common observations and interpretations. We need a method of thought that shall keep us alive to these basal considerations. To this end I believe it to be conducive to soundness of intellectual procedure to regard our whole system of interpretation as but an effort to develop a consistent system of workable hypotheses. I think we should do well to abandon all claims that we are reaching absolute truth, in the severest sense of that phrase, and content ourselves with the more modest effort to work out a system of interpretation which shall approve itself in practice under such tests as human powers can devise. Wherein lie

The Basal Criteria of our Sciences

I believe they lie essentially in *the working quality.* Whatever conforms thoroughly to the working requirements of nature probably corresponds essentially to the absolute truth, though it may be much short of the full truth. That may be accepted, for the time being, as true which duly approves itself under all tests, *as though it were true.* Whenever it seems to fail under test in any degree, confidence is to be withdrawn in equal degree, and a rectification of conceptions sought. This may well hold for all conceptions, however fundamental, whether they relate to the physical, the vital, or the mental phenomena which the earth presents. Let us entirely abandon the historic effort of the metaphysicians to build an inverted pyramid on an apex of axioms assumed to be incontestable truth, and let us rear our superstructure on the results of working trials applied as widely and as severely as possible. Let us seek our foundation in the broadest possible contact with phenomena. I hold that *the working test* when brought to bear in its fullest, most intimate, and severest forms, is *the supreme criterion* of that which should stand to us for truth. Our interpretative effort should, therefore, be to organize a complete set of working hypotheses for all phenomena, physical, vital, and mental, so far as appropriate to our sphere of research. These should be at once the basis of our philosophy and of our science. These hypotheses should be constantly revised, extended, and elaborated by all available means, and should be tested continually by every new relation which comes into view, until the crucial trials shall become as the sands of the sea for multitude and their severity shall have no bounds but the limits of human capacity. That which under this prolonged ordeal shall give the highest grounds of assurance may stand to us for science; that which shall rest more upon inference than upon the firmer modes of determination may stand to us for our philosophy; while that which lies beyond these, as something doubtless always will, may stand to us for the working material of the future.

THE RELATIONS OF THE EARTH-SCIENCES IN VIEW OF THEIR PROGRESS IN THE NINETEENTH CENTURY

BY WILLIAM MORRIS DAVIS

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Facts of earth-science have now been so abundantly acquired and so thoroughly systematized that there is some danger of our substituting the schemes in which earth-knowledge has been summarized for first-hand knowledge of the earth itself.

For a fundamental matter like the globular form of the earth, we resort to a hand globe, so admirable in its imitation of nature that we must beware lest the little globe rather than the earth in its true dimensions satisfies our imagination. We have so conveniently divided the geological record of the earth's history into ages and periods that their easily repeated names are apt to replace the laborious conception of long divisions of time.

Our escape from the danger of taking scheme for fact has lain in the resort to individual observation, and the past century must long be famous for the extent to which advantage has been taken of the opportunity for outdoor study.

The earth has been explored and measured as never before. The lands have been mapped, the oceans have been charted, by original observers. The air has been followed in its circuits, great and small. The structure of the earth's crust has been patiently traced out. Thus "Go and see" came to be our watchwords one hundred years ago. As long as we, like Antæus of old, can return to the earth for new stores of the strength that we find in facts, we need not fear being strangled by any voluminous Hercules of theory.

It is the active appeal to observation that has checked the freedom of speculation which our brilliant predecessors enjoyed in an earlier century, when their fanciful schemes were little restrained by the barriers of fact that have since then been built up on every side. Indeed, schemes came to be for a time so much in disrepute that some investigators wished to suppress theorizing altogether, as was seen in the effort to supplant the name, geology, by geognosy. I rejoice that the effort did not succeed; for if earth-science were really limited to facts of direct observation, it would be at best a dreary subject.

How uninspiring would be such a knowledge of tides as could be gained only by actual observation along the seashore! A collection of such records would be like an orphanage, where the foundlings are doubtless well cared for and thoroughly drilled in their little duties, and yet left without the inspiriting, enlarging influence of parental care that they find on adoption into the family of earth, moon, and sun.

Hence, whatever the danger of schemes and theories, they give the best of life to our bodies of facts, and our science cannot survive without them. Indeed, we have come to know that the danger of systems and theories lies not in their dependence on the imagination, but in the possibility of their careless growth and of their premature adoption, and even more in the acceptance of a personal responsibility for their maintenance instead of leaving that responsibility to external evidence.

If there is any subject in which the aid of schemes and theories based on observations has been absolutely necessary for progress, it is earth-science, where so many of the essential facts are invisible. It cannot be too carefully borne in mind that observation and theory are alike in their objects, however different they may be in their methods. Both seek to discover the facts of their science: one deals with facts that are visible to the outer eye; the other with facts that cannot be seen, whether because they are too small or too large for outer vision, or because they are hidden within the earth, or in past time, or because they are impalpable abstractions or relations. In both, fancy is sometimes taken for fact, more often so, perhaps, in theorizing than in observing; but we must not for that reason give up either means of investigation. We have learned that both observing and theorizing must be carefully conducted: and we have therefore replaced the earlier watchwords, "Go and see," with the later ones, "See and think."

We may still give praise to those who apply themselves chiefly to gaining first-hand knowledge of observable facts, but we have learned to give greater praise to those who, on a good foundation of visible facts, employ a well-trained, constructive imagination in building ingenious and successful theories which shall bring to sight the invisible facts. We have been longest familiar with the need of theory in those branches of our subject which have, by reason of association with mathematical problems, traditionally employed deductive methods in their discussion, as in earth-measurement; we are least familiar with it in those branches that have until lately followed for the most part inductive or even only empirical methods, as has so generally been the case with geography.

For example: in the study of the tides already referred to, how unanimous we are as to the inadequacy of inductive methods; how universally we accept the marvelous theoretical scheme of interaction between planet and satellite, deduced from tidal theory; how we admire its extension to the supposed relation of the inferior planets to the sun. But in general geography, how little attention has been given to the deductive and systematic consideration of its many problems: how many geographers still look rather askance at those of their number who propose to treat geographical problems through theory as well as through observation! It seems to me clear that, while the earlier progress of geography was very largely inductive, the later progress has been largely determined by a free acceptance of deductive as well as inductive methods, and that geography as well as geology is to-day profiting greatly from the use of our faculty of insight as well as of outsight.

The objections that are not infrequently urged against the employment of indirect, inferential, as well as of direct, observational methods in certain branches of our science come from two sides. On one side is a misapprehension as to the nature of our tasks, a belief that our work may really be largely inductive, that observation alone will suffice, if patiently continued, to discover all pertinent facts. This is a serious mistake: there is everywhere more unseen than seen. On the other side is the fear that theories may become our masters and that we may appeal to them as infallible, and thus set ourselves up as authorities. This is a most natural induction from the history of our earlier progress, for we have repeatedly seen the sincere young investigator grow into the impatient old autocrat: it is a bit of human nature that we share with the rest of the world; it is analogous to the change of meaning in the word tyrant, from a mere king to an arbitrary despot. But there is another verbal analogy in the change of the word skeptic, from inquirer to doubter, and it is this analogy that we are now following. We have learned to doubt because we know we may be deceived; we mistrust careless eyes as well as careless thoughts, and insist that careful scrutiny be given to the work of each: we reduce the dangers of theorizing, just as we reduce the errors of observing, not by avoiding that indispensable means of investigation, but by practicing it carefully, until we become experts in thinking as well as in seeing; and all this constitutes an important element in our recent progress.

In spite of what has already been gained by good theorizing, few realize how largely earth-science, apparently a matter of observation, is really built up of inferences that go far beyond mere inductions. Many of the inferences have gained a certification so good and so familiar that in respect to verity they take rank with seen things, and we are apt to forget their origin. The successive deposition of bedded rocks, the organic origin of fossils, the original horizontality and continuity of folded and eroded strata: these inferences are today accepted as if they had never been doubted; but they all were once doubted, and they had to make their way against opposition. Whatever order of certainty they have now acquired, they are not facts of observation, but facts of inference; and, like the great body of earth-science, these well-accepted facts of past time have not been determined by direct seeing, but by inference on the basis of seeing. We may, therefore, justly claim great progress for earth-science not only in the extent and accuracy of our observations, but also in the extent and accuracy of our inferences. While there is yet need of more conscious recognition and more thorough training, especially in the deductive processes by which many problems may be solved, we may still say that among the most significant steps that we have taken in the past century are those by which the necessity and the value of theorizing have gained frank acceptance among investigators and by which many of the results of theorizing have gained an order of verity that compares well with that of facts of mere observation.

An illustration of this phase of our progress is to be found in two definitions, each of which has a certain currency. By some writers, geology is defined as the study of the earth's crust, thus emphasizing the observational side of the subject; by others, geology is defined as the study of the earth's history, thus giving fuller recognition to the growth of inference upon observation. The second definition does not lessen the essential importance of observation as the foundation of knowledge, but it accords a proper value to inferences, and in this way is characteristic of what seems to me sound scientific progress. The earth's crust contains the incomplete, partly concealed, partly undecipherable records on which we are to construct the science of geology: just as human monuments and writings are the records on which we are to construct human history; but in neither case are the records and the history identical, for the history in both cases includes a great body of inferences as well as of more directly recorded or observed facts.

The wholesome appeal to observation in the search for visible facts has loosened the control of supposed authority, and has given us much of the freedom necessary for progress; but the assistance of the trained imagination in the search for invisible facts has in a far greater degree corrected the assumptions of an earlier stage of inquiry; it has even revised the dicta of philosophy and remodeled the dogmas of religion.

The inferential element of our progress has worked most beneficently. It is largely through our inferences that we have come to recognize the interdependence of the different parts of earth-science. The climatologist may remain as provincial as he wishes; or he may enter through the gateway of present conditions the vast domains of past time, and on the way make friends with all the world; for he will then join hands with the petrographer who has evidence of ancient desert conditions in the form of the grains in certain sandstones; and with the paleontologist who infers the existence of ancient ocean-currents from the drift of graptolite stems; and with the glaciologist who is asking the astronomer and the physicist whether one or the other of them can best account for the pleistocene ice-sheets.

Not only do the different parts of earth-science thus connect with one another, but, as the last illustration showed, they interlace most interestingly with the branches of other sciences in the forest of knowledge. The systematist would indeed be at a loss to classify our work, if in classification he thought to keep it apart from other kinds of work. Better let it grow up naturally, with interlacing treetops and crowded underbrush, each tree showing its individualized effort in the universal competition, than seek to trim it into an orchard of separate trees. The departments and sections into which we are divided in this Congress do not represent objectively disconnected groups and units of knowledge, but associated parts in contiguous growths of acquisition; we must not hesitate to go out of conventional bounds, and to trespass, as it is called, on other departments, when it is to our advantage. Others are surely free to do the same by us. When we employ methods called mathematical and physical in our study of the winds, the profit is not only found in direct results but also in the use of deduction and experimentation, so familiar in mathematics and physics, and so much less practiced, yet so much needed, in all parts of earth-science: in return we supply data for the study of the phenomena of gases on the largest terrestrial scale.

We must be chemists, geometricians, and physicists in studying the minerals of the earth's crusts; and in return we supply to the chemist a great variety of natural compounds, and to the physicist the material basis for a remarkable variety of optical phenomena. We must, indeed, marvel at the skill displayed by minerals — which invade, colonize, migrate, and settle again in the dark inner world — in handling external rays of light, and we may wonder if they have not had some preliminary practice on radiations of a kind that physicists have yet to describe. Admirable also are the crystalline forms that give realization to the early inventions of geometers, much in the way that planets and comets give us in their orbits great natural examples of the conic sections, familiar for centuries as mathematical abstractions.

But it is particularly with biology in all its branches that we have learned to borrow and lend. The evolution of the earth and the evolution of organic forms are doctrines that have reinforced each other: the full meaning of both is gained only when one is seen to furnish the inorganic environment, and the other to exemplify the organic response. Without question, the interaction here discovered in the working of two great processes is the most notable discovery of the present century, not the less glorious because we share it with other sciences. For if they have to do with the players, we have to do with the scenery and the properties for the all-world stage, where the success of the players has been conditioned by our setting since the play began. In the universal habit of respiration as a means of gaining the energy by which all plants and animals do their life-work, we see a successful response to the presence of free oxygen, mixed in the air or dissolved in the waters, and hence we infer that free oxygen has been present in atmosphere and oceans at least as long as life has existed on the earth. In the development of stem and root, of dorsal and ventral parts, we perceive the everlasting persistence of gravity: to fail in the recognition of this elementary example of interaction between life and environment would be on a par with neglect of the earth's rotation because the evidence of it is found in the commonplace consequences of sunrise and sunset.

The races of mankind, so inappropriately treated as a chapter of physical geography in many of our text-books, but really the prime factor of political geography, are obviously determined by the larger features of the lands; just as the development of the higher organic forms has been determined, not on the monotonous ocean-floors, but on the lands, where variety has really been the very spice of life.

If we turn to history, — not simply to the politics of the past, but to the real history of human thought and action, — the progress of our own science furnishes innumerable examples of response to environing opportunity: how natural that the later geological series should have been first deciphered in England, where it is so well displayed; that the study of earthquakes and the invention of seismographs thrive in Italy and Japan; and that geomorphy advanced rapidly in our arid West through the study of the nude, just as sculpture flourished in Greece.

It is but the commonplace of economics to show the large dependence of modern civilization on the occurrence of mineral deposits. Like the quiescent crystalline forces in the rounded quartz-grains of ancient sandstones, still capable of determining the settlement of new molecules around the old ones, the marvelous stores of dormant energy and strength in beds of coal and iron ore have long bided their time. After ages of neglect, they have become the centres of great populations: and, now that our princes of industry have through countless difficulties touched and awakened them to life, we find a new meaning in the old fairy-story of the Sleeping Beauty.

Even those broader considerations that we meet in philosophy and religion have developed new phases as the schemes of earlier times have been modified in view of the geological record: the place of work in the world, not a curse, but a duty; the date of the golden age, not behind us, but ahead; the view of death, not a punishment, but a natural element in the progress of life; even the conception of immortality has come to be — with some — directed less to speculations about a continued life elsewhere than to the study of the continuity of life here.

Religious ideas themselves — at least, when we examine them objectively in the beliefs of others than our own people — are seen as if in a mirror held to nature: and the very gods of the lower religions are but reflections of the powers of the earth.

It is only when we consider these broad phases of earth-science that we gain our share of profit from the revolution that replaces the teleological philosophy of the first half of the nineteenth century by the evolutionary philosophy of the last half. Our conception of the earth as well as of its inhabitants has been profoundly modified by this revolution, and much of our progress has been conditioned on the full acceptance of the newer view.

Now if apology is needed for introducing the preceding considerations, which some might call irrelevant, let me urge that, whatever share they may make of other sciences, they are also so closely grafted into one or another branch of earth-science that we, as geologists or geographers, cannot afford to neglect them. In so far as they are related to elements of our science as consequences are to causes, as responses are to environment, we must take at least some account of them, even if their study in other relations is left to specialists in other subjects. In doing so, we are only carrying out our work to its legitimate conclusion. It is without question our responsibility to study the ancient inorganic conditions that determined the location and the migration, the development and the extinction, of ancient faunas, for these conditions were at least in part geological factors of one kind or another; it is equally our responsibility to study the modern conditions that determine the location of cities and the routes of trade, for these conditions are largely geographical factors; but the examples of organic response here adduced are merely a few of many, and all the rest stand on an equal footing with them, whether they are commonly classed with biology or history, with economics or religion. We long ago saw that the more simple, immediate, and manifest examples of organic, especially of human responses, belonged in the realm of geography; and from this beginning we now realize that there is no stopping-place till we include all other examples, complex, indirect, or obscure as some of them may be; for there is a graded series of connecting examples from the most artful human response down to the most unconscious plant response, and from the immediate responses of to-day back to the earliest responses of the geological ages.

It would be most arbitrary to draw a division in our studies, when no division exists in the things studied. It is therefore a piece of good fortune that geographers are coming to follow the practice of geologists, and thus to accept among their responsibilities the great breadth of physiographic and ontographic relationships existing to-day, as geologists have accepted them for the past. And it is also a good fortune that biologists are coming to accept the responsibility of studying environment as well as response: for only in this way have the earth and its inhabitants really learned to know each other. I rejoice, therefore, whenever a student of earth-science completes his studies by carrying them forward to their organic consequences, as seen from the side of the earth; and again whenever a student of biology, of language, of economics, of religion, carries his studies backward to a consideration of inorganic causes, as seen from the side of life: for thus and thus only we may hope that the knowledge of both causes and consequences shall increase in fullness. Our present understanding of this interdependence, not only of different branches of our own science, but of the branches of our own and of other sciences, is truly a great step toward the solution of the wonderful riddle of the world.

The real foundation of the broad consideration of earth-science rests on the continuity of ordinary processes through the long periods of recorded earth-history. Nothing has so profoundly modified the appreciation of other subjects, as well as of our own, as the teaching of geology concerning the conception of time and the long procession of orderly events that has marched through it. Such a conquest of the understanding is enough to make us proud indeed; yet when we realize how short a share of time has been allotted to us, how sincere should be our humility! To-day we may be lords of creation, powerful through cephalization: yet in face of the repeated extinction of dominant races in geological history, how can we think otherwise than that we are clad only in a little brief authority; how can we seriously believe that we represent the highest stage, the acme of organic development, comforting and flattering as this deductive opinion may be!

The conception of the continuity of processes, without extra-natural interference, has been forced to fight its way against opposition; now it has gained at least a very general verbal acceptance among us, and is quietly drifting into popular belief. To realize its full meaning is an arduous task, not only because of the opposition of inherited prejudices, but even more because of the inherent difficulty of the problem. To think that processes such as those of to-day have done all the work of the past is appalling; yet we are constrained to believe it. Even as waves, beaten up in a stormy sea subside after the winds are calmed, so the mountain waves or wrinkles of the earth's crust, growing as long as orogenic storms are at work, are in time calmed to plains; and this not by unusual processes, but by the patient weathering and washing of scraps and grains. While these slow changes go on in the extinction of mountain systems, the races of plants and animals that originally gained possession of the lofty young mountains, that grew up with them, so to speak, must either adjust themselves to the changes in their surroundings, or migrate to other homes, or vanish, all in due order through the flowing current of time.

Nowhere is the orderliness of geological changes better attested than in the forms of ridge and valley seen to-day in various examples, young and old, of wasting mountain ranges themselves, and in the systematic adjustment that is attained by the drainage-lines with respect to the structures on which they work. Here indeed is cumulative testimony for uniformitarianism; for nothing but the long persistence of ordinary processes can account for these marvelous commonplaces. So wonderful is the organization of these land and water forms in physiographic maturity and old age, so perfect is their systematic interdependence, that one must grudge the monopoly of the term organism for plants and animals, to the exclusion of well-organized forms of land and water. By good fortune, evolution is a term of broader meaning: we may share its use with the biologists; and we are glad to replace the violent revolutions of our predecessors with the quiet processes that evolution suggests.

It is the assurance of orderly continuity that binds the past to the present in the endless sequence of events, and shows us that geography is only to-day's issue of a perpetual journal, whose complete files constitute geology. He must be a geographer of the old school who would now maintain that his subject, in content and treatment, really belongs outside of the geological curriculum. It may, on the other hand, be justly contended that the whole of earth-science is made up of geographic sheets, — until to-day, paleogeographic, if you like, — all horizontally stratified with respect to the vertical time-line. In every sheet we find news of the relation of earth and life, of environing control and organic response, of physiography and ontography. Every little item of news here published is worthy of close attention. The reader may examine all sorts of items on a single sheet and consider their temporary, areal distribution, and so acquire the geographic view; or he may examine the changing items of certain areas, following their chronological sequence in successive sheets, and so acquire the geological view; but it would be unfortunate if, in so doing, he did not perceive the interchangeable relations of these two methods of investigation.

There is to my understanding a great profit that has been gained from conceiving the whole body of our science in the way thus suggested. Branches such as meteorology and terrestrial magnetism, which we ordinarily treat as parts of physical geography and thus associate with present time, are seen really to have their ancient as well as their modern, their geologic as well as their geographic phases. We can gain some hints as to ancient meteorology, for we find records of paleozoic raindrops, of remote glacial deposits, and we hope yet to find evidence concerning the distribution of early climatic zones. As far as ancient records of this kind can be pieced together, we may study them in their momentary or geographic, as well as in their continuous or geologic relations. Concerning ancient phases of terrestrial magnetism, we are at a loss; yet our conception of even this branch of earth-science, as well as that of the meteorological branch, is certainly broadened when it is regarded as a contemporary of all the geological ages, and not merely as a latter-day characteristic of the globe.

Similarly, those geological events which we are accustomed to treat in their time-sequence gain fuller meaning when they are decomposed into their momentary elements, and when each element is treated as a geographical feature associated with its contemporary fellows. The columnar sections of stratified rocks, for example, so useful in the understanding of historical geology, are like the edgewise view of a closed book. The book must be opened, the leaves must be turned over one by one, the pages of these early records must be read, like so many gazetteers of ancient times. Never mind if some pages are worn and others are missing: those that can still be deciphered assure us that the past was generally like the present, and warrant the generalization that geology is like nothing so much as a whole series of geographies.

At the present stage of our progress, the sciences of the earth may be given a somewhat different classification from that of the eight sections into which they are divided for the purposes of this Congress. These sections, as it seems to me, represent the subjective divisions of our sciences, within each of which specialists may limit their studies more or less closely, and for each of which speakers may be provided. But when regarded objectively, the divisions, their grouping, and their relative values, must be otherwise presented. Geology objectively considered is not merely one of the earth-sciences; it is the whole of them: it is the universal history of the earth. It is true that geology has so largely to do with past time that it is not popularly understood to include the present; but it certainly does include the present, and the future also, as fast as it arrives. There is no possibility, in the understanding that we have now gained of earth-science, of stopping the geological record at any stage of the pleistocene, and calling the rest geography: that would involve the resurrection of buried theories, which held the past to be unlike the present order of things.

Conversely, geography is stultified when absolutely limited to the things of to-day, as if the things of the past were of another nature. It is of course popularly so considered, and perhaps for that reason its scientific development is stunted. When regarded objectively, the geography of to-day is nothing more nor less than a thin section at the top of geology, cut across the grain of time; and all other thin sections are so much more like the geography of to-day than they are like anything else, that to call them by another name — except perhaps paleogeography — would be adding confusion to the earth's past history instead of bringing order out of it. Our plain duty is to emphasize the continuity of events, that great result of our studies, and not to imply a break in their succession by using unlike terms for different members of a single series.

Geology thus being composed of a succession of countless geographies, geography, in its widest sense, is likewise composite, including its inorganic and its organic parts. It is particularly concerned with the surface of the earth to-day, as the home of life; but surface and to-day must here be very freely construed; for we must draw upon the sub- and supersurface parts, and on the days before to-day, whenever we find profit in so doing. When we study the shape and size of the earth, we touch upon what may be developed into geodesy. When we study the inorganic parts of the earth for themselves, in what may be called their static relations, we enter upon mineralogy and petrology, or geochemistry; for it must be remembered that water is a mineral and that air is a rock. When we study the dynamic relations of the inorganic parts of the earth, we have geophysics, within which oceanography and meteorology are subdivisions, of rank similar to terrestrial magnetism and to that large category of phenomena that includes the activities of the earth's crust. It is true that physical or dynamical geology is the heading under which erosion, volcanoes, and earthquakes, are usually treated, as if the present phenomena of the earth's crustal envelope were to be set aside from the phenomena of the hydrosphere and atmosphere, and associated chiefly with the history of the past. But we have now certainly reached a point when the unity of all these subjects, their interaction in space, and their continuity through time, demand their association in a single group of studies which shall embrace all the activities of the earth in their present manifestation; with the full understanding that the present is only the latest addition to the past and that the past is only the integration of a vast series of ancient presents.

All these present physical activities, even if carried down to such specialties as potamology and kumatology, are so closely associated with the standard subjects of geography that it is difficult and unadvisable to cut them asunder. Yet every one of them may be carried to such a degree of detail as to stand apart and gain rank as an independent study. The accuracy of the geodesist, the minuteness of the mineralogist, the high flights of the meteorologist, have now gone so far in their special development as to lead far away from each other, when they are studied for themselves, however closely their more general results may be associated.

When, however, we study the inorganic features of the earth not as independent phenomena, but as elements of organic environment, they all belong strictly in physical geography, or physiography. Parenthetically let me say that I regret the excessive breadth given to this term by British students, and the narrowness imposed upon it by those Americans who would limit it to the study of the lands. When we pursue the subdivisions of physiography, nomenclature becomes incomplete. Climatology is unique in being a name for the study of the atmosphere in so far as it determines organic environment; economic geology is a study of useful minerals and rocks, but is less strictly treated as an objective subdivision of physiography than is climatology, and there is associated with it so much of ingeniousness and artifice in the exploitation and treatment of mineral products that we are apt to put the cart before the horse and think that we make gold or coal serve our needs, instead of realizing that we make ingenious use in money and fuel of the properties that gold and coal possess, just as we make use of moving air in windmills, and of falling water in factories.

There are no special names for the phenomena of oceans or of the other divisions of physiography, considered as elements of organic environment, and there is perhaps no need of such names; yet I hold that it is desirable and even important to recognize the two ways in which the inorganic features of the earth may be studied: either for themselves, without regard to their controls over organic life; or as elements of an inhabited planet, with continuous attention to the controls that they exert over the inhabitants.

When we come to the organic inhabitants of the earth, it is evident that they fall under biology when studied for themselves, and that they may be divided under botany and zoölogy, and subdivided as often as desired. This is manifestly true as well of fossils as of living forms. When, on the other hand, the inhabitants of the earth are studied with respect to the responses that they have made to their inorganic or physiographic environment, they are appropriately included under geography. It has been recognized for many years that no geographical description of a region is complete without some account of its plants and animals, and especially of its peoples: just as no paleogeographic account of a geological horizon would be satisfying if its fossil fauna and flora were left unmentioned. But in recent years it has been seen to be necessary to treat uniformly all the organic elements of geographical descriptions in their relations to environing controls: for, as I have already shown, if a beginning is made there is no reasonable stopping-place until this end is reached.

We are, in this matter, still sometimes too much under the control of traditional methods of treatment; we do not fully enough put into practical effect the greater lessons that we have learned. The earth as the home of man is a primitive, elementary definition of geography; the earth as the home of life is more consistent with present progress. Earth-science has now certainly reached a position in which the unity and continuity of life are recognized. Let us then adopt this position as our starting-point in the organic half of geography that may be called ontography. Let us make it practically useful by treating all organic responses to environment under one general heading, even though we afterwards find it desirable to treat human responses in a separate chapter. For even if man's will sets him high above the other forms of life, it must not be forgotten that his will often leads him along physiographic lines; and that he possesses many structures and habits entirely independent of his will, and similar to the structures and habits of lower animals, as examples of ontographic responses. Even human houses and roads are only different in degree from the houses and roads made by animals of many kinds. Still more, if we accept the principle of the continuity of geography through geology, we must recognize that most of the successive geographies of the past have had nothing to do with the human will; and that man and his works are after all only modern innovations.

The chief impediment to action upon this view, which, as I have said, has been unfolded before us by the progress that our science has already made, is the habit of studying geography and geology too separately, and of regarding the former as a subject for narrative treatment, while the latter is admittedly a subject for scientific investigation. The hint to this effect that is given by the unlike constitution of geographical and geological societies, the world over, ought not to pass unnoticed. Membership in many geological societies is limited to experts; if membership in a single geographical society is similarly restricted, I have yet to learn of it.

Let us then build on the progress we have made; let us realize that only when ontography is treated as thoroughly as physiography will geographical work gain the best geographical flavor. So empirical has been the traditional geographical treatment of the organic elements, so imperfectly have the organic elements been generally recognized as balancing the inorganic elements in the make-up of the subject as a whole, that no name has come into use for the organic half of geography corresponding to physiography for the inorganic half; and it is to supply this lack that I have elsewhere suggested the name above used. I believe that the adoption of some such name would aid in the systematic cultivation and in the symmetrical development of geography, and thus of geology also as a whole, by bringing more prominently forward the necessity of giving — or at least attempting to give — as scientific a treatment to the inhabitants of a region in their geographic relations as to the region itself.

The adoption of some such term as ontography would tend to correct the false idea that geography is concerned only with the elementary and manifest examples of organic responses; it would promote thoroughness of study, and thus more fully continue the progress that we have thus far made. The adoption of the term would moreover emphasize the principle of continuity through time, of the geographical stratification of geology, which is of so great importance in the scientific development of our subject: for ontography, in which persistent physiographic influences make themselves felt through inheritance, is then seen to be only the modern member of a great series with whose earlier members we have long been familiar in paleontology. The recognition of the continuity, the essential unity, of these two subjects — one dealing with the living forms of to-day, the other with the dead forms of the past — dignifies the first and vivifies the second; and adds yet another argument in favor of an objective rather than a subjective classification of the sciences of the earth. The beginning of the cultivation of ontography, already made more or less consciously, strongly suggests a larger development for the future. We are thus assured that as the details of organic responses are worked out, and the importance of physiographic details is recognized, the difference between physiography as the study of environment, and geochemistry and geophysics as the study of the earth for itself, will diminish. To-day no one can say how far the details of these semi-independent sciences may not be found essential in physiography.

Let me now amuse you for a moment with a scheme of terminology that might have a little value if some of its terms were not already appropriated in other meanings. The scheme does not represent the historical development of earth-science, but sets forth its several parts in the relations that our progress up to date shows them to stand.

Suppose we should use the ending, *ology*, to denote the conception of sequence in time, and *ography* to denote the conception of temporary distribution. We should then have our whole subject, geology, in which time-sequence is the dominant idea, made up, like an endless prism of mica, of an indefinite number of momentary sheets of geography that cleave across the time-axis. Biography would then lose its limitation to man, and become the study of temporary floras and faunas in successive geographies; while biology would give up its usual meaning and become the study of life in the developmental sequence of organic evolution through geological time. The study of the minerals and rocks of any epoch would be minerography and petrography, while mineralogy and petrology would treat problems of paragenesis and metamorphism, in which the passage of time is essential; and for one, I should then be able to remember what petrography and petrology mean. So we might go on with physiology, meteorology, and oceanology, as made up of a succession of physiographies, meteorographies, and oceanographies, and we should have glaciology and climatology made up of glaciography and climatography; and ontology, or the sequence of organic responses to the changing earth, would be made up of a succession of ontographies.

Schemes of terminology, however, are not often successfully made to order in this fashion; they are slowly evolved without much regard to system, as is seen in the haphazard nomenclature of oceans, seas, gulfs, and bays. Minerography is strange to the point of offense to the ear; we cannot take over biography and physiology from their present uses; we must get along with such terms as we have, and with such new ones as are added from time to time. My only object in suggesting this fanciful scheme is to bring more clearly forward the space and time relations that are recognizable in all branches of our subject, as well as in geography and geology. The progress of the last century has certainly brought us now to a stage when these general relationships may be in good part understood, if we give heed to them. We fail to take the best advantage of our progress, if we see only the specialized development of our several sub-sciences.

It has often seemed to me as if petrologists were rather overwhelmed at present with the flood of new facts that modern methods of research have let loose upon them; yet how greatly is the study of both mineralogy and petrography broadened by the addition of the continuous to the momentary consideration of minerals and rocks that the flood has swept before us; for even the rocks have their phases of youth and age. So brief is our life, that geomorphologists are even to-day hardly accustomed to the systematic mobilization of landforms; yet the description of the lands is greatly strengthened when their forms are seen to be fixed only in the sense that an express train seems to be fixed before the instantaneous wink of the camera's eye. The ontographer may be bewildered when he realizes what the evolutionary struggle for existence means to the individual; and when he thinks how long the world was the scene of relentless strife before pity was born, and how young and impotent pity is still, we may well wonder whether we have yet learned much of omnipotence. Yet how superb is the conception of the procession of life, never halting in its march through the corridors of time. These are great acquisitions by which our science has enriched human thought, and it behooves us to occupy as often as possible the point of view to which they carry us.

It has not appeared desirable to give place in this address to special problems, because they will receive due attention in the addresses that are to follow under the eight headings allotted to our Department. And besides, it would be impossible even in the whole of an address to do justice to the great body of work that includes not only the establishment of the great age of the earth, and the continuity of ordinary orderly process, inorganic and organic, but a flood of lesser results: the penetration of all the lands, except those of the polar caps, the sounding of the oceans, the refined analysis of the atmosphere, the optical study of rocks, the discovery of glacial epochs in the past, the measurement of tremors that have passed through the body of the earth, and countless others.

I have therefore sought to consider only the prospect from the point of view to which the progress of a hundred years has led us. Vast as is the expanse over which we look, innumerable as are the elements of the view, the chief impression that we gain is one of well-ordered interaction in the continuous progress of events, all of whose momentary geographic phases — with all their parts of earth, air, water, and responding life — are spread upon successive pages in the great volume of geological records.

SECTION A — GEOPHYSICS

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(*Hall* 14, *September* 21, 10 *a. m.*)

CHAIRMAN: PROFESSOR CHRISTOPHER W. HALL, University of Minnesota.
SPEAKERS: DR. GEORGE F. BECKER, Geologist, U. S. Geological Survey.
SECRETARY: PROFESSOR E. M. LEHNERTS, Winona Normal School, Minn.

THE Chairman of the Section of Geophysics was Professor Christopher W. Hall, of the University of Minnesota, who stated, in presenting the speakers:

"Scientific men have hitherto followed their several lines of research with such success that vantage-ground is secured from which to take a survey of broader fields, not only within their own especial department of research, but into neighboring grounds. Indeed, they are discovering by this survey that what had appeared a wall of obstruction on this side, and a line of demarkation on the other, has been an illusion. As they approach for closer scrutiny, neither wall nor line can be found. Their own field is broader than they supposed; they can travel on and on without discovering the first obstruction; they find themselves within the vast field of facts and phenomena without let or hindrance, save in the limitations of their own powers. They find grouped around themselves still others who have entered the field from other directions and, under similar conditions, attracted by the same spirit of inquiry and led on by successes in research, have set their faces toward a new future of promising discovery.

" Gathered to-day from different parts of the world, some of us geologists and others physicists, we stimulate each other in a common zeal and aid each other in a common search for the gems of truth which this common ground shall reveal. We are to be told what has already been done and what are some of the problems of the immediate future. This field is a most promising one: were I to act the seer I should tell you that nowhere else within reach of human genius and industry is there greater promise of return; out of the field of geophysics are to come rewards of toil that shall give mankind a clearer view into fundamental causes, and a firmer grasp upon its natural environment, than elsewhere in the broad field of intellectual accomplishment can be had. Stirring suggestions as to the origin of the world and the physical activities springing from that creation are already nerving investigators to action."

PRESENT PROBLEMS OF GEOPHYSICS

BY GEORGE FERDINAND BECKER

[**George Ferdinand Becker,** U. S. Geologist in charge of Division of Chemical and Physical Researches, Geophysicist of Carnegie Institution. **b.** January 5, 1847, New York City. B.A. Harvard, 1868; Ph.D.Heidelberg, 1869; Mining Engineer, Berlin, 1871. Instructor in Mining and Metallurgy, University of California, 1875–79; Special Agent Tenth Census, 1879–83. Member of National Academy; Geological Society of America; Washington Academy. **Author of** numerous books and articles on geology.]

ADVANCES in science are seldom made without a view to the solution of specific, concrete problems, even when the results of investigation possess the widest generality. The history of science is full of instances of the fruitfulness of researches the immediate purposes of which were narrowly defined. Geophysics is only that portion of general physics, including under that term physical chemistry, which is applicable to the elucidation of the past history and present condition of the earth. It is thus a very definite branch of applied science, the exigencies of which call for the solution of a group of related problems. These, however, possess great interest apart from their application to the globe, while for the most part they offer very serious experimental and theoretical difficulties. Had they been easy, they might have been solved long ago, for many of these problems have been propounded and more or less discussed from the birth of modern science to the present day. Their difficulty, not lack of recognition of their importance, has postponed their solution.

The main purpose of this paper is to deal with the order in which it would be expedient to investigate the questions embraced under the head of geophysics, but a brief and incomplete enumeration of the problems from a geological standpoint will serve to lend a coherency and a human interest to the subject which it would otherwise lack.

Physical geology begins with the solar nebula and the genesis of the earth-moon system. The harmonies of the solar system compelled the immortal Kant and the ever-living Laplace to seek the origin of the planets, the sun, and the other stars in heterogeneous nebulas which they supposed to have condensed about one or several nuclei. Every attempt to devise an essentially different hypothesis has failed, and every history of the globe which begins after the birth of the planet is unsatisfying. In the drama of the universe there must have been pre-nebular scenes, but of these we have as yet no inkling. The nebular hypothesis, as its authors propounded it, explains the similarity in the composition of the members of the solar system which is indicated by the analysis of meteorites and by the spectroscope, though the facts thus revealed were unknown to Kant and Laplace. It is also compatible with and accounts for the heterogeneity in the composition of the earth manifested in the actual asymmetric distribution of oceans, mountain ranges, and anomalies of gravitational force, as well as in the curiously local occurrence of certain ores (such as those of tin and mercury) and in the predominance of certain alkalies among the rocks over wide areas.

This heterogeneity, however, is of a small order of magnitude. The general dependence of gravity on latitude, the nearly spheroidal shape of the earth, and other phenomena show that the distribution of density is nearly symmetrical, while the divergence of the spheroid from the figure characteristic of a fluid of the same mean density and mass as the earth demonstrates that the interior layers of equal density are oblate. These and similar facts are consistent with and are strong evidence for the hypothesis that the globe has been fused at least to a considerable depth from the growing surface of the gathering nebulous mass. Nevertheless, Houghton, and more recently Professor Chamberlin, have supposed that the accretion of nebulous matter was so slow that the heat of impact did not suffice to produce fusion. The hypothesis of superficial fusion is not incompatible with the minor heterogeneity pointed out above; for the laws of diffusion in viscous fluids give proof that sensibly perfect homogeneity could not be produced even in 50,000,000 years throughout a body of liquid originally heterogeneous and possessing a tenth of the mass of the earth. On the other hand, there is no known ground other than mere convenience for supposing an original homogeneity either of the nebula or of the earth.

The problem of the distribution of density in the earth is one of the most important in all geophysics. It is as significant for geodesy and terrestrial magnetism as for geology. That Laplace's empirical law represents it approximately is generally acknowledged, but it appears substantially certain that this is merely an approximation without theoretical value. Only extended researches, however, can replace it by one better founded.

The solidity of the earth is now very generally accepted, though Descartes's hypothesis of its fluidity, invented to satisfy his erroneous theory of vortices, died hard. Lord Kelvin showed from tidal phenomena that the effective rigidity of the earth is about that of a continuous globe of steel. Professor Newcomb pointed out that the Chandlerian nutation leads to the same conclusion and an almost identical value of the modulus of rigidity, and Professor George H. Darwin demonstrated that, if the earth is a viscous liquid, its viscosity must be some 20,000 times as great as that of hard brittle pitch near the freezing-point of water. From the point of view of modern physical chemistry, and in consideration of Professor Arrhenius's opinions, the matter requires further consideration. In particular it is most important to know whether the earth is substantially a crystalline solid or an amorphous substance, for many modern physical chemists consider amorphous matter as liquid. This opinion is far from being established, however, and recent experiments by Mr. Spring show that mere deformation at ordinary temperatures, attended by only a very small absorption of energy, suffices to convert crystalline metals into substances exhibiting characteristics of amorphous bodies. Since Nordenskiöld's great discovery of large masses of terrestrial iron, or rather nickel steel, in Greenland, and the wide distribution since proved for similar metal imbedded in igneous rocks, a great amount of evidence has accumulated that a large part of the earth is composed of material indistinguishable from that of metallic meteorites. Meteoric iron is of course a highly crystalline material.

It is a very striking fact that the mean rigidity of the earth is about that of steel, for the only substance likely to occur in extensive continuous masses and displaying such rigidity at ordinary temperatures and pressures is steel itself. Nevertheless, the conclusion cannot yet be drawn from the resistance to deformation displayed by the earth, that it is chiefly composed of steel. Elastic resistance is known to be a function both of pressure and of temperature, and until this function has been determined by theory and experiment, the bearing of the evaluation of rigidity by tidal action cannot be ascertained.

Having shown the earth to be a solid globe, Lord Kelvin calculated its age from one of Fourier's theorems, assuming for purposes of computation an initial temperature of 7000° F. (nearly 3900° C.) and that the thermal diffusivity of the earth is that of average rock. These assumptions, with the observation that the temperature near the surface of the earth increases at the rate of 1° F. for every 50 feet of depth, lead to an age of 98,000,000 years; but on account of the uncertainty as to conductivities and specific heats in the interior, the conclusion drawn by Lord Kelvin was only that the time elapsed since the inception of cooling is between 20 and 400 million years.

Clarence King subsequently took a further important step on the basis of data determined at his request by Professor Carl Barus on the volume changes which take place in diabase during congelation, and on the effects of pressure in modifying the melting and solidifying points. Assuming that the earth can never have had a crust floating on a liquid layer of inferior density, computation leads him to 24 million years as the maximum period for the time since superficial consolidation was effected, provided that the superficial temperature gradient and conductivity are correctly determined.

These researches, together with Helmholtz's investigation on the age of the solar system, which is incomplete for lack of knowledge of the distribution of density in the sun, have had a restraining influence on the estimates drawn from sedimentation by geologists. Many and perhaps most geologists now regard something less than 100 million years as sufficient for the development of geological phenomena. Yet the subject cannot be regarded as settled until our knowledge of conductivities is more complete. An iron nucleus, for example, would imply greater conductivity of the interior and a higher age for the earth than that computed by King, though probably well within the range explicitly allowed by Lord Kelvin in view of the uncertainty of this datum.

The researches of Kelvin and Darwin, supplementing those of Kant and others, have left no doubt that the moon was formerly closer to the earth than it now is, and that the rotation of the latter was more rapid, involving a greater ellipticity of the meridian than it now shows. In a fluid or Cartesian earth the change of figure might have produced little effect on the structure of the planet. If the earth is chiefly a mass of crystalline nickel steel, it is very possible that such a change in the figure of equilibrium might rupture it. Since the epoch at which the earth rotated in 5 hours 30 minutes, the polar axis must have elongated by several per cent, most of it before the time of rotation was reduced to 11 hours.[1] Were the earth chiefly composed of forged steel, such elongation might be produced by plastic deformation; but meteoric iron is rather comparable with cast-iron, or better still, with relatively brittle, unforged cement steel, and might break.

Now it is an indubitable fact that a majority of the outlines of the great oceanic basins and of the chief tectonic lines of the globe, lie nearly on great circles tangent to the Arctic Ocean and to the Antarctic continent.[2] These lines, or most of them, are of extremely high geological age, their main features having found expression as early as the oldest known fossils and in some cases still earlier. It appears to me very possible that these fundamental ruptures of the globe were due to the change of figure attendant upon diminution of the earth's period of rotation. Their symmetrical disposition with reference to the polar axis is unquestionable, as well as the fact that they penetrate to great depths. They must be due to some tremendous force acting axially, which actually altered the ellipticity of the meridian, since these fissures could not have been formed without modifying the shape of the globe, and the only known disturbance of this description is the change of figure referred to. On the other hand, were the earth homogeneous, such ruptures would be expected to have as envelopes small circles in latitude 45° instead of at about latitude 70°. But since the earth is not homogeneous, this discordance does not invalidate the suggestion.

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[1] Compare Thompson and Tait, *Nat. Phil.*, § 772, where the rotational period and eccentricity are given for a fluid of the mass of the earth and possessing its mean density. When the period is 5h. 30m., this table gives the data for computing that the polar axis has a length equal to 0.95 of the length which it has when the period is a sidereal day. For rotation in 10h. 57m. the polar axis is 0.99 times that for a day.

[2] In 1857 Professor R. Owen, of Tennessee, and, independently, Benjamin Peirce, called attention to the tangency of the coast-lines to the polar circles (not to the coast-lines of the arctic sea and the antarctic continent), each attributing the facts to the influence of the sun. In the first *Yearbook* of the Carnegie Institution I failed to refer to these publications.

Be this as it may, upheavals, subsidences, and attendant contractions have been in progress throughout the whole of historical geology, or the period within which fossils afford a guide to the succession of strata. The so-called contractional theory has shown itself wholly inadequate to account for the amount of deformation traceable in the rocks of the globe, nor has the extravasation of igneous rock been sufficient to account for the phenomena. To me the earth appears to be a somewhat imperfect heat-engine in which the escape of thermal energy is attended by the conversion of a part of the supply into the vast amount of molar energy manifested in the upthrust and crumpling of continents. The subject will probably turn out to be accessible mathematically after certain experimental determinations have been made, and I shall return to it presently.

Orogeny, or mountain-building, is a mere detail of the more general subject of upheaval and subsidence, but it exhibits problems of great complexity both from the experimental and from the theoretical points of view. There is no question that unit-strains are often reached or even surpassed in contorted strata and in belts of slate, but the theories of elasticity and plasticity as yet developed are inadequate to deal with these strains in complex cases. An investigation on finite elastic and plastic strain is now under way in my laboratory and has made gratifying progress thus far; but this is not the place for detailed results. Something also has been done in the way of working out homogeneous finite strains in rocks, so that the general nature of joints, faults, and systems of fissures, and the mechanism of faulting is now fairly clear. The theory of slaty cleavage is a subject of dispute in which I have taken part. Few colleagues appear to agree with me that this cleavage is due to weakening of cohesion on planes of maximum slide, but I am not hopeless that my view will make its way to favor in time.

Seismology is a vast subject by itself, but one almost totally lacking in theoretical foundation. Seismological observations should afford the means of exploring the elastic properties of the earth throughout its interior, but the theory of the vibrations of a spheroid like the earth is not yet worked out. Meantime observations are being accumulated, but it can be foreseen that these will contribute little to elucidation until they include the vertical components of the vibrations as well as the horizontal ones. In other words, we must know the angle at which the wave emerges from the surface, as well as its azimuth.

The causes and conditions of earthquakes afford a separate topic of great interest. That some of them are of volcanic origin is evident; others appear to be due to paroxysmal faulting, yet there is very possibly a common underlying cause.

On no subject are opinions more divergent than concerning the origin and mechanism of volcanoes. To the ancients they were the mouths of the river Phlegethon. To those who adhere to the Cartesian doctrine they are communications with the liquid interior of the earth. Most geologists think of them as connected with hypogeal reservoirs of melted matter subsisting for indefinitely long periods of time. Finally, it is conceivable that the lava may be extruded as soon as the melted mass has accumulated in sufficient quantity, somewhat as water may break through an obstructing dam after its depth reaches a certain value. The continual movements of the rocks show that they must be to some extent in a state of elastic strain, so that a given cubic mile of rock resists surrounding pressure in virtue both of its rigidity and of its compressibility. If that cubic mile becomes liquid, its rigidity is gone and the change of shape of surrounding masses may aid in its expulsion. Of course imprisoned gases, especially the "juvenile waters" of Professor Suess, may also play a very important part in expulsion. But the more I have studied the matter, the less probable it seems to me that considerable bodies of melted lava can remain quiet for long periods of time in the depths of the earth. The influences tending to their expulsion would seem to be at a maximum immediately after the fusion of enough material to supply an eruption.

Relief of pressure is often invoked to explain fusion of lava, but it is not a wholly satisfactory cause. If a deep crack were to form, the rock at the bottom might melt indeed, but, as the crack filled, the pressure and the solidity of the source would be restored. To me, Mallet's hypothesis is more satisfactory, so far as the explanation of fusion is concerned. Only those who have studied the minute evidence of mechanical action in mountain ranges can appreciate the evidence they present of stupendous dissipation of energy. This has not indeed been enough to fuse the rocks, but it is hard to conceive that it is always insufficient to furnish the latent heat of fusion to rocks already close to their melting-point under the prevailing pressure. From this point of view, volcanism is a feature of orogenic movement, and it is to be looked for where relative motions are concentrated in zones so narrow that the local dissipation of energy is relatively intense. It is also possible that percolating waters, by reducing the melting-points of rocks, sometimes bring about fusion without change of temperature. Such an hypothesis might fit the volcanoes of the Hawaiian Islands, where there is no known faulting in progress.

The physics of magmatic solutions is a great subject which is experimentally almost untouched, although a vast amount of geological speculation has been based upon assumed properties of magmas. It is only within a few months that even satisfactory melting-point determinations of those most important rock-forming minerals, the lime-soda feldspars, have been made. The feldspars are only one series of isomorphous mineral mixtures. Their study is fundamental and must be followed by that of the remaining class, *i. e.*, the eutectics. These, in my opinion, will lead to a rational classification of igneous rocks, themselves mixtures and incapable of logical description except in terms of standard mixtures, the eutectics.

It appears to me highly probable, for many reasons, that the magmas of the granular rocks are not liquids but stiff emulsions, comparable with modeling-clay, the solid constituents (perhaps free oxides) being merely moistened with magmatic liquids. Such masses behave mechanically like soft solids; they display some rigidity and in them diffusion is reduced to a vanishing quantity. They may be ruptured and the (aplitic or pegmatitic) liquid portion may then seep into the cracks. Such a magma might be forced into minute fissures, as is the case when clay is moulded to terra-cotta articles, and yet it would support permanently, on its upper surface, rocks of superior density. Only in such a magma can I comprehend the simultaneous growth of crystals of various minerals; for in a liquid not exactly eutectic, the formation of crystals must follow a definite order. Again, if banded gneisses and gabbros had been fluid, the bands would show evidences of diffusion which as a rule are absent or barely traceable in these rocks.

The relations between consanguineous massive rocks have occupied a large part of the attention of geologists for many years. At one time it was supposed that homogeneous liquid magmas might split up into two or more homogeneous magmas by processes of molecular flow due to differences of osmotic pressure. This process was called the differentiation of magmas. It has been shown, however, that these processes are so much slower even than heat-diffusion, that they cannot be efficient beyond distances of a few centimeters. For this reason Mr. Teall,[1] who first suggested the application of the Soret process to account for differentiation, Professor Brögger,[2] and others, have abandoned the hypothesis of differentiation on a considerable scale by molecular flow. Nevertheless, observations on laccolites and other occurrences leave no doubt that a single magma may solidify to different though consanguineous rocks. If the separation is not molecular, it is self-evident that it must be molar. The only molar currents readily conceivable in a body of magma are convection currents, and these, or even an equivalent mechanical stirring, would necessarily lead to fractional crystallization, a familiar process known even to the pupils of Aristotle, and which is almost unavoidable when mixed solutions solidify. This process is one of precipitation, and is absolutely distinct from the differentiation (or, more properly, segregation) of rock magmas, in which a single liquid is supposed to separate into two or more distinct liquids. The general conditions of the order of precipitation during fractional crystallization in accordance with the phase rule are by no means beyond the reach of discussion, and the able investigations of Messrs. J. H. L. Vogt and J. Morozewizc have a direct bearing on this subject.

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[1] *Proceedings, Geological Society*, London, vol. 57, 1901, p. lxxxv.

[2] *Eruptivgestiene der Kristianiagebietes*, part III, p. 339

A mystery which will assume greater importance as the accessible supply of coal diminishes is the origin of petroleum. There is much to be said in favor of the unpopular hypothesis of Mendeleef, supported by experiments on cast-iron, that liquid hydrocarbons are due to the decomposition of the iron carbides of the terrestrial nucleus. Such vast accumulations of oil as exist on the Caspian and in the Caucasus seem incompatible with the hypothesis of animal or vegetable origin, although oils belonging to the same series as do the petroleums have been produced in the laboratory from organic materials. On the other hand, some meteorites contain hydrocarbons (which may themselves be due to the alteration of iron carbides), and there are geologists who infer that the petroleum may be derived from the mass of the earth itself.[1] If the origin of the oil is not animal or vegetable, the supply is very likely inexhaustible. More extended study of the connection between volcanic phenomena and the origin of asphaltic and other hydrocarbons is a desideratum.

Ore-deposits themselves form the branch of geology which was earliest cultivated and which will never lose its interest so long as mankind remains gainful. Yet much remains to be done by experiment for the theory and practice of mining-geology. The mechanism of the secondary enrichment of ores, particularly those of copper, detected by Mr. S. F. Emmons and enlarged upon by Mr. W. H. Weed, is being studied experimentally in the laboratories of the U. S. Geological Survey. A feature deserving careful experimental study is the osmotic separation of ores from their solutions by the wall-rock. Many minutiæ of occurrence suggested that the walls of veins often act as a species of diaphragm or molecular filter and have a dialytic action on the ore solutions.[2] The origin of the ores themselves is still very obscure and will hardly be elucidated until more is known of the earth's interior. Sometimes they seem to be derived from adjacent rocks; in other cases conditions suggest that the rocks and the veins derive their metallic content from a common deep-seated source. Here, as in several other connections, Professor Suess's theory of "juvenile waters" is very suggestive. It is generally held that many of the great iron deposits are due to magmatic separation. Deposition of lead ores by replacement of calcite is a known process, but takes place under unknown conditions. In some cases replacement of rock by ores appears to me to be alleged without sufficient proof. Pseudomorphoss is the only adequate test of replacement.

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[1] See H. L. R. Fairchild, *Bulletin of the Geological Society of America*, vol. xv, 1904, p. 253.

[2] *Mineral Resources of the U. S. for* 1892, p. 156.

Erosion appears to be a subject which is capable of more exact treatment than it has received. Weathering and abrasion proceed with a rapidity which increases with the surface exposed per unit of volume.[1] Hence these processes lead to minimum surfaces. Therefore also the mathematics of erosion is essentially identical with that of capillarity.

Geological climates are as interesting to astrophysicists as to meteorologists and geophysicists. Messrs. Langley and Abbot appear to have evidences of recent variations in solar emanation. If these have been considerable in the course of the period of historical geology, light should be thrown upon them by the paleontology of the tropics. Variations in the composition of the atmosphere must have been very influential in determining both the mean temperature of the earth's surface and the distribution of temperature; but so also is the distribution of water. No theory of the glacial period seems generally accepted. Croll's theory is discredited. I have shown to my own satisfaction that the astronomical conditions most favorable to glaciation are high obliquity and low eccentricity of the earth's orbit,[2] but cannot claim any extensive following. If I am right, it should be possible to obtain a definite measure of geological time in years as soon as the astronomers have completed the theory of secular variations in the planetary system so far as to be able to assign the lapse of time between successive recurrences of low eccentricity and high obliquity.

A most interesting observation, which promises much light on the past history of the globe, is that lavas and strata indurated by lavas retain the polarity characteristic of the locality in which they cooled.[3] The time may come when this will lead to determinations of the relative age of lavas, the duration of periods of eruption, and possibly even absolute determinations of date.

Geology has long, and with some justice, labored under the reproach of inexactitude. As has been illustrated in the preceding pages, the science is still in the qualitative stage, and almost wholly lacks the precision of astronomy. Even its most ardent students have seldom succeeded in ascertaining the quantitative relations between effects and operative causes, and have been perforce content to indicate tendencies. Thus geological doctrine is far too much a matter of opinion, but this is hardly the fault of the areal geologist. The country must be mapped both for economic reasons and to accumulate a knowledge of the facts to be explained. Working hypotheses the field geologist must have, or he could not prepare his map; and he is only responsible for living up to the standard of knowledge of his time. He is continually face to face with phenomena for which physics and chemistry should account, though they have not yet done so, and must accept seeming probabilities where certainty is unattainable. So, too, Kepler's predecessors recorded facts and guessed at generalizations as best they might.

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[1] *U. S. Geological Survey*, mon. XIII, 1888, p. 68.

[2] *American Journal of Science*, vol. XLVIII 1894, p. 95.

[3] Brunhes and David, *Comptes Rendus*, vol. 133, 1901, p. 153.

The physics of extreme conditions still awaits satisfactory exploration. The geologist turns to the physicist for help, and in most cases meets with the reply: We cannot tell. Astrophysics is in much the same situation. Astronomers know as little of the distribution of density in the stars or planets as do geologists. Real knowledge of the physics and chemistry of high temperatures would be as welcome to them as to us. After all, physical geology is the astrophysics of this, the only accessible planet. Geodesy, too, and terrestrial magnetism are waiting for the solution of geophysical problems. How much might be done, Lord Kelvin and Mr. George H. Darwin have shown; but there are many problems too broad and too laborious to be solved by individual effort, and these are as essential to the rounding-out of the science of physics as they are to the development of geology and astrophysics.

In the brief review which precedes, I have endeavored to show that the history of the earth bristles with problems, few of them completely solved, though in many cases we have some inkling of the solution. This sketch has been drawn for the purpose of considering the strategy of a campaign against the series of well-intrenched positions occupied by our great enemy, the unknown.

Generalizing the results of the sketch presented, it is easy to see that nearly all the problems suggested involve investigation of the properties of solids, or of liquids, or of the transition from one phase to the other. It is the business of the experimental physicist to establish linear relations; it is the occupation of the mathematical physicist to draw logical inferences from these relations. Each will have plenty to do in a methodical study of geophysics.

There can be no doubt that the character of the earth's interior and the physical laws which there prevail constitute the most fundamental object of geological and geophysical research, while the results of successful investigation would be immediately applicable at least to the moon and Mars. No one questions that enormous pressures and very high temperatures exist near the earth's centre, while the quality of matter which constitutes the interior cannot be satisfactorily determined until we know how substances would behave under extreme pressures and at temperatures approaching 2000° C. There is every reason to suppose that under purely cubical compression, dense, undeformed solids are perfectly elastic. Hence the basal problem of geophysics is to find the law of elastic compressibility. This cannot be accomplished by direct means, but the task is, nevertheless, as pointed out above, not a hopeless one, and has been taken in hand. Should success be achieved, researches will follow on the variation of elasticity with temperature. This feature of the investigation will present very great experimental and theoretical difficulties, but there is no good reason to despair of success.

When the law of resistance of solid bodies becomes known as a function of both temperature and pressure, even for isotropic substances with only two moduluses of elasticity, the way will be opened to various important investigations, largely mathematical in character. It is true that thoroughly isotropic bodies are seldom met with, yet geological masses must, nevertheless, often approach closely to this ideal. Many of the most important rocks are chiefly composed of triclinic feldspars, which, indeed, occur about as abundantly as all other minerals found at the surface of the earth put together. A triclinic feldspar crystal rejoices in the full possible number of elastic moduluses, 21. Yet a large spherical mass of small, fortuitously oriented feldspars will behave to external forces of given intensity and direction in the same way no matter how the sphere may be turned about its centre, and it will, therefore, act as an isotropic body. This fact is enough to show that an infinite variety of intimate molecular structures are compatible with molar isotropy.

Thus a knowledge of isotropic elasticity will suffice as a basis for testing reasonable hypotheses of the constitution of the earth's interior, taking into account its known rigidity and density. Still greater light can be thrown on this subject by including in the investigation the moon and Mars; for their masses and dimensions are known, and there seems every probability that they are composed of the same materials as the earth, though in different proportions. If a given hypothesis as to the chief constituents satisfies the known conditions of all three planets, it will doubtless find acceptance. Such a result would open the way to fresh advances in geodesy and terrestrial magnetism, and cast backward through the vista of time a ray of light on the nebular hypothesis.

Again, when the law of elasticity and the approximate constitution of the globe are known, it will be possible to work out a satisfactory theory of the simpler modes of vibration in a terrestrial sphere, and then seismological observations can be applied to determining more precisely the intrinsic elastic moduluses of the earth along the paths of earthquake-waves.

It will also be practicable to examine critically the possible rupture of the globe as a consequence of change of figure and to study intelligently the simpler cases of the crumpling of strata, fissuring, and other problems in the mechanics of orogeny.

The science of elasticity has had a very disappointing history. Simple as is the assumption *ut tensio, sic vis*, the attempt to solve even such seemingly elementary problems as the flexure of a uniformly loaded rectangular bar leads to insoluble equations; so that the science has been relatively unfruitful. It remains to be seen whether a truer relation between load and strain will not simplify formulas and increase the applicability of algebra to concrete cases.

From an astrophysical point of view the dialytic action of mineral septa is unimportant, but it is very interesting in its bearing on metamorphism and ore deposition, and may readily contribute to economic technology.

The relations of viscosity to the diffusion of matter have not yet been elucidated, even for ordinary temperatures. This subject is one of much importance in connection with the genesis of rock species, and of course it should be studied at 10° before undertaking researches at 1000°.

High temperature work is essential even to the investigation of the elastic problem, and it is almost a virgin field. Even thermometry is very imperfect above the melting-point of gold, though it is destined soon to become exact at least as high as 2000°, a range which will probably suffice for geophysics. But we are also in almost total ignorance of the extent to which the laws of physics, studied at ordinary temperatures, prevail at 1000 or 2000 degrees. One of the less difficult problems of this group is that of thermal conductivity and specific heat of solid bodies at high temperatures. For the principal metals this is already known as far as 100°, but not for rocks or minerals. It would be especially desirable to have such determinations for granite, basalt, and andesite, the last representing the average composition of the accessible part of the lithosphere.

It seems to me that when the thermal diffusivities are known for these rocks, over a range of a thousand degrees, the question of upheaval and subsidence can be attacked with a good prospect of success. A cooling sphere is conceivable in which the distribution of thermal diffusivity is such that the flow of heat would be "steady," in Fourier's sense, and thus accompanied by no superficial deformation. With any other distribution of diffusivities, deformation would occur, and the globe would act as an imperfect heat-engine, the work done being that of upheaval or subsidence. Now when the assuredly variable value of diffusivity for the materials of the globe is known, the mathematical conditions for steady flow can be worked out, and if these are not consistent with the facts of the globe, a *vera causa* for upheaval will have been found, which may lead to further and more detailed conclusions. It should also either elucidate or simplify the subject of the fusion of magmas and their eruptive expulsion.

The data for constitution and thermal diffusivity will readily be applicable to the problem of the earth's age and will yield a corrected value of the probable lapse of time since the initiation of the *consistentior status* of the Protogæa.

The most difficult field in geophysics is the study of solutions at high temperatures. This is largely because both methods and apparatus require to be invented. When work of this kind was undertaken in the laboratory of the Geological Survey, three years since, no furnace existed in which pure anorthite could be melted and a trustworthy determination of the temperature of fusion made. For the study of aqueo-igneous fusion, which must, of course, be performed at considerable pressures, extremely elaborate preparation is necessary; indeed, all attempts hitherto made in this direction have been only very partially successful.

Were it not that the number of important rock-forming minerals is small, the study of igneous solutions for geophysical purposes would be an almost hopeless task. The feldspars, the pyroxenes, the amphiboles, and the micas appear to form isomorphous series, and must be studied as such. They, with quartz, make up nearly 93 per cent of the igneous rocks, nepheline, olivine, leucite, apatite, magnetite, and titanium minerals substantially completing the list which enter into these rocks in sensible proportions. After the melting-points of the minerals have been determined and their isomorphism has been studied, the most important research to be undertaken is that on their eutectic mixtures. Other features, however, must receive attention, such as their latent heat, ionization, viscosity, and diffusivity. Immensely interesting will be the study of melts into which hydroxyl enters as a component and which may turn out to be emulsions rather than solutions. Such researches will constitute a most substantial addition to physical science, and, as pointed out above, offer a good prospect for the rational classification of rocks.

Enough has been said to show how closely geophysical researches interlock. Researches at high temperatures must accompany investigations at common temperatures, physics must be supplemented by physical chemistry, mathematical ability of the highest order must be called upon at every step to elucidate difficulties and to draw inferences capable of being again submitted to inquiry, and some geological knowledge, too, is requisite to appreciate the bearing of results and to indicate the questions of importance. No human being has the length of days, the strength, the skill, or the knowledge needful to undertake, without help, the investigation of geophysics as a whole. Only a few of the topics touched upon in the earlier pages of this essay are independent of coöperation; for instance, the astronomical conditions favorable to glaciation, and perhaps the application of the mathematics of capillarity to the problem of erosion. On the other hand, the list of geophysical problems requiring coöperation could be almost indefinitely extended even now, and will be supplemented when the most pressing questions approach their answers.

Organization increases efficiency in scientific work as much as in technical pursuits, though it has seldom been attempted. Instances in point are the U. S. Geological Survey, the Reichsanstalt and astronomical surveys of the sky. Geophysics, then, is too difficult a subject to be dealt with excepting by a well-organized staff, working on a definite plan resembling that indicated above. The tastes and convenience of individuals must give way to the methodical advancement of knowledge along such lines that the work of each investigator shall be of the utmost assistance to the progress of the rest.

Work in geophysics is already in progress in this country, thanks to the appreciative sympathy of Director Walcott of the Geological Survey, and the liberality of the Carnegie Institution, by members of my staff and in part under my direction. Messrs. A. L. Day and E. T. Allen have made an excellent series of determinations of the melting-points of the triclinic feldspars and studied their other thermal properties. They are now preparing to make experiments in aqueo-igneous fusion. Mr. C. E. Van Orstrand has made a novel application of the theory of functions to elastic problems, and has reduced several series of important observations on elastic strains for comparison with theory. Dr. J. R. Benton is occupied in experimental investigation of elastic strains in various substances. The men engaged in these researches are able and devoted to their work, but they are too few in number, and they are required to make determinations of the most delicate character in an office-building standing in the busiest portion of Washington, where the walls are in a state of incessant tremor, and where there is no suggestion of uniformity of temperature. Under such circumstances the results of observation cannot be of the most refined character, and must be obtained at great expense of time and effort.

Most of the great physicists of the world have expressed their interest in geophysics and their belief that the time is ripe for its investigation. Geologists are eager for its results, but no government can undertake investigations so remote from industry as this. I do not think I can more fitly conclude this paper than by quoting a resolution introduced by Mr. S. F. Emmons at Vienna a year ago. It was passed by acclamation by the Geological Congress, after a ringing speech by Professor Suess, and it expresses my own views most accurately.

Emmons's Resolution

"It is a well-known fact that many of the fundamental problems of geology, for example those concerning uplift and subsidence, mountain-making, vulcanology, the deformation and metamorphism of rocks and the genesis of ore-deposits, cannot be discussed satisfactorily because of the insufficiency of chemical and physical investigations directed to their solution. Thus, the theory of large strains, either in wholly elastic or in plastic bodies, has never been elucidated; while both chemistry and physics at temperatures above a red heat are almost virgin fields.

"Not only geology, but pure physics, chemistry, and astronomy, would greatly benefit by successful researches in these directions. Such researches, however, are of extreme difficulty. They would require great and long-sustained expenditure, as well as the organized coöperation of a corps of investigators. No existing university seems to be in a position to prosecute such researches on an adequate scale.

"It is, therefore, in the judgment of the Council of the Congrès Géologique International, a matter of the utmost importance to the entire scientific world that some institution should found a well-equipped geophysical laboratory for the study of problems of geology involving further researches in chemistry and physics."

SECTION B — GEOLOGY

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(*Hall* 14, *September* 21, 3 *p. m.*)

CHAIRMAN: PROFESSOR T. C. CHAMBERLIN, University of Chicago.
SPEAKER: PRESIDENT CHARLES R. VAN HISE, University of Wisconsin.
SECRETARY: PROFESSOR R. D. SALISBURY, University of Chicago.

THE PROBLEMS OF GEOLOGY

BY CHARLES RICHARD VAN HISE

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THE subject "The Problems of Geology" was assigned to me. I should not have ventured to select so formidable a topic for a brief address.

Relations of the Sciences

We are all aware that geology is a many-sided subject. While at the outset it was a simple observational study, it soon developed physical, chemical, astronomical, and biological sides. The importance of these different sides has continuously increased, so that we now often speak of physical geology, chemical geology, astronomical geology, and biological geology.

To appreciate the position of geology among the sciences it is necessary to go back to fundamental definitions. Natural philosophy in the old and broad sense may be defined as the science which treats of energy and matter. But investigations have shown that the ether also must be considered, and hence this definition needs modification. Some physicists have been inclined to extend the scope of the term "matter" to include matter in the old sense, and also ether. But it seems to me that until the two, which appear to be so different, are shown to be essentially one, it is better to use the term "matter" strictly in its old sense. But it is advisable to have a term which shall include both matter and ether, and for this place the word "substance" seems suitable.[1] Using the term in this sense, natural philosophy may be defined as the science which treats of energy and substance.

Physics is the science which treats primarily of energy; chemistry is the science which treats primarily of matter. Thus physics considers mainly the actions and transformations of energy through matter and ether; and chemistry considers mainly the actions and transformations of matter through energy. But since energy is manifest to the senses only through matter, and since matter does not exist without manifestations of energy, the relations of the two sciences are very intimate. In any book upon either subject the treatment constantly passes over to the other; indeed, energy and matter are inseparable, — one cannot be considered without the other. Recently the relations between physics and chemistry have become even closer by the rise of the intermediate science, physical chemistry. This science completely bridges the gap between the two and unites them as a whole into the conjoint science of physics-chemistry, which is the science of energy and substance. As thus defined, physics-chemistry becomes a synonym of natural philosophy in its broad sense.

While physics and chemistry are really a single science, it is to be repeated that the chief point of view of physics proper is that of energy, and the chief point of view of chemistry proper is that of matter. This will be appreciated if one but mention the subjects considered in text-books of physics and chemistry. Some of the subjects of physics are sound, heat, light, and electricity. These are all forms of energy. The chief subjects for consideration by chemistry are the elements and their combinations, such as helium, chlorine, iron, calcium-carbonate, etc. These are all forms of matter. Since physics-chemistry treats of all the energy and substance within the reach of our senses, physics and chemistry are the two sciences the principles of which are believed to be applicable to the entire visible universe.

Astronomy treats of energy and substance in the heavens. It is concerned primarily with the nature and development of the heavenly systems. Under the above definition, astronomy is the science of the physics and chemistry of the heavens. Biology treats of energy and substance in living organisms. Under this definition, biology is the science of the physics and chemistry of organisms. Geology treats of the energy and substance of the earth. Under this definition, geology is the science of the physics and chemistry of the earth. It includes mineralogy. These definitions may not be complete, but at least they are true so far as they go.

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[1] This definition of the word "substance" is different from that of Holman, who, as I understand it, makes the term so comprehensive as to include matter, ether, and energy. By him the word "matter" is apparently used to comprise what is here covered by both matter and ether. See Silas W. Holman, *Matter, Energy Force and Work*, p. 135 ff.

It is not necessary, for present purposes, to consider the possible defects of the definitions given, except that for geology. Objections may be raised to defining geology as the science of the physics and chemistry of the earth, on the ground that this definition is inadequate to cover descriptive and historical geology. It may be said that it is a part of geology to describe the facts exhibited by the earth as they appear, without reference to physics or chemistry. It may be said that the history of events, as shown by the rocks and fossils, does not necessarily require physical or chemical treatment. There is some truth in these statements, but on the other side it may be held that the facts are the results accomplished by physical and chemical work. These facts become important and significant mainly as they are interpreted in physical and chemical terms. The objects of the earth — the complex results of chemical and physical work — if described without reference to the manner in which the results came about, have comparatively little interest. In reference to historical geology it may be said that this subject gives a chronological arrangement of the results of chemical and physical work.

It thus appears that physics and chemistry are the elementary sciences, while astronomy, biology, and geology may be defined, possibly with some lack of completeness, as the applications of the principles of physics and chemistry to various complex systems. In this sense astronomy, biology, and geology are applied sciences.

We are now in a position clearly to indicate the relations of geology to the sciences mentioned. So far as the earth is one member of one of the heavenly systems, it is a subject of astronomy. So far as organisms constitute a small part of the earth, they are the subject of geology. Since the earth is one of the subjects of astronomy, and since the entire kingdom of organisms constitutes a small part of the material of the earth, geology is closely related on one side to astronomy, upon the other side to biology. Geology is one of the children of astronomy. Geology begins with the earth at the time of its astronomic birth. As geology is one of the children of astronomy, so biology is one of the children of geology. As the result of various processes upon the earth, chemical and physical, organisms have been formed, and have gone through their long and complex development. But astronomy, geology, and biology — grandparent, parent, and child — have long existed side by side, and their interaction and mutual effects have been most profound. One cannot be comprehended independently of the others.

While geology is very closely related to astronomy and biology, we have seen that it is still more closely related to physics and chemistry. Since physics-chemistry is the science of energy and substance in general, and since geology is the science of the energy and substance of the earth, geology is not simply related to those subjects — it rests upon them as its one secure foundation. They are the elementary sciences upon which geology is based; for they are the sciences of all energy and substance of which the object of geological science is an insignificant fraction.

We have now reached the most fundamental problem of geology, — the reduction of the science to order under the principles of physics and chemistry. To a less extent geology is subject to the sciences of astronomy and biology.[1]

While the relations of geology to the other sciences, as above set forth, are incontestable, it was possible to appreciate those relations only after the sciences were well developed. Geology did not begin consciously as the science of the physics and chemistry of the earth. The phenomena of the earth were studied as objects, and thus geology was at first an observational study. The next step, a revolutionary one, was to explain the observed phenomena in terms of physical and chemical processes, many of which could be observed. But few have asked the question: "What is a geological process?"

Geological Processes

It is a curious fact that, while the word "process" is used in innumerable geological papers and text-books, I have been unable to find anywhere a definition of a "geological process."

I shall define a "geological process" as the action of an agent by the exertion of force involving the expenditure of energy upon some portion of the substance of the earth.

Physical definitions of "force," "work," energy," and "agent." In order to understand the above definition of "geological process"

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[1] The earth is the vastest aggregate of matter within the direct reach of man. By a study of a small part of this aggregate the principles of physics and chemistry have been formulated. The material which has been studied is but an inappreciable part of the material of the earth, and but an infinitesimal part of the substance of the universe. Yet the doctrine is unhesitatingly accepted that the principles of physics and chemistry, wrought out with reference to this minute fraction of substance, are not only applicable to all the materials of the earth, but to all parts of the visible universe. This daring generalization has received astonishing confirmation by studies of other portions of the visible universe through the spectroscope and photographic plate.

In the generalization that the principles of physics and chemistry, developed by study of small masses of material, apply to all parts of the universe, we have a case of the extension of a generalization from a part to the whole, which surpasses almost any similar extension of reasoning. Indeed, some philosophers have seriously questioned the legitimacy of the conclusion.

In view of the foregoing, it is rather curious that the geologist now finds his most important problem, the problem of problems, in the explanation of phenomena exhibited by the heterogeneous earth in terms of those principles of physics and chemistry built up mainly by observation, experiment, and reasoning upon a minute fraction of the earth it is necessary to define the terms "force," "work," "energy," and "agent."

Hoskins defines "force" as action exerted by one body upon another tending to change the state of motion of the body acted upon.[1] According to Daniell's more simple definition, "force" is any cause of motion.[2]

When a force applied to a body moves the body in the direction toward which the force acts, it does work.[3] In this sense "work" is the product of force into displacement, the common formula being $W = FS$. The unit of work is defined as the quantity of work done by a unit force acting through a unit distance.[4]

Hoskins defines "energy" in the terms of force and work. Thus he says when the condition of a body is such that it can do work against a force or forces that may be applied to it the body is said to possess energy. The unit of energy is the same as that of work.[5] According to Daniell's more simple definition, "energy" is the power of doing work.[6]

The order of definition of the above terms is that in which knowledge of them has developed. The actions of forces in doing work are observed. From such observations the existence of energy is inferred. Wherever forces act upon matter and work is done, energy must exist. Further reasoning shows us that bodies may possess energy which is latent and is not exerting force. Hence many physicists have defined "energy" without introducing the words "force" or "work." Thus, according to Holman, "energy" is power to change the state of motion of a body.[7] If energy be recognized as the primary thing, then "force" can be defined in terms of energy. According to Holman, "force" is that action of energy by which it produces a tendency to change the state of motion of bodies.[8] Similarly, the word "energy" may be introduced into the definition of the word "work." Thus Holman says "work" is that action of energy by which it produces motion in a free body, or produces or maintains the motion of a body against resisting forces.[9]

An "agent" is any portion of the substance of the earth which may exert force and thus expend energy to perform geological work. Thus ether, air, water, and rock are agents.

The next step in the comprehension of geological processes is a consideration of the kinds of energies, forces, and agents, and their relations.

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[1] T. M. Hoskins, *Theoretical Mechanics*, pp. 2 and 16, 1900.

[2] Alfred Daniell, *A Text-book of the Principles of Physics*, 3d ed. (1895), p. 4.

[3] Hoskins, *op. cit.*, p. 298.

[4] *Ibid.*, p. 298.

[5] *Ibid.*, p. 308.

[6] Daniell, *op. cit.*, p. 2.

[7] Silas W. Holman, *Matter, Energy, Force and Work*, p. 20, 1898.

[8] *Ibid.*, p. 41.

[9] *Ibid.*, p. 17

Kinds of energy and force. Ultimately the forms of energy may be reduced to a few, and possibly to a single kind. Indeed, some physicists believe that all forms of energy are really but different manifestations of kinetic energy. But the number of elementary kinds of energy in the universe is a problem for the physical philosopher, not the geologist. The geologist is concerned in all the kinds of energy which he observes at work. These are: (1) gravitation energy, (2) heat, (3) elasticity energy, (4) cohesion energy, (5) chemical energy, (6) electrical energy, (7) magnetic energy, (8) radiant energy (including radiant heat, radiant light, and electromagnetic radiation).[1]

From another point of view energy may be classified into kinetic energy and potential energy. Under static conditions of all the parts of a system any or all of the kinds of energy above named may be exerting force, but so long as no motion occurs and no work is done they are all potential. When anywhere in the system movement takes place and work is done, some portion of the energy becomes kinetic. Work and kinetic energy are inseparable. As multifarious kinds of work are always going on in the world, potential and kinetic energy are always existent. For the most part we can trace the kinetic energy back to one or more of the various classes of energy above mentioned, but some part of it may be derived from other unnamed sources.

Any of the forms of energy may exert force, hence we have the terms "force of gravitation," "force of heat," "force of elasticity," "force of cohesion," "chemical force," "electrical force," "magnetic force," and "radiant force."

Any or all of these forces may be exerted both under static and dynamic conditions. When the conditions are static, the energy is potential. When the conditions are dynamic and work is done, some portion of the energy is kinetic. To illustrate: For many years a cliff may stand; but finally a portion of it falls and geological work is done. The force of gravitation is exerting the same pressure upon the material concerned during all the years of quiescence and during the brief period of movement, and, for that matter, continues to be exerted after movement ceases. During the static conditions the energy of gravitation is potential. During movement some part of it, by pressure of the force of gravitation, passes into kinetic energy. And this energy, through the agency of the falling part, the agent, does further geological work upon the material at the foot of the cliff.

All of the forms of energy and force are important in geology, but the geological work of some of them has been more clearly discriminated than that of others. For instance, the geological results produced by electricity and magnetism have not been worked out, although I have no doubt that electrical and magnetic energy have produced important permanent effects upon the earth which ultimately will be discriminated.

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[1] Silas W. Holman, *Matter, Energy, Force and Work*, p. 37, 1898.

Geological use of the words "force," "energy," and "work." To the present time the geologist has much more frequently used the word "force" than "energy." This is because the geologist is usually more concerned with the exertion of force by an agent than he is with the source or amount of energy which the agent contains. Physical investigations seem to show that substance contains enormous quantities of energy, only a small part of which is manifest to the senses, and this only under special circumstances. So far as geological bodies have great stores of energy which are not manifest as force, there is no change of condition — no geological process. The geologist is primarily concerned with the energy which is manifesting itself either statically or dynamically by the exertion of force. Consequently, he more often refers to the forces of geology than the energies of geology. This is the more natural since the unit of force and the unit of energy are the same, and that energy is measured only by its action as a force. While in the past the primary interest of the geologist has been in force rather than in energy, it is probable that in the future he will become more and more concerned in the energy itself and its sources.

Often the geologist has made no discrimination between the words "force" and "energy." He has frequently used "force" in the old sense, both to cover the thing itself, the energy, and the action of energy, the force, in accomplishing work. This formerly was the practice of physicists also, who, for instance, spoke both of the conservation of force and the exertion of force. If the conclusion be correct that the source and amount of energy concerned in a process should be discriminated from its action as a force, it is clear that the time has now come when the geologist must in his writing clearly differentiate the two ideas.

Since the physicist now makes an important discrimination between the words "energy" and "force," it may be necessary for the geologist to follow him in his definitions of these words, although much can be said against technicalizing and narrowing the use of the general term "force." Probably the interests of all the sciences would have been best subserved if the physicists had introduced a new word for the technical sense assigned to the word "force," and had left this term to be used in the general way in which it has been used in the past in science, and will continue indefinitely in the future to be used in literature. This is especially true since, if we confine the word "force" to its physical definition, we are in constant need of a word to cover both energy and force, as defined by the physicists. If the latter word be technicalized, I can think of no better word than "power" for the conception which includes both. This was the word used for this place by Hutton in the opening pages o his epoch-making paper on the *Theory of the Earth.*[1]

It is also to be noted that the word "work," as above defined, i also technicalized, having reference only to the exertion of force i producing change of state of motion. With this meaning it has n relation to the material results. To illustrate: By the expenditure of energy, the crust of the earth may be fractured, or material be transported from one place to another. In the general sense used by geologists, these results are often spoken of as "work." It is certainly a very grave question whether geologists can afford to restrict the word "work" to its physical definition, and thus be obliged to discontinue its use in an indefinite sense, both for the expenditure of the energy, and the effect of such expenditure, or for either alone. While this is so, it may be said there are very considerable advantages in having a technical word for the physical meaning of work. This would assist the geologist to think clearly and discriminate between the expenditure of energy and the material effects of such expenditure.

Whatever meaning the geologist assigns to the words "force" and "work," he should have a clear understanding of the conceptions which the physicists have of their meaning, and should attempt to express these conceptions in some way. Also he should make it clear, in case he decides not to use the words "force" and "work" in the physical sense, that the old general usage is retained for them. In this paper I shall use "force" in its technical sense, but retain the common usage for the word "work."

The agents of geology. We are now ready to classify the agents of geology. They may be grouped into ether, gases, liquids, and solids. Possibly organisms are so peculiar a combination of gases, liquids, and solids that they should constitute a fifth group, and in this case the agents may be classified into ether, gases, liquids, solids, and organisms. From another point of view the agents may be classified into their chemical elements, some seventy or more in number, but of which only about twenty are so abundant as to be important.

The small number of categories of energies and agents given might lead to the conclusion that the subject of geology is reduced to simpler terms than is really the fact. Each of the forms of energy, gravitation, heat, elasticity, cohesion, chemical affinity, electricity, magnetism, and radiation is most complex and acts as forces in most diverse ways. The number of gases, of liquids, and of solids which occur in nature are beyond number. They are most diverse in character. For instance, the liquids vary from nearly pure water to magma. The solids comprise all kinds of minerals, of which there are many hundreds, and the various combinations of these minerals in rocks, the different phases of which are very numerous. Gas without the presence of liquids and solids, liquids without the inclusion of gases and solids, and solids which contain no gases or liquids, while perhaps possible in a physical or chemical laboratory, are not found in nature. As remarked by Powell, gases, liquids, and solids are everywhere commingled upon the earth. All are commingled with ether. Thus the various combinations of agents are beyond computation. Also definite agents, for instance, water, may occur in various kinds of bodies, each of which acts in a manner peculiar to itself.

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[1] Charles Hutton, *Theory of the Earth, Philosophical Transactions of the Royal Society of Edinburgh*, 1785, pp. 212–214.

The materials upon which the agents act are of the same kinds, and have the same diversities and complexities, as the agents themselves. Moreover, the work done inevitably affects both the material acted upon and the agent. The agent that grinds the rock-floor at the bottom of a glacier is also ground. This necessity of work upon both agent and substance acted upon comes under the law of Newton in reference to action and reaction. The fact of work, both upon agent and substance upon which the agent acts, raises the question as to the distinction between the two. The answer is: The agent is the substance containing energy which it expends in doing work upon other substances. The substance upon which work is done may thereby receive energy, and thus become an agent which does work upon other substances; and so on indefinitely. Indeed, the rule is that one process follows another in the sequence of events, until the energy concerned becomes so dispersed as to be no longer traceable. Theoretically this goes on indefinitely.

Analysis of geological processes. We have seen that the action of one or more agents through the exertion of force and the expenditure of energy upon one or more substances is a geological process. It is rare indeed, if it ever happens, that a single agent works through a single force upon a single substance. Commonly two or more agents are doing work by the expenditure of energy of various kinds at the same time upon more than one material. The processes of geology, therefore, vary in their complexity from the action of a single agent through a single force upon a single substance, to the action of all kinds of agents through all classes of force upon the most diverse combinations of substances. Thus the solution by rain-water of pure calcite is a process. Also erosion, which is the work of all the agents by the expenditure of various kinds of energy upon the most diverse combinations of materials, is called a process. It is plain that the number of processes of geology, comprising as they do all possible combinations of energies, agents, and substances, are beyond number, if indeed they are not infinite. If geology is to be simplified, the processes must be analyzed and classified in terms of energies, agents and results. Each of the classes of energy and agent should be taken up, and the different kinds of work done by it discussed. For instance, the work of the force of gravitation through gases, liquids, and solids should be analyzed. To some extent this has been attempted, but very imperfectly indeed. And such discussion has scarcely been seriously undertaken for the other forms of energy. Text-books should consider each of the classes of energy by itself, the nature of the forces it exerts, the processes through which it works, and the results accomplished through the various kinds of agents.

The general work of each of the agents and the results accomplished should be similarly considered. Not only so, but the work of the different forms that each of the agents takes should be separately treated. Thus, besides considering the work of water generally, the work which it does both running and standing must be treated. The first involves the work of streams; the second, the work of lakes and oceans. This involves the treatment of streams as entities, or, to use a figure of Chamberlin's, as "organisms." The treatment of the work of gases should involve the subjects of gases of the atmosphere, gases of the hydrosphere, and gases of the lithosphere. The treatment of the agents will be more satisfactory in proportion as the work done by each of the forms of each of the agents is explained under physical and chemical principles in the terms of energy.

It is plain that the treatment of the energies of geology and the treatment of the agencies of geology will overlap, since one cannot be considered without also considering the other; but this is an advantage rather than a disadvantage, for each of the two points of view is very important in enabling the mind to grasp the composite whole. Just as in the science of physics-chemistry it may sometimes be advantageous to consider the subject mainly from the point of view of substance, and at another time mainly from the point of view of energy, and the treatments from both points of view are necessary to build up the science of physics-chemistry; so it is necessary to consider the subject of geology from the points of view of energy and of agent, if an approximation to adequate comprehension be gained.

As already intimated, another point of view from which geology may be considered is the result. This was the chief point of view of the early geological papers and text-books, which were content to tell of phenomena. Phenomena may, and often are, observed and described in advance of their physical-chemical interpretation. But the naming or even the description of the phenomena of the earth, without reference to energy or agent, is very unsatisfactory. And usually the valuable descriptions of before unobserved phenomena are made in connection with theories of their physical and chemical significance. But it is still true that observation and description present a third important point of view which interlocks with and overlaps the treatment of geology from the points of view of energy and agent.

So complex is the earth that to enable the mind to comprehend the intricately interlocking whole, the subject should be considered from as many points of view as possible. If only the human mind were sufficiently powerful, and means of expression adequate, the ideal method of treatment would be simultaneous consideration and exposition of all possible points of view. But since this method of treatment is an impossibility, we must necessarily at any time consider each portion of the subject in part and treat it in part. The problem is then the selection of the various partial points of view which are important, and the determination of the order of their consideration.

No one, I think, can hold that any of the points of view above mentioned — process, energy, agent, and result — is unimportant in a general treatment of the subject of geology. It is therefore clear that all these points of view must be handled. There may be difference of opinion as to the order in which they shall be presented; and for different parts of the subject of geology and for different purposes the best order will vary.

We are now in a position to foresee the future development of the science of geology. The early papers and text-books were content to tell of accomplished results. Almost nothing was said with reference to processes. As the science developed, there crept into the literature of the subject more and more reference to processes. The present year a text-book of geology by Chamberlin and Salisbury has appeared, the first which avowedly attempts to treat geology from the point of view of processes rather than phenomena.[1] This is a great step in advance. But a large part of the task of reducing the processes to order in terms of energies, agents, and results still remains to be done. When this is accomplished, we shall have a statement of the principles of geology in terms of physics and chemistry.

How knowledge of processes has developed. The principles of geology have been developed in the past and will continue to be developed in the future both from the study of processes now in operation and by the consideration of the results of processes which cannot be observed. An excellent illustration of a branch of geology, the principles of which have largely been established by the observation of processes now in operation, is furnished by physiography. So far as one can see, the surface of the land is now being modified by the energies and agents of geology as rapidly as at any time in the past. These energies and agents may have varied in their efficiency from time to time and place to place, but the above statement is broadly true. There are other branches of geology, the principles of which have been mainly developed from results accomplished rather than from observation of the present actions of energies and agents. In such branches the probable energies, agents, and processes which produced the observed results were developed from a consideration of the methods by which chemical and physical energy through the agents available could have produced the results observed. For instance, the development of the solar system occurred but once. During that development the earth was formed, including the atmosphere, hydrosphere, and lithosphere. The process of differentiation was not observed by man, cannot be repeated by him. The only method of reaching a probable conclusion as to the manner of accomplishment of the complex result is to consider in what possible ways physical and chemical energy may have acted upon the enormous masses of universe stuff out of which the earth was constructed, and to check this reasoning by the attainable knowledge of what is now occurring upon other heavenly bodies.

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[1] Chamberlin and Salisbury, *Geology*, vol. I, *Processes and their Results*, 1904.

The qualitative and quantitative stages of explanation. The task of explaining geology in terms of processes involving energy and agent has two stages — the qualitative stage and the quantitative stage. For most problems we have as yet been unable to go beyond the qualitative stage. In the qualitative stage of a problem it is shown that a cause is real. In this stage the question is not asked as to how far the explanation applies; *i. e.*, its quantitative importance. Most geologists are content when they reach the qualitative stage. A certain cause is determined to be real in the explanation of certain phenomena. It is then usually assumed that this cause is the only cause. For instance, it has been generally accepted that the loss of heat by the earth results in decreased volume, and that such condensation is a cause for crustal deformation. Many geologists have stopped at this point satisfied. They have not asked the question: To what extent can loss of heat by the earth explain crustal deformation, and are there any other causes which can be assigned? Some years ago I listed a number of causes, each of which partly explains deformation. In addition to secular cooling, they are as follows; volcanism, cementation, change of oblateness of the earth, change of pressure within the earth, change of physical condition of the material of the earth, and loss of water and gas from the interior.[1] Evidently, in order that we may have even an approximately correct idea of the chief causes for crustal deformation, the question must be answered as to the quantitative importance of each of the causes.

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[1] C. R. Van Hise, *Estimates and Causes of Crustal Shortening, Journal of Geology*, vol. VI (1898), pp. 10–64.

The consideration of the processes of geology by quantitative methods is superlatively difficult, yet this task must be undertaken if the science ever approximates certainty of conclusions. This leads to the relations of mathematics to geology. The moment we pass to the quantitative treatment of processes the assistance of mathematics is needed. For simple quantitative calculations arithmetic and algebra may suffice, but for the more difficult problems of geology the assistance of higher mathematics is needed. This, then, raises the question as to whether or not it is expected that the geologist, in addition to knowing physics and chemistry, must also be a mathematician. Undoubtedly this is the ideal equipment of a geologist, which, unfortunately, few if any possess. There are many geologists who apply simple mathematics to various problems. But the man who is so familiar with forces, agents, processes, and phenomena of geology that he is able to handle them, and at the same time is capable of handling higher mathematical reasoning, is rare indeed. Those geologists who have made the attempt to combine mathematical with their geological reasoning usually have shown marked deficiency in their mathematics. Upon the other hand, those mathematicians who have attempted to handle the problems of geology mathematically have usually been so deficient in a knowledge of geology that their work has been of comparatively little value. In view of these unfortunate results, it seems to me that the time has come for coöperation between geologists and mathematicians in the advancement of the science of geology to a quantitative basis. Two or more men should work together, some of them geologists with a broad familiarity with the phenomena and methods of their science, and the others expert mathematicians. In continual consultation, the geologist and mathematician will be able safely to handle the problems of geology quantitatively. This happy condition of coöperation, once reached, will be sure rapidly to advance the science.

The quantitative solution of geological problems is likely to emphasize also another of the principles of geological method of the greatest importance. The causes offered to explain the phenomena do not necessarily exclude one another. It is believed that often each of them is a real cause, and partly explains the phenomena, — that the different causes are complementary. While a majority of geologists have been content with suggesting a single physical cause for a phenomenon, others have taken more than one possible cause into account. Thus Chamberlin[1] has formally adopted the method of multiple hypotheses. But the great majority of those who have considered more than one hypothesis in connection with a geological problem have carried on their discussions as if one of the suggested causes must be selected to the exclusion of the others.

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[1] T. C. Chamberlin, *The Method of Multiple Working Hypotheses*, *Journal of Geology*, vol. v. (1897), pp. 837–848.

As a matter of fact, almost every complex geological phenomenon has not a simple, but a composite, explanation. To illustrate, in Chamberlin and Salisbury's text-book of geology it is stated that the explanation of volcanism may be given upon the assumption that the lavas are original; or, second, on the assumption that the lavas are secondary. Under the first assumption it is suggested (1) that lava outflows from a molten interior, and (2) that lavas flow from molten reservoirs. Under the second assumption it is suggested that lavas may be assigned (3) to the reaction of water and air penetrating to hot rocks, (4) to relief of pressure, (5) to melting or crushing, (6) to melting by depression, and (7) to the outflow of deep-seated heat.[1] At the close of the discussion it is said that these hypotheses "must be left to work out their own destiny."[2] I fear many will make the inference, although I have no idea that the authors so intended, that one among these hypotheses will be victorious in the struggle for existence and the others totally overthrown. My point in this connection is that the two main suppositions, and all of the hypotheses under them, may be true in part; that these various explanations are not necessarily exclusive of one another, but may be supplementary. When we have a quantitative discussion of the probable effects which may be expected from each of the causes suggested, we shall have some idea of their possible relative importance. For my own part I have no doubt whatever that volcanism is to be explained by some combination of the seven causes mentioned, with doubtless other causes which have not yet been suggested, rather than by a single cause. As soon as it is appreciated that to explain a complex phenomenon several causes are usual, if not invariable, rather than exceptional, it becomes plain that their relative importance should be determined, and this can be done only by quantitative methods.

The Individual Problems of Geology

Thus far we have been considering the problem of geology as a general one. The subject assigned, "The Problems of Geology," might imply a treatment of the particular problems at present being considered by geologists. For an address this interpretation of the subject is impracticable. Adequately to discuss one of the unsolved problems of geology from the point of view advocated would require a monograph. Not only is it impossible to discuss unsolved problems of geology, but it is impracticable, within the limits of this paper, even to list the problems demanding solution. As evidence of the correctness of this statement it may be noted that a subcommittee of the Carnegie Institution stated scores of problems upon the investigation of rocks, the statement of which, limited to the briefest possible terms, occupies a number of printed pages.[1]

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[1] Chamberlin and Salisbury, *Geology*, vol I, *Processes and their results*, pp. 595–602, 1904.

[2] *Ibid.*, p. 602.

Illustrations of Treatment of Geological Problems from the point of view of Energy, Agent, and Process

While it is not practicable to discuss, or even to list, the particular problems of geology, it is possible to mention illustrations of the systematization and simplification of the science by the treatment of processes in terms of energy and agent. These I shall take from my own publications, for the reason that I can more easily give them than any others. My chief subjects of study have been (1) the gross and minor deformations of the lithosphere, and (2) the interior transformations of the rocks, or metamorphism. When I began the study of the first of these subjects, I found a heterogeneous mass of facts in reference to the deformation of many regions, with various guesses as to how the results came about, but with no consistent attempt to reduce the many observed phenomena to order under the principles of physics and chemistry. The subject of metamorphism was in an even worse condition. The work upon this subject was of the most random character; indeed, nothing short of chaos prevailed. A person who attempted to carry the multitudinous statements of facts in his mind would need more than cyclopædic powers of memory, and the statement would not even have had the artificial order of an encyclopædia. I became convinced that, if the treatment of metamorphism was to continue along the old lines, the subject was doomed to hopeless confusion.

With the above condition of affairs before me, I set about attempting to ascertain the principles which control the various kinds of deformation of rock masses, and which underlie the transformation of rocks. It soon became plain to me that the task was a great problem in applied physics and chemistry. When this was realized, it became clear that it was necessary to know the principles of physics and chemistry applicable to the deformation of matter and to the alteration of rocks. Thus my first task was to remedy the defects of my basal training by gaining a working knowledge of the well-established principles of these subjects. This task I found a formidable one, which occupied much of my time for several years, and which I can claim to have only very imperfectly accomplished.

In order to understand the diverse phenomena of crustal deformation, it was plainly necessary to know the principles of deformation of small masses, such as can be handled in the laboratory. Unfortunately it was found that this part of the subject of physics is in a very imperfect condition. No systematic statement is available as to the manner in which different substances behave under varying conditions of stress. While studies have been made of the deformation of iron under a moderate range of conditions, comparatively little has been done concerning brittle bodies such as constitute the rocks. Exact knowledge is needed as to the behavior of rocks under the most extreme variations of stress, temperature, amount of water, and other conditions. But while it is highly desirable to have this knowledge, the geologist cannot wait until it is available. The only practicable course is to study closely the phenomena of rock deformation, and interpret these facts in the light of the physical and chemical knowledge available.

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[1] *Carnegie Institution Year-Book*, no. 2, pp. 195–201.

A broad study of the phenomena of deformation by various men showed two classes of very diverse phenomena. In some areas the prominent deformations of the rocks are those of fractures, such as joints, faults, brecciations, etc. In other places the deformations are mainly those of flexure. For instance, in some places one finds that brittle rocks, such as jaspilite and quartzite, are deformed almost wholly by numerous fractures, and in other places have been bent within their own radius, or even minutely and extremely crenulated with no sign of fracture. A close study of the geological conditions under which these two classes of deformation occurred shows that the more modern rocks, which have at no time been very deeply buried, are those which are most likely to exhibit only the effects of rupture; whereas the ancient rocks, and especially those which have been deeply buried, are likely to show the evidence of profound folding without rupture, although often there is superimposed upon the flexures more recent fracture deformation. Physical experiments had shown that, when a brittle substance like a rock is stressed beyond the limit of elasticity under the conditions of the earth's surface, cohesion is overcome, and rupture takes place. This fact correlated with the general observation of rupture in recent rocks and those deformed near the surface, led to the conclusion that normally the deformation of the outer part of the earth is by fracture.

After this conclusion was reached, it was a natural step to the conclusion that at a very moderate depth below the surface of the earth the superincumbent pressure is greater than the strength of any rock, and that, if openings could be supposed to exist, they would be closed by pressure; in other words, that the pressure due to the force of gravitation is sufficiently great, so that the molecules of the rocks are held within the limits of molecular attraction or are within the limits of the force of cohesion. This naturally led to the suggestion of a deep-seated zone of rock-flowage, in opposition to a zone near the surface, that of fracture. At the time this conclusion was reached, no experiments had been made actually showing the deformation of rocks under the conditions of the deep-seated zone, but since that time Adams and Nicolson have deformed rocks by flowage in the laboratory.[1] Thus observation of the geologist, inference from the observation, and experimental work have led to advance in the science of physics.

For the present purpose the important thing is to observe that a realization of the very diverse results which follow from deformation under different physical conditions led to a satisfactory classification of two great sets of phenomena which had been noted, but without any reason being assigned why one occurs at one place and the second at another place. Thus in the text-books of geology, joints, faults, and folds were described. But there was no attempt to explain why fracture occurred here, folds there, and in a third place both. After it was realized that the great earth-movement makes joints, faults, and other fractures at and near the surface, and at depth, below these structures, other structures which have been called folds, it was possible to reduce the gross deformation of rocks to some systematic order under the principles of physics. There of course remains the working-out of the precise physical conditions which result in the various diverse phenomena. For instance, what are the exact conditions of stress which result in the many complex systems of joints? While progress has been made upon this and other problems of gross deformation, a vast amount of work remains to be done before the subject will be even approximately reduced to order in the terms of energy, agent, and process.

It has already been intimated that the subject of rock alteration was in an even more unsatisfactory state than that of gross deformation. The particular alteration of this or that rock was given without any adequate consideration of the geological, physical, or chemical conditions under which the change took place. Thus there were many thousands of descriptions of rock alterations, but no understanding of the reasons why the particular alteration for a given rock found at a given place occurred. To make the matter worse, almost every description of rock alteration was accompanied by vague guesses as to the causes of the changes, the majority of which were little short of grotesque.

After it was appreciated that the gross deformation of rocks is very different in an upper and a deeper zone, the question naturally arose as to whether there are not differences in the rock alterations in these zones. This idea, when followed up, resulted in astonishingly fruitful results. It was found that in the upper zone, that of fracture, the chief alterations which take place are those of oxidation, carbonation, and hydration. These reactions occur with liberation of heat and expansion of volume. In other words, the reactions are controlled by chemical energy. In the lower zone the dominant factor controlling alterations is physical energy. Pressure diminishes the volume. In order to accomplish this, the chemical reactions of the upper zone are reversed. Deoxidation, silication with decarbonation, and dehydration occur with absorption of heat. The reactions controlled by the force of gravitation are under the principles of physics. It thus appears that the reactions of the two zones are largely opposed. It is plain that if the subject of metamorphism is to be reduced to order, the alteration of the upper zone, that of fracture, must be discriminated from that of the deep-seated zone, that of rock-flowage.[1]

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[1] F. D. Adams and J. T. Nicolson, *An Experimental Investigation into the Flow of Marble*, *Philosophical Transactions of the Royal Society of London*, Series A, vol. cxcv (1901), pp. 363–401.

The working-out of the principles of metamorphism was a physical-chemical problem. The handling of the problems of rock alteration with fairly satisfactory results was possible because of the rise of physical chemistry. Had this science not been developed within the past score of years, it would not have been possible to have gone far upon the problem of metamorphism.

It is to be emphasized that gross deformation is not independent of metamorphism, or metamorphism independent of gross deformation; the two interlock. The general solution of the problem of gross deformation made it possible to formulate the principles controlling the interior transformations of rocks. In a similar manner these problems interlock with the other problems of physical geology, and physical geology interlocks with the other sides of the subject. The whole science is one interlocking system, a part of which cannot be satisfactorily developed independently of the other parts. For instance, weathering can be placed in order only when considered in connection with general metamorphism, erosion, and sedimentation. Ore-deposits can be explained only by combining the principles of volcanism, deformation, metamorphism, etc.

In attempting to reduce a small part of the subject of geology to order under the principles of physics and chemistry, the plan was followed of oscillating between observations of the facts as exhibited in the field and laboratory, and their physical-chemical explanation. After a large number of facts were observed in the light of known principles, the attempt was made better to formulate the principles which explain them. After this was done, the facts were again more comprehensively studied in the field and in the laboratory in the light of the new principles. The statement of principles was then modified and improved by use of the new facts. The improved statement of principles was again tested by further facts. Thus the process of development has been a series of approximations toward both completeness of statement of fact and perfection of formulation of principle, but neither has been attained, nor, so far as we can see, will they ever be reached.

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[1] The necessarily narrow limits of this paper render it extremely difficult to show the manner in which the subject of metamorphism has been treated under the system advocated as a general method for geology. By referring to *Monograph XLVII of the United States Geological Survey*, a treatise on metamorphism now just appearing, the reader will better appreciate the illustration In this volume the forces of metamorphism, the agents of metamorphism, and the zones of metamorphism, are first fully treated, the point of view being mainly physical-chemical. After the general principles contained in these chapters are given, the alterations in each of the different belts and zones are developed. The point of view of the latter chapters is mainly geological, but the geology is interpreted in the terms of the principles earlier formulated.

Necessity for Advance in the Sciences of Physics and Chemistry

Very often, in the attempt to find principles applicable to the phenomena of deformation and metamorphism, it has been found that the science of physics-chemistry is not sufficiently advanced to make this possible. In such cases physicists and chemists have been asked to develop this subject at the needed points. But at innumerable places the problems have proved to be so numerous and complex that the necessary aid has not been obtainable. Thus there has arisen, with reference to my own work, a great line of unsolved problems which demand the coöperation of physicists and chemists. The same is true of the work of all other geologists interested in the fundamental problems of geology. As a consequence, when a committee was appointed by the Carnegie Institution to consider what could best be done for the advancement of geology, it was unanimously decided that the most pressing need of the science was, not further support of the study of the phenomena of geology, but the advancement of the principles of physics and chemistry upon which geology is based.[1] In a small way some of the physical and chemical problems, the solution of which are asked by geologists, have been taken up by the Carnegie Institution. Thus the demands of the geologists that their science shall be reduced to order under the principles of physics and chemistry are likely to result in important advances of these sciences.

Defects of Geological Literature

If further proof than that already given were needed of the importance of the knowledge by geologists of the basal principles of the elementary sciences, and of their application to geological problems, it is furnished by the literature of geology. It seems to me that the radical defect which pervades the literature of the subject is due to the lack by geologists of such knowledge. Because of this, many geologists are wholly unable to make a logical arrangement of their material, or respectably to discuss the phenomena observed with reference to causes.

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[1] *Carnegie Institution Year-Book*, no. 1, 1902, no. 2, 1903.

Indeed, some geologists seem to take pride in lack of knowledge of principles and of their failure to explain the facts observed in the terms of the elementary sciences. I have heard a man say: "I observe the facts as I find them, unprejudiced by any theory." I regard this statement as condemning the work of the man, for the position is an impossible one. No man has ever stated more than a small part of the facts with reference to any area. The geologist must select the facts which he regards of sufficient note to record and describe. But such selection implies theories of their importance and significance. In a given case the problem is therefore reduced to selecting the facts for record, with a broad and deep comprehension of the principles involved, a definite understanding of the rules of the game, an appreciation of what is probable and what is not probable; or else making mere random observations. All agree that the latter alternative is worse than useless, and therefore the only training which can make a geologist safe, even in his observations, is to equip him with such a knowledge of the principles concerned as will make his observations of value.

It is doubtful if more than one or two text-books of geology have been written which do not contain many statements capable of arousing the amusement of the physicist. When the geologists who write the standard books of the science are properly equipped with a working knowledge of the principles of physics and chemistry, the books will cease to be a heterogeneous mass of undigested material mingled with inferences as to the meaning of the phenomena, which to any one familiar with the principles of physics and chemistry are often ludicrous. From the above point of view, it might be said that the problem of geology, the problem of problems, is to get men who write geological papers and books so well trained in the elements of the sciences upon which geology is based that they shall be able to reason correctly as to physical and chemical causes, and consequently to observe and describe accurately and discriminatingly. It is plain that the geologist who hopes to advance the principles of his science must have a working knowledge of physics and chemistry.[1]

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[1] C. R. Van Hise, *Training and Work of a Geologist, Proceedings of the American Academy of Sciences*, vol. LI (1902), pp. 399–420.

Principles of Geology the same for the Entire Earth

The phenomena of geology for any extensive area — for instance, a continent — are so numerous that, had the science originated in Europe, in America, and in Asia independently, the principles of the science developed in these three regions would have been essentially the same. The chief differences would have been that the emphasis placed upon the different principles would have varied. Also the principles of certain divisions of the subject would have been somewhat more fully developed in one case than in another. For instance, because of differences in the range of latitude and other climatic conditions, certain parts of the principles of physiography would have been more fully developed on one continent than on another.

It is, of course, understood that the foregoing statements premise that men of equal ability and attainments had been at work on the problems of geology in the various continents. This supposition is, of course, erroneous, for it is evident that the great constructive work of geology has been done largely by a comparatively few individuals. Indeed, the contrast between nations in the number of creative geologists which they have produced is so great that it is a fair inference that the differences in the principles of the science developed in the three continents under the conditions named would have been more largely due to difference in the capacity of the geologists than to variation in the phenomena demanding explanation. In geology, as in other lines of human endeavor, the exceptional man, the genius, is a factor of paramount importance.

The Problems of Provinces and Districts

Thus far we have considered only the development of the principles of geology. They are applicable to the entire earth. There is another great field of geology, which has not yet been suggested, — the application of the principles to provinces and districts.

This second line of problems of geology is illustrated by such subjects as the stratigraphy of a given district, its physiography, its paleontology, etc. The working-out of the stratigraphy, or physiography, of a given county or township may be of great importance to the inhabitants of that county or township, or even of some consequence to the nation. They are, however, of much less importance to persons interested in the advancement of the principles of geology, unless their elucidation adds to the science some new principle, or some unusually fine illustration of an old principle.

The principles of geology may be broadly comprehended by a single individual. No individual can be familiar with more than a minute fraction of the applications of the principles to the numerous geological provinces of the world. Scarcely a score of years ago it was possible for a geologist not only to know the developed principles of the science, but to know somewhat fully the facts upon which those principles were based. At the present time this is impossible. A man may know the more important facts in reference to a few districts, the broader facts in reference to states, and some of the more general facts in reference to an entire continent, or even more than one continent; but no man can know more than an inappreciable portion of the geological facts of even the countries which have been somewhat closely studied; and these countries comprise but a small part of the earth.

But it is unnecessary for a man to know all the facts of geology. He need only know the more important facts for a sufficiently broad region so that he may understand the recognized principles of the science, assist in their development, and take part in the discovery of new principles. The discoveries will be found to be largely applicable to the vastly greater regions of the world which are not considered by the discoverer. All this is very fortunate for the science of geology. A student beginning the subject may fully comprehend the truthfulness of many principles which have been developed in various parts of the world through the illustrations furnished by his native parish.

From the foregoing it appears that the geology of the future is to have two aspects, which, as time goes on, will become more and more clearly differentiated: first, the principles of geology; second, the application of principles to various parts of the world.

Conclusion

It is clear that the evolution of the science of geology has followed a strictly natural course. Before the subject was recognized as a science, the earth was being observed. When man turned to nature-study, he began to observe the phenomena exhibited by the earth, such as the stratification of the rocks, and the presence in them of objects which are called fossils. After such observations were made, it was inevitable that sooner or later the question should arise as to the manner in which the results observed were accomplished. Thus the observation of phenomena led to a study of processes. Sands like those observed in a consolidated form were seen in the process of building. The conclusion followed that the consolidated stratified rocks were formed by the processes observed upon the seashore. Seashells were seen to be produced by animals and to be deposited with the upbuilding sands. This led to the explanation that the fossils in the sedimentary rocks were due to the processes observed.

After a large number of explanations, the methods of which were the same as in the illustrations given, the general doctrine was evolved that the geological results of the past are to be explained by present processes, or the present is the key to the past. While the above conclusions now seem almost axiomatic, we need not go far back to find them astonishing novelties. So far as we are aware, the natural explanation of fossils was first reached by that amazingly versatile genius, Leonardo da Vinci, in the fifteenth century. The conclusion that the present is the key to the past required for its formulation the intellect of the great Hutton.[1] It was not announced until 1785, and the doctrine was not generally accepted until after Lyell's *Principles* appeared in 1830.

As the science of geology developed, the practice of explaining the phenomena in terms of processes gradually became more common, until, as we have seen, it is dominant in the latest geological text-book. But, as already intimated, the analysis of processes in terms of energy, force, and agent has only begun. It is my belief that at some time in the future a text-book of geology will appear which shall begin with a discussion of the energies, forces, and agents of geology, the understanding of which is necessary in order adequately to comprehend processes. It has been stated that the problem of geology is the reduction of the science to order under the principles of physics and chemistry. This is equivalent to saying that the problem of geology is the discussion of the subject in terms of energies, forces, agents, processes, and results. Such a discussion will constitute the principles of geology.

It is my deep-seated conviction that by the solution of this problem only can geology be so simplified as to be comprehended with reasonable fullness by the human mind. When this work is done, the broad principles of the science will be capable of statement with comparative simplicity and brevity. But so broad and complex is the science of geology that a comprehensive statement of the principles of the entire subject will necessarily be somewhat voluminous.

Supplementary to the principles of geology, which are applicable to the entire earth, there will be a long series of volumes of the geology of different continents, the various political divisions of these continents, the states under those divisions, or even the minor areas, such as counties or townships; for so numerous are the facts of the science that it requires a volume to discuss in detail even a small area. For instance, to give the geology of a township with sufficient fullness to make clear the earth-story there illustrated may require a good-sized volume.

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[1] How clearly the great Hutton appreciated the doctrine commonly called that of uniformity is shown by the following quotations from his *Theory of the Earth:* "In what follows, therefore, we are to examine the construction of the present earth, in order to understand the natural operations of time past; to acquire principles, by which we may conclude with regard to the future course of things, or judge of those operations by which a world so wisely ordered goes into decay; and to learn by what means such a decayed world may be renovated, or the waste of habitable land upon the globe repaired." The concluding sentence of his work is: "The result, therefore, of our present inquiry is, that we find no vestige of a beginning — no prospect of an end." Charles Hutton, *Theory of the Earth, Philosophical Transactions of the Royal Society of Edinburgh* (1785), p. 218; *ibid.*, p. 304.

We have seen that geology rests upon physics and chemistry as its foundation; that it is closely related upon one side to astronomy, upon another side to botany; that in its broader sense it includes mineralogy; and that for its satisfactory development the aid of the higher mathematics is needed. It is evident that the man who is to advance geology must be broadly trained in science, and have a firm grip upon the nature of energy, ether, and matter, and their interactions.

It is my conviction that when geology is placed in order under the principles of physics and chemistry the science will have passed through a greater revolution than at any previous time in its history.

SECTION C — PALEONTOLOGY

SECTION C — PALEONTOLOGY

(*Hall* 11, *September* 22, 10 *a. m.*)

Chairman: Professor William B. Scott, Princeton University.
Speakers: Dr. A. S. Woodward, F.R.S., British Museum of Natural History, London.
Professor Henry F. Osborn, Columbia University.
Secretary: Director John M. Clarke, State Museum, Albany, N. Y.

THE RELATIONS OF PALEONTOLOGY TO OTHER BRANCHES OF SCIENCE

BY ARTHUR SMITH WOODWARD

[**Arthur Smith Woodward,** Keeper of the Geological Department, British Museum. **b.** Macclesfield, England, May 24, 1864. Educated in Macclesfield Grammar School; Owens College, Manchester. (Hon.) LL.D. Glasgow. Connected with the British Museum since 1882, occupied with researches in extinct vertebrata, especially fishes. Received Wollaston Fund from the Geological Society of London, 1889; Lyell Medal, *ibid.* 1896. Fellow of the Royal Society of London; also of the Linnean, Zoölogical, Geological, and Royal Geographical Societies; Member of Société Belge de Géologie; New York Academy of Sciences; Boston Society of Natural History; Secretary of Paleontographical Society. **Author of** *Catalogue of Fossil Fishes in British Museum; Outlines of Vertebrate Paleontology;* and various memoirs and papers in scientific journals.]

The satisfactory interpretation of fossils depends on knowledge of so many kinds that it is not surprising the study of them was scientifically pursued for nearly half a century before it received a distinctive name. Even after paleontology had been added to the roll of the sciences, the universities still regarded it as a department of geology, zoölogy, or comparative anatomy. In fact, to this day there is no separate ordinary chair of paleontology in any of the European universities, and there are very few chairs devoted to this science even in the more progressive universities of America. It is the general custom for the professor of geology to treat the invertebrate fossils, with special reference to their use in determining the age of rocks; while the professor of zoölogy or comparative anatomy usually includes the vertebrate fossils in his course, to supply some of the many links which are missing in the surviving chain of life. Under such circumstances, there is no difficulty in recognizing that paleontology is intimately related to other geological and biological sciences. The obstacle to a correct appreciation of the subject is rather that the divided teaching fails to impart to the student any adequate idea of its fundamental broad principles and their true meaning.

Relations to Geology

It is quite natural that paleontology should still be regarded as a subsidiary part of geology, for it developed from the study of the so-called "figured stones" and "mineral conchology," which were so much discussed more than a century ago. It is based entirely upon fossils, which lose much of their real value unless they are carefully collected by a geologist; and the fossils themselves can only be properly understood by one whose eye is accustomed to the examination of rocks and mineral structures. Moreover, it has been quite clear since the days of William Smith, Cuvier, and Brongniart that fossils always occur in a definite order in the rocks of different ages, so that they afford a means of correlating the formations of widely separated localities whose mutual relationships are otherwise uncertain. To use Mantell's well-known phrase, they are therefore "medals of creation," and an intimate knowledge of them is absolutely essential to a geologist when he attempts to determine the relative age of sedimentary deposits which he cannot directly observe in superposition.

The researches of paleontologists during the last two decades, however, have considerably amplified the original conception of fossils as an index to geological time. So long as detailed observations were mainly confined to one small portion of the earth's surface, it was possible to enumerate a few characteristic genera for each stratum of rock; and when geological discoveries began to be made in distant countries, it was found that the general succession of fossil groups of animals was always the same — that graptolites and trilobites, for example, were invariably older than ammonites, and that these again always preceded the volutes. At the present day a skilled paleontologist can determine the age of a fauna with much greater precision. The broad outlines of the evolution of most groups of animals have now been ascertained; and when a new set of fossils is discovered in a hitherto unknown formation, the paleontologist does not occupy himself so much with the search for familiar genera as with an inquiry into the stage of evolution of the various groups represented.

This has been pointed out by many authors, but none have stated the case more clearly than Gaudry, who has devoted special attention to the mammalia.[1] The warm-blooded quadrupeds or mammals began as little small-brained animals, each with a continuous series of bluntly-cusped teeth round the edge of the mouth, with flattened vertebræ, and with five toes on each foot. A group of fossil remains representing only such animals would be referred to the Eocene Tertiary; and if some of the species had grown to bulky proportions and developed horns, the fauna might be described without hesitation as Middle or Upper Eocene. Groups of mammals progressively differing from this original race in (1) the larger size of the brain, (2) special adaptation of the teeth to flesh-tearing or vegetable-grinding, (3) greater mobility of the neck, and (4) adaptation of the feet either to grasping prey or to running on hard ground, mark successive geological periods. The general succession is always the same whatever may be the local circumstances; and for this reason it is impossible to accept the published conclusions of the brothers Ameghino as to the age of the various mammal-bearing Tertiary deposits of Patagonia. The mammals of South America are certainly anomalous, but the marine fossils intercalated between some of the deposits containing bones in Patagonia prove that the rate of mammalian evolution was much the same there as in other lands. Even Australia, which is in many respects a remnant of the Mesozoic world, can be readily recognized by its mammals as modern Tertiary. The monotremes are certainly a very ancient type, but their large brains, peculiar skulls, and rudimentary or lost teeth show that they belong to a far later period than that at which their lowly tribe flourished. Similarly the kangaroos have highly specialized teeth and feet which cannot be misinterpreted.

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[1] A. Gaudry, *Essai de Paléontologie Philosophique* (1896), pp. 178–197.

Relations to Cosmical Physics

While fossils prove that the succession of life during geological time has been essentially the same everywhere, it is still impossible to determine exactly which faunas were contemporaneous in different parts of the world. A deposit containing Carboniferous fossils, for example, in Australia was not necessarily formed at precisely the same time as a rock yielding similar fossils in the Arctic regions. There may have been migration, and the Carboniferous animals and plants may have quitted the Australian region long before they reached the Arctic Circle, or *vice versa*. To obviate the use of the word "contemporaneous" in referring to such a case, Huxley long ago proposed the more indefinite adjective "homotaxial," which postulates nothing more than the identity of two rocks in their fossil contents; and there is at present no prospect of dispensing with this provisional term. It is therefore unfortunate, but true, that paleontology gives only very uncertain information about the distribution of heat over the surface of the globe in past ages. It is perfectly clear from fossils that climates have changed in nearly all if not all parts of the world; it is not equally evident how these changes of climate in different regions were related to each other.

Fossils, however, can only be used as tests of climate with special caution. When, by analogy with the existing world of life, a whole fauna agrees with the associated whole flora in indicating certain climatic conditions, the mean temperature under which it flourished is doubtless approximately determinable. When the evidence is less nearly complete, it can hardly be satisfactory. To appreciate this, it is only necessary to remember that a fossil elephant, a rhinoceros, and a tiger have been found in undoubted glacial deposits in the arctic regions; while the hippopotamus is represented by abundant remains in the Pleistocene river-gravels of England, which were deposited under a by no means warm clime. Even in the case of plants, there is the oft-quoted occurrence of palms at the present day in the neighborhood of glaciers in New Zealand.

Allowing for such difficulties and uncertainties, the general inference to be deduced from all the available evidence of fossils is, perhaps, that until the end of the Mesozoic period the difference of mean temperature between the various latitudes was much less than it is at present. Paleontology suggests, indeed, that the polar ice-caps were comparatively insignificant until the latter half of the Tertiary period. Fossils of many ages, indicating at least a temperate climate, have long been known within the Arctic Circle;[1] and similar discoveries have just begun in the ice-bound Antarctic regions. The Swedish Antarctic expedition has brought back from Louis-Philippe Land in S. lat. 63° 15′ a series of Jurassic ferns, cycads, and conifers, which, according to Professor Nathorst,[2] might have been collected in the Inferior Oolite of the Yorkshire coast. The same expedition has also obtained remains of ferns, conifers, and dicotyledons from a Tertiary formation in S. lat. 64° 15′. In this case, however, the fossils were found in a marine deposit and may possibly have been drifted for a long distance. As remarked by Professor Nathorst,[3] "The dredgings of Dr. Agassiz have proved that a mass of leaves, wood, and fruits may occur at the bottom of the sea even at a distance of more than 1000 kilometers from the nearest land." Hence it must be left for future discoveries to decide whether or not the Tertiary Antarctic plants actually grew in the latitude where they were found.

While thus of some value in indicating ordinary climatal changes, fossils do not date back far enough to be considered in relation to any of the fundamental problems of cosmogony. It has been ingeniously argued[4] that life must have originated at the poles because those regions cooled first; and some authors have maintained that even during the Tertiary period fossils prove the land within the Arctic Circle to have been the main centre from which successive new types of life arose and dispersed. The oldest known fossils, however, occur in rocks at the base of the Cambrian series, both in the tropics and in the far north; and there is as yet no means of determining whether the animals represented by these fossils spread from the north, south, or equatorial regions, or from several points. There is thus no direct evidence from fossils for or against the theory of the polar origin of life. The facts supposed to show that the same area continued to be a source of new organisms even until the later Tertiary period admit of other interpretations which are in better accord with the newest discoveries.

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[1] J.W. Gregory, *Some Problems of Arctic Geology*, *Nature*, vol. 56 (1897), pp. 301–303, 351, 352.

[2] A. G. Nathorst, *Sur la Flore fossile des Régions antarctiques*, *Comptes Rendus*, vol. 138, pp. 1447–1450 (June 6, 1904).

[3] *Loc. cit.* p. 1450.

[4] G. Hilton Scribner, *Where did Life begin?* (ed. 2, New York, 1903).

Even of changes which may have occurred since the globe became habitable, fossils furnish no reliable indications. Professor G. H. Darwin's theory of the former magnitude of the tides is as completely unsupported by paleontology as by geology. The idea that the earth's atmosphere has gradually altered in constitution since life began is equally destitute of support from fossils. The microscopical structure of the leaves of the Carboniferous plants suggests that even at so remote a period as that when they flourished, the air was essentially identical with that of the present day, without any superfluity of carbon dioxide or anything to obstruct the full influence of the sun's rays.[1]

Relations to Geography

So far as can be judged at present, paleontology justifies the assumption that each type of animal or plant has only originated once and from one set of ancestors. Fossils can therefore be used as an aid to the solution of geographical problems. If a more or less sedentary group of animals is found to be essentially identical in two widely separated seas, it may be reasonably assumed either that those seas were once connected, or that they received their life from a common source. Similarly, if two distant tracts of land are inhabited by the same animals and plants, and there is no possibility at present of migration between these two regions, a former connection either with each other or with a common centre may also be postulated. The same is true in reference to all periods of the earth's history, and hence the varying distribution of fossils at different epochs affords a clue to the successive changes in the disposition of lands and seas, gradually culminating in their present arrangement.

For instance, it has been lately noticed[2] that the mollusca living on the two opposite coasts of the North Pacific during the Pliocene period were much more nearly identical than they are at the present day. In other words, the coast-line seems to have been continuous at that time, a neck of land uniting Asia and North America where now there exists the Bering Strait. The Pliocene land-animals of the northern hemisphere agree in suggesting the same connection. Hence, the ultimate separation of the so-called Old and New Worlds is shown by fossils to be quite a modern event in geological history.

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[1] A. C. Seward, *Fossil Plants as Tests of Climate* (*Sedgwick Prize Essay*, 1892), pp. 71–76.

[2] R. Arnold, *The Paleontology and Stratigraphy of the Marine Pliocene and Pleistocene of San Pedro, California*. Memoirs of the Calif. Acad. Sci., vol. III (1903).

Again, it has been proved by recent researches [1] that the mollusks, brachiopods, and trilobites found in the Devonian rocks of South Africa, agree much more closely with those occurring in the corresponding formations of South and North America than with those of Europe. The South African sea in the Devonian period seems therefore to have extended directly into the American region, but to have been separated by a barrier from the European region. Similarly, there is evidence of circumscribed seas separated by land-barriers in the Triassic, Jurassic, and other epochs; and when the fossils from all parts are sufficiently well known, it will be possible to determine even some of the minor geographical features of each successive period.

To restore the old continents and to discover their varying connections and disintegrations is an especially fascinating problem. A means of solution is provided by the various terrestrial vertebrates, which, under ordinary circumstances, are unable to cross seas. When a new race suddenly appears in any land, it obviously implies the removal of the barrier which previously prevented that race from spreading. The primitive elephants, for example, suddenly invaded Europe at the beginning of the Miocene period. Recent discoveries in the Egyptian desert have proved that their ancestors lived and evolved in the Eocene and Oligocene periods in northern Africa.[2] Therefore, during this earlier time, the European and African regions were separated by some barrier, doubtless the sea; at the dawn of the Miocene period earth-movements of some kind resulted in a land connection over which mammals could migrate.

The use of terrestrial vertebrates in deciphering the past history of continents is, however, less simple than it may at first sight appear; and the case of South America may be quoted as an interesting illustration. With reference to the latest phases in the development of this land, only two main conclusions are well founded. The first fact to notice is that, of the jaguars, pumas, wolves, bears, tapirs, deer and llamas, which now characterize South America, and of the mastodons and horses which lived there in the Pleistocene period, there are no remains in the geological formations of that country below the top of the Pliocene. Hence, as representatives of all these quadrupeds lived at an earlier date in North America, there must have been some

[1] F. R. C. Reed and P. Lake, *Ann. S. African Museum*, vol. iv, pts. 3, 4, 6 (1903–04).

[2] C. W. Andrews, "On the Evolution of the Proboscidea," *Phil. Trans.* 1903, on. B. 217.

barrier, evidently a sea, which separated the northern and southern parts of America during the greater part of the Tertiary epoch, and only disappeared to allow the free migration of land animals towards the close of the Pliocene period. The removal of this barrier, which is also indicated by purely geological researches, resulted thus in a mingling of the native South American Tertiary fauna with a host of invaders, whose ancestors flourished on the lands of the northern hemisphere. In other words, the surviving land animals of South America have been derived from two sources — some from the native stock which evolved in the country itself during the Tertiary epoch, some from the late Pliocene invasion of northern life. Now, the native stock just mentioned is of uncertain origin, but in its prime it included the New World monkeys, many peculiar rodents, the sloths, anteaters, and armadillos, and numerous remarkable hoofed animals — altogether an assemblage unknown in any other region of the world. Therefore, it seems impossible to escape from the further conclusion that during the greater part of the early Tertiary epoch South America was an isolated land, and its mammals developed independently of those of other continents. On the other hand, it is to be observed that during part of this time there lived in South America several primitive carnivorous animals, perhaps marsupials, which were most strikingly similar to the thylacines and dasyures of the Australian region. There was also a horned land-tortoise, *Miolania*, essentially identical with one of which species occur in the Pleistocene deposits of Australia and Lord Howe's Island. Finally, there was the familiar mud-fish, *Ceratodus*, which now survives only in the Queensland rivers. It has therefore been thought that the occurrence of remains of these animals among the Tertiary fossils of South America favors the theory of a former land-connection between that country and Australia. In fact, they are sometimes quoted as helping to confirm the hypothesis of the former existence of a great Antarctic continent, which has broken up into the lands now known as Australia, New Zealand, South Africa, and South America. The surviving thylacines of the Australian region, however, are the very slightly altered descendants of a race of small-brained, primitive carnivores, which are known to have lived throughout the northern hemisphere, and were probably cosmopolitan at the beginning of the Tertiary epoch. The Middle Tertiary carnivores of South America and the modern thylacines of Australia may, therefore, be merely the last survivors of an effete race, which was exterminated early at all points except the two extremes of its once extensive range. Similarly, *Miolania* is a horned and armored member of a suborder of tortoises (*Pleurodira*), which was probably almost as nearly cosmopolitan at the end of the Mesozoic and beginning of the Tertiary epoch as is the suborder of Cryptodiran tortoises in the existing world.

Ceratodus was also universally distributed in the waters, probably even in the seas, of the middle part of the Mesozoic epoch. So that in each of these three cases Australia and South America may be merely refuges for old forms of life which were lost much earlier by extinction in other parts of the world. They need not have been directly connected.

In short, when using land animals or fresh-water animals as tests of former changes in the distribution and connection of land areas, it is necessary to make a distinction between those of restricted range and those of past or present cosmopolitan distribution, the former alone affording reliable evidence.

Relations to Biology

It is already clear that the scientific value of a fossil depends upon the exactness with which the circumstances of its discovery are determined by a geologist. The briefest experience is also enough to demonstrate that the well-mineralized remains of an organism can only be satisfactorily interpreted by an observer who is familiar with the structure of rocks and their common constituents. The student of fossils needs as much elementary training in the geological succession of the rocks and the varied nature of mineralization as the student of histology and embryology requires to locate his sections with exactitude, and to understand the action of the different stains and media he employs. In the one case nature makes the preparation; in the other case the processes of laboratory technique are responsible for the difficulties. In both cases, there is scope for numerous fantastic conclusions if the properties of the preservative medium are misunderstood.

Paleontology, however, is essentially a department of biology, and it can only be prosecuted with success by a skilled biologist, who has had the elementary geological and mineralogical experience just mentioned. It bears, indeed, the same relation to the whole world of life that embryology bears to the structure of an individual organism. The one deals with the rise and growth of races and their varying relationships; the other describes and interprets the evolution of an individual and the processes by which the different parts of its mechanism are finally adjusted. Both unfortunately depend on extremely imperfect material; for fossils are nearly always mere badly preserved skeletons, and they represent only an infinitesimal fraction of the life that has passed away, while embryos are so much adapted to the peculiar circumstances of their environment that many of the essential stages in their growth and development are obscured and modified by temporary expedients.

The past history of the world of life, as revealed by fossils, has long been familiar in its general outlines. At least a century has elapsed since it was made clear that the various organisms come into existence at different times and in a definite order, according to their grade in the scale of being, the lowest first, the highest latest. Several decades have also passed away since it was recognized that within each group the lowest or most generalized members appeared earliest, the highest, most specialized, or most degenerate towards the end of the race. Modern research is concerned only with the details of this succession, and with the laws which can now be deduced from the rapidly multiplying available facts.

Our present knowledge of the geological succession of the fishes may be briefly summarized to show how paleontology contributes to the solution of the fundamental problems of biology. The earliest recognizable fish-like organisms, which occur in Upper Silurian formations, seem to have been mere grovelers in the mud of shallow seas, nearly all with incompletely formed jaws and no paired fins, devoting most of their growth-energy to the production of an effective armor by the fusion of dermal tubercles into plates (*Ostracodermi*). With them were a few true fishes which had completed jaws, but which possessed a pair of lateral fin-folds, variously subdivided, instead of the ordinary two pairs of fins (*Diplacanth Acanthodii*). The main features of Silurian fish-life were, therefore, the acquisition of dermal armor, definite jaws, and the beginning of paired fins. Some of the lowly types thus equipped survived and further evolved in the Devonian period; but the multitude of new-comers which then formed the majority were much higher in the scale of being (*Crossopterygii*). They were still adapted for the most part to live on the bottom of shallow water or in marshes, but they were typical well-formed fishes in respect to their jaws, branchial apparatus, and two pairs of fins. Nearly all their bones were external, very little of their internal skeleton being ossified; and the only changes they seem to have been undergoing related to the fusion of some of the head-bones and the more exact adaptation of their fins and tail to their environment. Fishes more fitted for sustained swimming were also beginning to appear, and these (*Palæoniscidæ*) formed the large majority in the succeeding Carboniferous and Permian periods. They were about equivalent in grade to the modern sturgeons, and the tendency towards change in their structure was in the direction of effective swimming, by the more intimate correlation between the fin-rays and their supports, and by the shortening of the upper lobe of the tail. They still exhibited scarcely any ossification of the internal skeleton. As soon as the best type of balancing-fin and the most effective type of propelling tail-fin had become universal among the highest fish-life of the Triassic period, the internal skeleton began to ossify and vertebral centra arose. In fact, the whole of the succeeding Jurassic period was spent by the highest fishes in improving and finishing their internal skeleton, while their external bony armor began almost universally to degenerate. Thus, by the early part of the Cretaceous period the most advanced members of the class had already become true bony fishes or *Teleosteans*. Having attained that stage of complexity they admitted of much more variation than formerly, and then arose the immense host of fishes which characterize the Tertiary period and the present day. For the first time in fish-history, there were fundamental changes in the head. First, in some genera the maxilla began to slip behind and above the premaxilla, so that it was excluded from the gape. Next, in these and most other fishes, the ear-capsules began to enlarge to such an extent that the original roof of the brain-case eventually formed only an insignificant part of the top of the skull. At the same time the lateral muscles of the trunk extended forward over the cranial roof, and various crests arose between them. Finally, it was quite common for the pelvic fins to be displaced forward beneath the pectoral fins, while the vertebræ, as well as some of the fin-rays, were usually reduced to a definite and fixed number for each family or genus. Simultaneously, many of the fin-rays were modified into spines, and there was a constant tendency for the external bones and scales to become spinose. At all stages of this progress there were, of course, stragglers left by the way; and the modern fish-fauna is, therefore, a mixture of slightly modified survivors of many periods in the earth's history.

To state this brief summary in more general terms, fossils prove that the earliest known fish-like organisms strengthened their external armor so long as they remained comparatively sedentary; that next the most progressive members of the class began to acquire better powers of locomotion, and concentrated all their growth-energy on the elaboration of fins; that, after the perfection of these organs, the internal bony skeleton was completed at the sacrifice of outer plates, because rapid movement necessitated a flexible body and rendered external armor less useful; that finally, in the highest types, the vertebræ and some of the fin-rays were reduced to a fixed and practically invariable number for each family or genus, while there was a remarkable development of spines. As survivors of most of these stages still exist, the changes in the soft parts which accompanied the successive advances in the skeleton can be inferred. Hence, paleontology furnishes a sure basis for a natural classification in complete accord with the development of the group.

Now, fishes are aquatic animals and nearly all the fossiliferous rocks were deposited in water. The past history of this chain of life ought therefore to be almost completely revealed by the geological records. Making due allowance for the imperfection of collections and the accidental nature of the discovery of fossils, the general outlines of this history may indeed be considered as tolerably well ascertained. Thus the facts of paleontology not only aid the biologist in discovering the true relationships of the fishes; at the same time they afford a definite means of determining with certainty some of the fundamental principles of organic evolution illustrated by them. As identical principles may be deduced from other departments of paleontology, most of them are not likely to be altered in any essential respects by future discoveries.

It must suffice here to allude only to a few of these general results which seem to be of far-reaching importance, omitting details which may be obtained from special treatises. Foremost among them is the demonstration that the evolution of the animal world has not proceeded uniformly but in a rhythmic manner. As soon as fishes had acquired the paddle-shaped paired fins, they suddenly became the special feature of the Devonian period in all parts of the globe that have hitherto been geologically examined, and they attained their maximum development, being more numerous and more diverse in form than at any subsequent time. None of these paddle-finned fishes (*Crossopterygii*) in the course of their varied development made much approach towards passing into the next grade of fish-life with short-based paired fins and a heterocercal tail (*Chondrostei*); but among their earliest representatives there was at least one member of the higher group, which suggests that the latter arose when the previous group was just becoming vigorous. At the beginning of the Carboniferous period the higher grade of fish-life just mentioned suddenly became the dominant feature, and during the Carboniferous and Permian it attained its maximum development. Towards the close of the Permian period the next higher group was heralded by only one representative, but as soon as it arose in the Trias it resembled its predecessors in becoming immediately dominant, surpassing all contemporary races of fishes both in the number of individuals and in the variety of genera and species. In the Cretaceous period the highest bony fishes appeared, and at the end of that period, with the dawn of the Tertiary, they suddenly diverged into nearly all the subdivisions which characterize the existing fish-fauna, accomplishing much more evolution in a brief interval than has taken place during the whole of the succeeding Tertiary time. In short, the fundamental advances in the grade of fish-life have always been sudden and begun with excessive vigor at the end of a long period of apparent stagnation; while each advance has been marked by the fixed and definite acquisition of some new character — an "expression point," as Cope termed it — which seems to have rendered possible, or at least been an essential accompaniment of, a fresh outburst of developmental energy. As we have seen, the successive "expression points" among fishes were the acquisition of (1) paddle-like paired fins, (2) shortened fin-bases but persistent heterocercal tail, (3) completed balancing-fins and homocercal tail, and (4) completed internal skeleton.

When fossils are examined more closely, it is interesting to observe that the geological record is most incomplete exactly at these critical points in the history of each race. There are abundant remains of the families and genera which are definitely referable to one or other order or suborder; but with them there are scarcely any of the links between these major divisions which might have been expected to occur. It must also be confessed that repeated discoveries have now left faint hope that exact and gradual links will ever be forthcoming between most of the families and genera. The "imperfection of the record," of course, may still render some of the negative evidence untrustworthy; but even approximate links would be much commoner in collections than they actually are, if the doctrine of gradual evolution were correct. Paleontology, indeed, is clearly in favor of the theory of discontinuous mutation, or advance by sudden changes, which has lately received so much support from the botanical experiments of H. de Vries.

Further results obtained from the study of fossils have a bearing even on the deepest problems of biology, namely, those connected with the nature of life itself. For instance, it is allowable to infer, from the statements already made, that the main factor in the evolution of organisms is some inherent impulse — the "bathmic force" of Cope — which acts with unerring certainty, whatever be the conditions of the moment. So far as human judgment can decide, the varied assemblage of fishes at each stage of the earth's history was always in perfect accord with its environment, and displayed very few signs of waning, even at the time when a new race suddenly took its place and provided every kind of fish once more on a higher plane or, so to speak, in a later fashion. The change was inevitable and according to some fundamental law of life whose influence is independent of temporary equilibrium. Equally inevitable and irreversible are the essential changes which may be observed during the evolution of each family of organisms. As the late Professor Beecher pointed out,[1] all animals with skeletons tend to produce a superfluity of dead matter which accumulates in the form of spines as soon as the race to which they belong has passed its prime and begins to be on the down grade; all vertebrates tend to lose their teeth when they reach the culmination of their life-history; nearly all groups of fishes end their career with eel-shaped representatives; and when a structural character has been definitely lost in the course of evolution it never reappears, but, if actually wanted again, is reproduced in a secondary makeshift. Finally, and perhaps most important of all, there is in the course of evolution of all groups of animals to their prime a tendency towards fixity in the number and regularity (or symmetry) in arrangement of their multiple parts. The assumption of a fixed number of vertebræ and fin-rays in the latest and highest families and genera of bony fishes has already been mentioned. An irregular cluster of grinding teeth characterized the Pycnodont fishes of the Lower Lias, while these teeth began to be disposed in definite regular rows in some of the Bathonian forms, and such a symmetrical arrangement henceforth pervaded the highest members of the family. Many of the lower vertebrates, both living and extinct, have teeth with multiplied cusps, and in some genera the number of teeth seems to be constant; but in the history of the vertebrates the tooth-cusps never became fixed individual entities, strictly homologous in whole races, until the highest or mammalian grade had been attained. Moreover, it is only in the same latest phase that the teeth themselves can be treated as definite units, always the same in number (44), except where modified by degeneration or special adaptation. The number of vertebræ in the neck of the lower vertebrates depends on the extent of this part, whereas in the mammal it is almost invariably seven, whatever the total length may be. Equally constant in the artiodactyl ungulate mammalia is the number of nineteen vertebræ between the neck and the sacrum.

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[1] C. E. Beecher, *The Origin and Significance of Spines*, *Amer. Journ. Sci.* [4] vol. VI (1898), July to October.

In short, the biologist equipped with an adequate knowledge of paleontology cannot fail to perceive that throughout the evolution of the organic world there has been a periodical succession of impulses, each introducing not only a higher grade of life but also fixing some essential characters that had been variable in the grade immediately below. He must also realize that in the interval between these impulses some minor characters in the families similarly acquired fixity in their prime, until old age and extinction approached. The general conclusion is, that if the unknown influence which Cope has termed "bathmic force" were able to act without a succession of checks from the environment and Natural Selection, animals would form much more symmetrical groups than we actually find, and their ultimate grades would display still more instances of numerical fixity in multiple parts than can be observed under existing circumstances.

This result almost tempts a paleontologist to risk the pitfalls of reasoning from analogy, and to compare organic evolution with some purely physical processes. It has already been pointed out more than once that the initial stages of animal races resemble the nascent states of chemical elements in their particular intensity of vigor and unwonted susceptibility to influence; while Cope himself has hinted that the "expression points" in the evolution of races may perhaps be compared with the phenomena of latent heat in the organic world. It now seems reasonable to add that each "phylum," or separate chain of life, bears a striking resemblance to a crystal of some inorganic substance, which has been disturbed by impurities during its growth, and has thus been fashioned with unequal faces or even turned partly into a mere concretion. In the case of a crystal, the inherent forces act solely upon molecules of the crystalline substance itself, collecting them and striving even in a disturbing environment to arrange them in a fixed geometrical shape. In the case of an organic phylum, the inherent forces of the colloid germ-plasm act upon a consecutive series of temporary outgrowths or excrescences of colloid substance (the successive individual bodies or "somata"), struggling not for geometrically arranged boundaries, but towards various other symmetries and a fixity in number of multiple parts. Paleontology thus contributes to biology by placing the oft-repeated comparison of life with crystallization in an entirely new light.

Relations to Sociology

It is to be noticed that when the extreme of bodily evolution had been reached by the production of a mammal, the final real advance in the world of life was a gradual increase in the effectiveness of the controlling nervous centre or brain. Then, for the first time in the history of the globe, brain rather than bodily state determined the survival of the fittest. In fact, it is clear that mental attributes have slowly arisen in obedience to the same laws which controlled the advance of the animal frame itself. Such being the case, it is not surprising that the highest use of these attributes by man should result in the arrangement of communities and methods of advancement which strictly conform to the laws discovered by the paleontologist. As Herbert Spencer, indeed, has well said, "All social phenomena are phenomena of life — are the most complex manifestations of life — must conform to the laws of life — and can be understood only when the laws of life are understood." In other words, the study of fossils has a distinct bearing on the problems of sociology.

The general resemblance between the evolution of human communities and animal groups is not difficult to perceive in any direction. In the progress of every nation there are clearly-marked periods of brilliance between others of comparative stagnation, corresponding with the rhythmic advance already described as observable among animals. At each period the real mental work and influence which lead to the next stage of progress are accomplished by a competent mediocrity, however much they may be consummated at intervals by the appearance of a guiding genius; in fact, the generalized rather than the specialized members of a community are the real groundwork of the future. Moreover, history seems to teach that every nation, on reaching its prime, begins to display within itself the elements which lead to decline or extinction; so that it completes a definite life-cycle with an inevitable end. Indeed, even in smaller matters, it is often not difficult to express sociology in the terms of paleontology. Newberry, for example, long ago pointed out that the evolution of warfare between human communities corresponds exactly with that between fishes in the course of their long history — the first tendency being towards protection by thickening the armor until a maximum is reached, when this method is abandoned and skillful movement gradually supersedes it. Other examples might be cited, and will readily occur to any one who is familiar with the details of the past history of any group of organisms. It must, however, suffice now merely to conclude by emphasizing a remark made at the outset, that these wider aspects of the subject can only be fully appreciated when paleontology is taught and learned as an independent branch of knowledge.

THE PRESENT PROBLEMS OF PALEONTOLOGY

BY HENRY FAIRFIELD OSBORN

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I congratulate myself that it has fallen to my lot to set forth some of the chief contemporary problems of paleontology, as well as to make an exposition of the prevailing methods of thought in this branch of biology. At the same time I regret that I can cover only one half of the field, namely, that of the paleontology of the vertebrates. From lack of time and of the special knowledge required to do a great subject justice I am compelled to omit the science of invertebrate fossils and the important biological inductions made by the many able workers in this field. There is positively much in common between the inductions derived from vertebrate and invertebrate evolution, and I believe a great service would be rendered to biology by a philosophical comparison and contrast of the methods and results of vertebrate and invertebrate paleontology.

The science of vertebrate fossils is in an extremely healthy state at present. The devotees of the science were never more numerous, never more inspired, and certainly never so united in aim, as at present. We have suffered some heavy personal losses, not only among the chiefs, but among the younger leaders of the science in recent years; Cope, Marsh, Zittel, Kowalevsky, Baur, and Hatcher have gone, but they live in their works and in their influence, which vary with the peculiar or characteristic genius of each.

As in every other branch of science, problems multiply like the heads of hydra; no sooner is one laid low than a number of new ones appear; yet we stand on the shoulders of preceding generations, so that if our philosophical vision be correct, we gain a wider horizon, while the horizon itself is constantly expanding by discovery.

In discovery the chief theatre of interest shifts from continent to continent in an unexpected and almost sensational manner. In 1870, all eyes were centred on North America, and especially on Rocky Mountain exploration; for many ensuing years, new and even unthought-of orders of beings came to the surface of knowledge, revolutionizing our thought, firmly establishing the evolution theory, and appearing to solve some of the most important problems of descent. Then the stage shifted to South America, where an equally surprising revelation of unthought-of life was made. We were in the very midst of the more thorough examination of this Patagonian and Pampean world when the scene of new discovery suddenly changed to North Africa, — previously the "dark continent" of paleontology, — and again a complete series of surprises was forthcoming. Each continent has solved its quota of problems and has aroused its quota of new ones. Now we look to Central and South Africa, to the practically unknown Eastern Asia, and possibly to a portion of the half-sunken continent of Antarctica, for a future stock of answers and new queries.

Rapid exploration and discovery, however, are not the only symptoms of health in a science; we do not aim to pass down to history as great collectors; we must accumulate conceptions and ideas as rapidly as we accumulate materials; it will be a reproach to our generation if we do not advance as far beyond the intellectual status of Cuvier, Owen, Huxley, and Cope as we advance beyond their material status in the way of collections of fossils. We must thoroughly understand where we are in the science, how we are doing our thinking, what we are aiming to accomplish; we must grasp, as the political leader, Tilden, observed, the most important things, and do them first.

Paleontology a Branch of Biology

Let us first cut away any remaining brushwood of misconception as to the position of paleontology among the sciences. I do not wish to quarrel with my superior officers, but I must first record a protest against the fact that in the classification scheme of this Congress, in the year of our Lord 1904, paleontology is bracketed as a division of geology. It is chiefly an accident of birth which has connected paleontology with geology; because fossils were first found in the rocks, geology, the foster mother, was mistaken for the true mother, zoölogy — a confusion in the birth-records which Huxley did his best to correct. The preservation of extinct animals and plants in the rocks is one of the fortunate accidents of time, but to mistake this position as indicative of scientific affinity is about as logical as it would be to bracket the Protozoa, which are principally aquatic organisms, under hydrology, or the Insecta, because of their aërial life, under meteorology. No, this is emphatically a misconception which is still working harm in some museums and institutions of learning. Paleontology is not geology, it is zoölogy; it succeeds only in so far as it is pursued in the zoölogical and biological spirit.

In order to make clear the special rôle of paleontology among the biological sciences, and at the same time the grateful services which it is enabled to render to its foster science, geology, as well as to geography, when pursued in a purely biological spirit, let us employ an imaginary problem. Figure to yourselves a continent absolutely unknown in any of its physical features of earth, climate, or configuration; let us imagine that from such an unknown continent all the animals and all the plants could be brought into a vast museum, the only condition being that the latitude and longitude of each specimen should be precisely recorded, and let us further imagine a vast number of investigators of the most thorough zoölogical and botanical training, and with a due share of scientific imagination, set to work on this collection. Such an army of investigators would soon begin to restore the geography of this unknown continent, its fresh, brackish, and salt-water confines, its seas, rivers, and lakes, its snow peaks, its glaciers, its forests, uplands, plains, meadows, and swamps; also even the cosmic relations of this unknown continent, the amount and duration of sunshine as well as something of the chemical constitution of the atmosphere and of the rivers and seas. Such a restoration or series of restorations would be possible only because of the wonderful fitness or adaptation of plants and animals to their environment, for it is not too much to say that they mirror their environment.

At the historic period commemorated by this great exposition of St. Louis, when Napoleon concluded to sell half a continent to strengthen his armies, it is true that such a solution of a physical problem by biological analysis might have been conceived by the pupils of Buffon, by Napoleon's great contemporaries, Cuvier, Lamarck, or Saint-Hilaire, but the solution itself would not have been possible. It has been rendered possible only by the wonderful advance in the understanding of the adaptation of the living to the lifeless forces of the planet. Finally, it is obvious in such a projection of the physical from the purely biological that the degree of accuracy reached will represent the present state of the science and the extent of its approach toward the final goal of being an exact or complete science. The illustrative figure need not be changed when the words paleozoölogy and paleobotany are substituted for zoölogy and botany. We still read with equal clearness the physical or environmental changes of past times in the biological mirror, a mirror often unburnished and incomplete owing to the interruptions in the paleontological records, but constantly becoming more polished as our knowledge of life and its all-pervading relations to the non-life becomes more extensive and more profound.

Such an achievement as the reconstruction of a continent would be impossible in paleontology pursued as geology or as a logical subdivision of geology. The importance of the services which paleontology may render geology as time-keeper of the rocks, or which geology may render paleontology, are so familiar that we need not stop to enumerate them. To emphasize the relation I have elsewhere suggested the phrase, *Non paleontologia sine geologia.* With other physical sciences paleontology is hardly less intimate; from the physicist it demands time for the evolution of successive waves of organisms, from the geographer it demands continental connections or even whole continents for the passage of land-animals and plants. As with geology, what it receives it is ever ready to return in gifts; the new branch of geography, for example, entitled paleogeography, appeals quite as often to the paleontologist as to the geologist for its data.

Problem of the Origin of Fitness

Naturally the central thought of paleontology as biology is the origin of fitness as the property which above all others distinguishes the living from the non-living. Here the paleontologist enjoys the peculiar advantage of being present at the birth of new characters and watching the course of their development; and to this advantage is attached the peculiar responsibility of observing the birth and course of development of such characters with the utmost accuracy and a mind free from prejudices in favor of any particular hypothesis, with full acquaintance with the phenomena of evolution as they present themselves to the zoölogist, the botanist, and the experimentalist, and with the philosophical temper which will put every hypothesis to the test of every fact. The laughing remark of Cope on seeing a newly discovered specimen which controverted one of his hypotheses, "If no one were watching I should be glad to throw that fossil out of the window," has a serious reality in our often unconscious protection of our own opinions.

The birth of new characters is the crucial point in the origin of fitness. With Darwin [1] himself, with Cope,[2] with Bateson,[3] we do not regard the Darwinian law of selection as the creative or birth-factor; by its very terms it operates after there is something of value to select. Forgetting this distinction, some naturalists are so blind as to fail to see that selection is still the supreme factor in evolution in the sense that it produces the most grand and sweeping results as well as the most inconspicuous results in the organic world. Certain of the creative factors cannot be seen at all by paleontologists; others, in my opinion, cannot be seen by zoölogists.

Before looking farther into the creation of fitness, let us clear away another misconception which happens to be of paleontological origin, although paleontologists are not responsible for it. It concerns the history of one of the great theories of the day. Many years ago, Waagen,[1] a German paleontologist, observed that the varieties or minor changes in time (chronological varieties) differ from varieties in space (geographical varieties); that the latter have a variable value and are of small systematic importance, while the former are very constant, and, though seen only in minute features, may always be recognized again. These varieties in time Waagen termed *mutations*. In 1891 Scott[2] unearthed this distinction of Waagen's and clearly defined it as the hereditary or phylogenetic change of animals in time. Previous to this, Osborn,[3] without knowing of Waagen's statement, had discussed the same facts of the birth of new characters, describing them as "definite variations." Cope, it happens, did not follow this line of thought at all; but many other paleontologists did, notably Hyatt,[4] whose peculiar style and multiplicity of terms obscured his depth of thought and extent of observation. Thus the term *mutation* acquired a definite significance among paleontologists.

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[1] Charles Darwin, *The Origin of Species*, 1859; *The Variation of Animals and Plants under Domestication*, 1868.

[2] E. D. Cope, *The Primary Factors of Organic Evolution*, Chicago, 1896; *The Origin of the Fittest*, 8vo, New York, 1887.

[3] Wm. Bateson, *Materials for the Study of Variation*, London and New York, 1894.

It happened that De Vries,[5] the eminent Dutch botanist, reading Scott's paper, mistakenly identified these new characters succeeding each other in time with those which he was observing as occurring contemporaneously in plants, and he adopted Waagen's term for the "mutation theory," which he has so brilliantly set forth, of the sudden production of new and stable varieties, from which nature proceeds to select those which are fit.

If paleontologists are correct in their observation, mutations may be figured graphically as an inclined plane, whereas De Vries's phenomena in plants represent a series of steps more or less extensive. Scott expressly excluded the element of discontinuity; and I believe there is no ground whatever for the assertion that the phenomena first named mutations by Waagen, and independently observed by many paleontologists, are identical with the phenomena observed by De Vries in plants.

On the contrary, De Vries's facts accord with the favorite hypothesis of Saint-Hilaire. They demonstrate the law of *saltation*. This is the inevitable interpretation of the expositions of De Vries himself, of

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[1] W. Waagen, *Die Formenreihe des Ammonites subradiatus, Benecke's Paleont. Beiträge*, vol. II, 1869, pp. 179–257.

[2] W. B. Scott, *On the Mode of Evolution in the Mammalia, Journal of Morphology*, vol. V, 1891, no. 3, p. 387; *On Variations and Mutations, American Journal of Science*, vol. XLVIII, Nov. 1894, pp. 355–374.

[3] H. F. Osborn, *The Paleontological Evidence for the Transmission of Acquired Characters; American Naturalist*, vol. XXIII, 1889, p. 562.

[4] A. Hyatt, *The Genesis of the Tertiary Species of Planorbis . . . Memoirs*, Boston Society of Natural History, 1883; *The Genesis of the Arietidae*, 1889; *Lost Characteristics, American Naturalist*, vol. XXX, 1896, pp. 9–17.

[5] Hugo De Vries, *Die Mutationstheorie*, Bd. I, 1901, Bd. II, 1903; *Species and Varieties, Their Origin by Mutation*, ed. by D. T. MacDougal, 8vo, Chicago and London, 1905. Since this address was given, De Vries points out that the botanists have a prior claim, since Godron used this term before Waagen.

Hubrecht,[1] and of the more recent references of Bateson [2] in his British Association address. That saltation is a constant phenomenon in nature, a *vera causa* of evolution, no one can longer deny. Bateson shows that it harmonizes with Mendel's conceptions of heredity, and it may be regarded as *par excellence* the contribution of the experimental method.

Similarly, I regard mutation as a quite distinct phenomenon, and as *par excellence* the contribution of the paleontological method; it is the gradual rise of new adaptive characters neither by the selection of accidental variations nor by saltation, but by origin in an obscure and almost invisible form, followed by direct increase and development in successive generations until a stage of actual usefulness is reached, where perhaps selection may begin to operate. While clearly setting forth the difficulties, I at one time attributed definite variation or mutation to Lamarck's principle of the inherited effects of habit as the only assignable cause; subsequently I realized that it was not explainable by the Lamarckian hypothesis.

I then attributed it to an unknown law of evolution, and there I believe it rests to-day, namely, as a process of which we do not know the cause. Still more recently, however, comes the discovery that original kinship is, partly at least, a control-principle. For example, in the descent of independent stocks of hornless animals arising from a common stock, rudimentary horn-cores are found to appear independently in exactly the same region of the skull, indicating a kind of predetermination in the stock, or potential of similar evolutions. The facts on which this law of mutation, properly called, rests have been misunderstood, totally denied, or explained away by selectionists as survivals of favorable out of indiscriminate variations. Even my colleague, Scott, has identified these phenomena with the saltations of De Vries. Nevertheless, I regard the genesis of new adaptive characters from almost imperceptible beginnings as a *vera causa*, and as one of the greatest problems we have to solve.

That a natural solution will be found goes without saying, although this principle, as stated, is undoubtedly of a teleological nature. Its philosophical bearings are of far-reaching importance. Just as we demand a continent to transfer land animals from Australia to South America, so we demand a natural law to explain these facts.

The creative factors of fitness coöperating with selection, which, in my judgment, are now well demonstrated, reside either primarily in the environment, in the bodies of animals, or in the germinal cells — they all ultimately find their way into the germinal cells. They may be summarized as follows:

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[1] A. A. W. Hubrecht, *Hugo De Vries's Theory of Mutations*, *The Popular Science Monthly*, vol. LXV, no. 3, July, 1904, pp. 205–228.

[2] W. Bateson. See Report of the British Association for the Advancement of Science, Cambridge meeting, 1904; (with Miss E. R. Saunders) *Reports* to the Evolution Committee of the Royal Society, 1902. [Gives summary of Mendel's life and work.]

[3] H. F. Osborn, *Recent Zoöpaleontology*, *Science*, 1905, N. S. vol. XXI, no. 530, pp. 315–316.

(1) *Segregation.* Besides the familiar geographical segregation of animals, which reaches its highest expression in insular forms, such as the pygmy fossil elephants of Malta [1] and those recently discovered in Cyprus (Bate),[2] there is the no less effective *segregation of habit* among animals existing in the same geographical regions and under the same climatic conditions, but seeking different varieties of food on different kinds of soil. These give rise to what I have called local adaptive radiations, a principle which explains the occurrence in the same country, and almost side by side, of very conservative as well as very progressive forms.

(2) *Adaptive Modification.* This is a plastic principle which tends in the course of life to an increasing fitness of the bodies of individuals to their special environments and habits, well illustrated among men in the influence of various trades and occupations and operating both in active and in passive structures. Consistent with the adaptive modification principle is the fact that every individual requires habit and environment to model it into its parental form; and in every change of environment or habit every individual is carried an infinitesimal degree beyond the parental form; the wonderful phenomena of correlated development which puzzled Spencer so much are chiefly attributable to this principle. These adaptive modifications are not directly inherited, as Lamarck supposed, but acting through long periods of time there results the *organic selection* (Morgan,[3] Baldwin,[4] Osborn[5]) of those individuals in which hereditary predisposition happens most closely to coincide with adaptive modification, and there thus finally comes about an apparent, but not real, inheritance of acquired characters, as Lamarck, Spencer, and Cope supposed.

(3) *Variations of Degree.* We should by no means exclude as true causes of evolution associated with both the above factors the selection of those variations of degree or around a mean which conform to Quetelet's curve, the subject of the chief investigations of the Galton

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[1] L. Adams, *On the Dentition and Osteology of the Maltese Fossil Elephants*, *Transactions* of the Zoölogical Society, vol. ix, pt. i, 1874.

[2] Dorothy M. A. Bate, *Further Note on the Remains of Elephas Cypriotes*, *Philosophic Transactions* of the Royal Society, London, ser. B, vol. 197, 1904, pp. 347–360.

[3] C. Lloyd Morgan [*Organic Selection*], *Science*, Nov. 27, 1896.

[4] J. Mark Baldwin, *A New Factor in Evolution*, *American Naturalist*, June and July, 1896; *Development and Evolution*, 8vo, New York, 1902.

[5] H. F. Osborn, *A Mode of Evolution requiring neither Natural Selection nor the Inheritance of Acquired Characters* (*Organic Selection*), *Transactions* of the New York Academy of Science, March and April, 1896, pp. 141–148; *The Limits of Organic Selection*, *American Naturalist*, Nov. 1897, pp. 944–951; *Modification and Variation and the Limits of Organic Selection*, *Proceedings*, American Association for the Advancement of Science, 46th meeting, 1898, pp. 239–242.

school, of Pearson [1] and of Weldon, and which form the strongest remaining ground for Darwin's theory of selection in connection with fortuitous variation. For example, I regard the appearance of long-necked giraffes, of slender-limbed ruminants and horses, of long-snouted aquatic vertebrates, as instances of the selection of variations around a mean rather than of the selection of saltations. The selection of such variations, where they happen to be adaptive, has been an incessant cause of evolution.

(4) *Saltation.* Although Geoffroy Saint-Hilaire [2] argued for paleontological evolution by saltation, I do not think we have much evidence in paleontology for the saltation theory. In the nature of the case, we cannot expect to recognize such evidence even where it may exist, because, wherever a new form appears or a new character arises, as it were, suddenly, we must suspect that this appearance is due to absence of the connecting transitional links to an older form. The whole tendency of paleontological discovery is to resolve what are apparently saltations or discontinuities into processes of continuous change. This, however, by no means precludes saltation from being a *vera causa* in past time, as rising from "unknown" causes in the germ-cells and as forming the materials from which nature may select the saltations which are adaptive from those which are inadaptive. The paleontologist has every reason to believe that he finds saltations in the sudden variations in the number of vertebræ of the neck, of the back, of the sacral region, for example. In the many familiar cases of the abbreviation or elongation of the vertebral column in adaptation to certain habits, a vertebra in the middle of a series cannot dwindle out of existence; it must suddenly drop out or suddenly appear.

(5) *Mutation.* These new characters are also germinal in origin, because they appear in the teeth, which are structures fully formed beneath the surface before they pierce the gum, and therefore not subsequently modeled by adaptive modification, as the bones, muscles, and all the other tissues of the body are. Mutations are found arising according to partly known influences of kinship. They do not, so far as we observe, possess adaptive value when they first appear, but then frequently, if not always, develop into a stage of usefulness.

Fitness is, therefore, the central thought of modern paleontology in its most comprehensive sense, as embracing fitness in the very remote past, in its evolution toward the present, and in its tendencies for the future. Just as the uniformitarian method of Lyell transformed geology, so the uniformitarian method is penetrating paleontology and making observations of animal and plant life as it is to-day the basis of the understanding of animal and plant life as it was from the beginning. Here again paleontology is not merely an auxiliary to zoölogy; it is chief of a division and enjoys certain unique advantages. We pass in review, with the pedigrees and the prodigies of fitness, the entirely unreasonable, irrational, paradoxical extremes of structure, such, for example, as the pterosaurs, which far surpass in boldness and ingenuity of design any of the creations of the modern yacht-builder which are mistakenly regarded by some as having reached an absurd extreme.

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[1] K. Pearson. See articles in *Proceedings* of the Royal Society of London, and in the *Grammar of Science*, London, 1900.

[2] Geoffroy Saint-Hilaire, *Recherches sur des grands Sauriens trouvés à l'état fossile*, *Mémoires* de l'Académie des Sciences, Paris, 1831; *Influence du monde ambiant pour modifier les formes animales*, *Mémoires* de l'Académie des Sciences, XII, 1833, p. 63.

Problem of Historical Study

The paleontologist must also be an historian; he has to deal with lineage, with ancestors, he comes directly upon the problem of kinship or relationship, and he has to determine the various means of distinguishing the true from the apparent relationships. It happens that fitness, while fascinating in itself, has led even the most faithful and skillful into the most devious paths away from the truth. The explanation of this apparent contradiction is in this wise. The ingenuity of nature in adapting animals is astounding, but it is not infinite; the same devices are resorted to repeatedly to accomplish the same purposes. In the evolution of long-snouted rapacious swimming forms, for example, we have already discovered that nature has repeated herself twenty-four times in employing the same processes to accomplish the same ends in entirely different families of animals.

This introduces us to one of the two great ideas which we must employ in the interpretation of facts, namely, the *idea of analogy*. We see far more clearly than Huxley did the force of this idea. Owen,[1] Cope,[2] Scott,[3] Fraas, and many others, under the terms "parallelism," "convergence," "homoplasy," have developed the force of the old Aristotelian notion that analogy is a similarity of habit, and that in the course of evolution a similarity of habit finally results in a close or exact similarity of structure; this similarity of structure is mistaken as an evidence of kinship. Analogous evolution does not stop in its far-reaching consequences with analogies in organs; it moulds animals as a whole into similar form, as, for example, the ichthyosaurs, sharks, and dolphins; still more it moulds similar and larger groups of animals into similar lines or radii of specialization. Thus we reach the grand idea of analogy as operating in the divergencies or adaptive radiations of groups, according to which great orders of animals tend in their families and suborders to mimic other orders, and the faunæ or collective orders of continents to mimic the faunæ of other continents.

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[1] R. Owen, *On the Nature of Limbs*, London, 1849; see also *Homology* in *The Anatomy of Vertebrates*, vols. I–III, 1866–68; see also *Homology, Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals*, London, 1843.

[2] E. D. Cope, *Primary Factors of Organic Evolution*, 8vo, New York, 1887.

[3] W. B. Scott, *On some of the Factors in the Evolution of the Mammalia*, . . . *Journal of Morphology*, vol. V, no. 3, 1891, pp. 379–402.

Amid this repetition on a grand scale of similar adaptations, which is altogether comparable to what we know as having occurred over and over again in human history, the paleontologist as an historian must keep constantly before him the second great *idea of homogeny,* of real ancestral kinship, of direct blood descent and hereditary relationship. The shark and the ichthyosaur superficially look alike, but their germ-cells are radically different, their external resemblances are a mere veneer of adaptation, so deceptive, however, that it may be a matter of half a century before we recognize the wolf beneath the clothing of the sheep, or the ass in the lion's skin.

These two great ideas, of analogy, or similarity of habit, and homogeny, or similarity of descent, do not run on the same lines; they are the woof and the warp of animal history. Analogy corresponds to the woof, or horizontal strands, which tie animals together by their superficial resemblances in the present; homogeny to the warp, or the fundamental vertical strands which connect animals with their ancestors and their successors. The far-reaching extent of analogous evolution was only dimly perceived by Huxley, and this fact constituted his one great defect as a philosophical anatomist. Its power of transforming unlike and unrelated animals has accomplished miracles in the way of producing a likeness so exact that the inference of kinship is almost irresistible.

The paleontologist who would succeed as historian must first, therefore, render himself immune to the misguiding influences of analogy by taking certain further precautions which will now be explained by watching his procedure as historian.

Paleontology as the history of life takes its place in the background of recorded history and archeology, and simply from the standpoint of the human pedigree is of transcendent interest. Although it has progressed far beyond the dreams of Darwin and Huxley, the first general statement which must be made is that the actual points of contact between the grand divisions of the animal and plant kingdom, as well as between the lesser and even many of the minor divisions, have yet to be discovered. You recall that the older grand divisions of the Vertebrata, to which we must confine our attention, were suggested by the so-called Ages of Fishes, of Amphibians, of Reptiles, and of Mammals. Even within these grand divisions we observe a succession of more or less closely analogous groups. Each of these groups has its more or less central starting-point in a smaller and older group which contains a large number of primitive or generalized characters.

The search for the primitive central form is always made by the same method of reasoning, a method which was first clearly outlined by Huxley, namely, by the more or less ideal reconstruction of the primitive central form from which radiation has occurred. This is a very difficult matter where the primitive central form is not preserved either living or as a fossil. In such instances we may by analysis of all the existing forms prophesy the structure of the primitive central form, as Huxley,[1] Kowalevsky,[2] and Cope [3] did in the case of the hoofed animals, a prophecy which was nearly fulfilled by the discovery in northern Wyoming of *Phenacodus*. In other more fortunate cases the primitive central form survives both living and fossil, as in the remarkable instance of *Paleohatteria* of the Permian and the tuatera lizard (*Hatteria*) of New Zealand, which gave rise to the grand adaptive radiation of the lizards, mosasaurs, dinosaurs, crocodiles, phytosaurs, and probably of the ichthyosaurs.

In the reconstruction of these primitive central forms, we must naturally discriminate between analogy and homogeny, and paleontologists are not agreed in all cases on such discrimination. On the border region, in fact, where the primitive central forms are still unknown, where analogy has reached its most perfect climaxes and imitations, are found the great paleontological controversies of to-day. For example, among the paleozoic fishes, the armored ostracoderms (*Pteraspis*, *Cephalaspis*, *Pterichthys*) and the arthognaths (*Coccosteus*, *Dinichthys*) by some authors (Hay,[4] Regan,[5] Jaekel[6]) are placed in the single group of placoderms, while by other authors (Smith Woodward [7] and Dean [8]) they are regarded as entirely independent and superficially analogous groups. The dipnoi, or lung fishes (*Ceratodus*, *Protopterus*), present so many analogies with the amphibians (salamanders and frogs) that they were long regarded as ancestors of the latter; but more searching anatomical and paleontological analyses and recent embryological discoveries have proved that the dipnoi and amphibia are parallel analogous groups descended alike from the crossopterygian fishes, fishes which are now represented only by the bichir (*Polypterus*) of Africa. It is interesting to recall parenthetically that two naturalists, Harrington, an American, and Budgett, an Englishman, have given their lives to the solution of this problem in searching for the embryology of *Polypterus*. The latter explorer only was successful.[1]

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[1] T. H. Huxley, *The Anniversary Address of the President*, *Quarterly Journal of the Geological Society*, London, vol. XXVI, 1870.

[2] Kowalevsky, *Osteology of the Hyopotamidae*, *Philosophic Transactions*, 1873, p. 69; *Versuch einer natürlichen Classification der fossilen Hufthiere* (*Monographie der Gattung Anthracotherium Cuv.*) *Paleontographica*, N. F., II, 3 (XXII), 1873.

[3] E. D. Cope, *On the Homologies and Origin of the molar teeth of the Mammalia Educabilia*, *Journal* of the Academy of Natural Sciences, Philadelphia, March, 1874, pp. 20, 21.

[4] O. P. Hay, *Bibliography and Catalogue of the Fossil Vertebrata of North America*, *Bulletin* of U. S. Geological Survey, no. 179, 1902, p. 253. [N. B. Dr. Hay includes with the Arthrodira the Antiarcha alone of the Ostracoderms.]

[5] C. Tate Regan, *The Phylogeny of the Teleostomi*, *Ann. and Mag. Natural History*, ser. 7, vol. XIII, May, 1904, pp. 340–346.

[6] O. Jaekel, *Ueber Coccosteus und die Beurtheilung der Placodermen*, *Sitzungs Ber. d. Ges. Naturforchender Freunde*, Berlin, Jahrg. 1902, no. 5.

[7] A. Smith Woodward, *Outlines of Vertebrate Paleontology*, Cambridge, 1898, p. 64, 3.

[8] Bashford Dean, *The Devonian Lamprey Paleospondylus, with Notes on the Systematic Arrangement of the Fish-like Vertebrates*. *Memoirs*, New York Academy of Sciences, vol. II, pt. I, 1900, p. 22 *et seq*.

Missing Links between the Great Classes of Vertebrates

Among the varied fins of the crossopterygians we have nearly, but not actually, discovered the prototype of the hand and the foot, the fingers and toes, of the primordial amphibian. Volumes upon volumes have been written by embryologists and comparative anatomists on the hypothetical transformation of the fin into the hand.[2] Considering the supreme value of the hand and foot in vertebrate history, this was certainly the most momentous transformation of all and worthy of volumes of speculation; but as a matter of fact, the speculation has been a total failure, and this problem of problems will only be settled by the future discovery in Devonian rocks of the actual connecting link, which will be a partly air-breathing fish, capable of emerging upon land, in which the cartilages of the fin will be found disposed very much as in the limbs of the earliest Carboniferous amphibians. The unity of composition in the hand and the foot points to an original similarity of habit in the use of these organs.

This missing point of contact, or of the actual link between amphibians and fishes, is equally characteristic of paleontology as history from the top to the bottom of the animal scale. We are positive that amphibians descended from fishes,[3] probably of the crossopterygian kind, but the link still eludes us; we have brought the reptiles within close reach of the amphibians,[4] but the direct link is still to be found; mammals are in close proximity to a certain order of reptiles,[5]

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[1] J. Graham Kerr, *The Budgett Memorial* (1) Note on the Developmental Material of Polypterus obtained by the late Mr. J. S. Budgett. *Reports* of British Association for the Advancement of Science, Section D, Cambridge, 1904, p. 29.

[2] Wiedersheim [Parker, W. N.]. See the bibliography of Fins and Limbs in Wiedersheim's *Elements of the Comparative Anatomy of Vertebrates*, translated by W. N. Parker, 1897. See also Gill, *Homologies of Anterior Limbs*, *Science*, N. S. xvii, March 27, 1903, p. 488.

[3] A. Smith Woodward, *Outlines of Vertebrate Paleontology*, Cambridge, 1898, pp. 123–125.

[4] Baur, Case, Cope, Osborn, Broili, F. See various papers on the Cotylosauria (Pareiasauria) by Baur, Case, Cope. For a summary of their relations see Osborn, *The Reptilian Subclasses Diapsida and Synapsida*, *Memoirs*, American Museum of Natural History, vol. i, pt. viii, Nov. 1903, p. 456. Especially, Broili, *Stammreptilien*, *Anatomischer Anzeiger*, xxv, No. 23, 1904, pp. 577–587.

[5] H. F. Osborn. For a summary see *The Anomodontia*, *Reptilian Subclasses Diapsida and Synapsida*, *Memoirs*, American Museum of Natural History, vol. i, pt. viii, Nov. 1903, pp. 460–466; *The Origin of the Mammalia*, *American Naturalist*, vol. xxxii, May, 1898, pp. 309–334. See various papers on the Anomodontia by Owen, Seeley, Cope, Baur.

R. Broom. [Many recent papers by Broom on the Theriodontia and their but the connecting form is still undiscovered; man himself is not far from the various types of anthropoid apes,[1] but his actual connecting relationship is unknown.

We are no longer content, however, with these approaches to actual contact and genetic kinship, we have toiled so long both by discovery and by the elimination of one error after another, and are so near the promised land, we can hardly restrain our impatience. I venture to predict that the contact of the amphibia with the fishes will be found either in America or Europe. No such prediction could safely be made regarding the connecting form between the amphibians and reptiles, because America, Eurasia, and Africa all show in contemporaneous deposits evidence that such connection may be discovered at any time. The transformation from reptiles to birds will probably be found in the Permian of America or Eurasia; chances of connecting the mammals with the reptiles are decidedly brightest in South Africa; while in Europe, or more probably in Asia, we shall connect man with generalized catarhine primates.

Passing from these larger questions of the relations of the great classes of vertebrates to each other, let us review the problems arising in the individual evolution of the classes themselves.

Geographical Problems

The primordial, solid-skulled, or stegocephalian amphibia of the Permian diverged into a great variety of forms which wandered over Eurasia and North America so freely that, for example, we find as close a resemblance between certain Würtemberg and New Mexican genera (*Metopias*) as between the existing stag of Europe and the wapiti deer. Which branch of these primordial amphibians gave rise to the modern frogs and salamanders we do not know. This and hundreds of similar facts suggest the vital importance of paleogeography.

As regards paleogeography, the great induction can be made that, throughout the whole period of vertebrate evolution, and until comparatively recent times, Europe, Asia, and North America constituted one continent and one life-region, or Arctogæa (Huxley, 1868, Blanford, 1890), with which the continents of the southern hemi-

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Allies.] *Annals* of the South African Museum, *Records* of the Albany Museum (Cape Colony), *Proceedings* of the Zoölogical Society of London (especially 1901), vol. II, pp. 162–190, 1904; vol. I, pp. 490–498.

[1] See the immense literature on *Pithecanthropus erectus*, Dubois. Especially: E. Haeckel, *The Last Link, Our Present Knowledge of the Descent of Man*, London, 1898, 156 pp.; *On Our Present Knowledge of the Origin of Man, Annual Report*, Smithsonian Institute, 1898, pp. 461–480; *Anthropogenie*, 2 vols. Leipzig, 1903, pp. 992.

A. H. Keane, *Man, Past and Present*, Cambridge University Press, xv, 1899, 548 pp.; *Ethnology*, University Press, 1900.

sphere, namely, Africa, South America, and Australia, were intermittently, but not continuously connected by land. A great southerly continent, Notogæa (Huxley, 1868), connected with a south polar Antarctica, now submerged, is a theory very widely supported by zoölogists[1] and, I believe, by botanists, although its existence is still denied by certain geographers (Murray). We find Permian, Jurassic, late Cretaceous, and early Tertiary proofs of Antarctica in the fresh-water crustaceans (Ortmann), in fresh-water fishes (Gill), in littoral mollusca (Ortmann), in reptiles (Smith Woodward and Osborn), in birds (Forbes and Milne Edwards), in worms (Beddard), in the Australian animals (Spencer), in the fossil mollusca of Patagonia (Ortmann), and in the fossil mammals of Patagonia (Ameghino). To marshal and critically examine all this evidence and convert this most convenient Antarctic hypothesis into an established working theory I consider one of the most pressing problems of the day.

Problem of the Source of the Reptiles and Mammals

Returning from this geographical détour to paleontology as history, we should first note that already in the Permian there was developed such an astonishing variety and differentiation of the reptiles that we must look to future discoveries in the Carboniferous to find the actual points of descent of reptiles from the amphibia. These Permian and Lower Triassic reptiles[2] are of three kinds, comparable to a parent (Cotylosauria) and two offspring (Anomodontia and Diaptosauria). In the parent group (the Cotylosauria, or solid-skulled reptiles), we find so many fundamental similarities to the Stegocephalia, or solid-skulled amphibia, that only by the possession of many parts of the body can we surely distinguish reptile from amphibian remains. The primordial reptile was probably altogether a land animal continuously using its limbs in awkward progression, bringing forth its young by land-laid eggs and probably possessing gills only as vestiges. These cotylosaurs show very wide geographical distribution, South Africa, Siberia, Great Britain, and North America, and equally remarkable adaptive radiations of habit into small and large, horned and hornless types, some of which were certainly dying-out branches, while others led into the two offspring groups.

Leaving this parental order, in the Permian and Lower Trias, we first see in the older offspring, the Anomodontia, reptiles of varied size and description, carnivorous and herbivorous in habit, most abundantly found in South Africa, in Asia, and in Europe, and not at all as yet in America, either North or South. The high degree of fitness for different habits, or radiation, of the anomodonts is distinguished from that of any other reptiles at any time by its numerous analogies to the radiation of the mammals, namely, into very large and very small forms, into carnivorous and herbivorous, into terrestrial and possibly into aquatic types; in fact, some of these animals, if seen on land to-day, might readily be mistaken for mammals.

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[1] A. E. Ortmann, *The Theories of the Origin of the Antarctic Faunas and Floras*, *American Naturalist*, vol. 35, Feb. 1901, pp. 139–142.
H. F. Osborn, *Science*, N. S., vol XI, April 13, 1900, pp. 564–566.

[2] K. Zittel [C. R. Eastman]. *Text-book of Paleontology*, vol. II, London, 1902, pp. 179–187, translated by Eastman. See Notes 4, 5, pp. 575, 576.

The second offspring of the Cotylosauria, on the contrary, the Diaptosauria, are essentially and unmistakably saurians; that is, if seen about us to-day they would undoubtedly at first be described as lizards. They were still more broadly cosmopolitan in range, being scattered over both Americas (Pelycosauria, Proganosauria), Europe (Protorosauria, Rhynchosauria), Asia (Rhynchosauria), and Africa (Proganosauria, Rhynchosauria). They are also found highly diversified in type, but all their analogies of fitness are with the reptiles and not with the mammals. It is of prime importance that more of these diaptosaurs be found, and that those already known in the museums should be more critically examined. What we already know, however, enables us to establish the following facts: first, that the parentage of these animals is more probably among the cotylosaurs than among the anomodonts, and second that already in the Permian they had formed a sufficiently large number of branches to be regarded as a fully evolved radiation.

Problem of the Adaptation of the Mesozoic Reptiles

In the Triassic the offspring of the anomodonts and of the diaptosaurs appear as the third generation from the cotylosaurs.

The recurrent difficulty arises that the actual points of contact or transition from the anomodonts are wanting, and we must continue to reason by the ideal reconstruction of the hypothetical linking forms. Such reasoning connects the Testudinata (turtles and tortoises), the Sauropterygia, or marine plesiosaurs, and, singularly enough, our own ancestors, the primordial mammals, with the group of anomodonts, and not at all with that of the diaptosaurs. Here in the Upper Permian and Lower Trias we must await both discovery and the closest critical analysis, but if this still hypothetical affiliation be confirmed by discovery, as I personally am sanguine it will be, then it will be true to say that the mammals, and hence man, are much more nearly affiliated to the anomodonts than to either the lizards or snakes, which are both on the great Diaptosaur branch. Our presence on the great anomodont branch and remoteness from the creeping and crawling reptiles will perhaps afford some consolation to those who still shrink from the ultimate consequences of

Darwin's *Descent of Man.* As regards degrees of probability, it must be said that while the affiliation of the Plesiosaurs and Testudinata with the anomodont group still requires confirmation, the connection of the mammals with certain anomodonts (Theriodontia) [1] is not only probable, but is almost on the verge of actual demonstration, and at present it seems likely that the Karoo Desert of South Africa will enjoy the honor of yielding the final answer to the problem of the *origin of mammals*, which has stirred comparative anatomists for the last sixty years.

Turning to the progeny of the other branch, the Permian diaptosaurs, we find them embracing (with the exception of the Testudinata and plesiosaurs) not only vast reptilian armies, marshaling into thirteen orders, mastering the distinctive Age of Reptiles (Triassic, Jurassic, and Cretaceous), and surviving in the four existing orders of lizards, snakes, crocodiles, and tuateras, but we also find them giving off the birds as their most aristocratic descendants.[2] The bold conception of the connection between these thirteen highly diversified orders and a simple ancestral form of diaptosaur, typified by the Permian *Palæohatteria* or the surviving *Hatteria* (tuatera of New Zealand) we owe chiefly to the genius of Baur,[3] a Bavarian by birth, an American by adoption. Absolutely diverse as these modern and extinct orders are, whatever material for analysis we adopt, whether paleontological, anatomical, or embryological, the result is always the same, — the reconstructed primordial central form is always the little diaptosaurian lizard. The actual lines of connection, however, are still to be traced into the great radiations of the Mesozoic.

The chief impression derived from the survey of this second branch of the reptiles in the Mesozoic as a whole is again of radiations and subradiations from central forms and the frequent independent evolution of analogous types. The aquatic life had been already chosen by the plesiosaurs and by some of the turtles, as well as by members of three diaptosaur orders (Proganosauria, Choristodera, certain Rhynchocephalia), two of which were surviving in Jurassic times. Yet it is independently again chosen by four distinct Triassic orders, always beginning with a fresh-water phase (Parasuchia, Crocodilia), and sometimes terminating in a high-sea phase (Ichthyosauria, Mosasauria, Crocodilia).[4] In the Jurassic period there

[1] H. F. Osborn, *Reclassification of the Reptilia*, *American Naturalist*, Feb. 1904, pp. 93–115. For the Diaptosauria, see Osborn on *The Reptilian Subclasses Diapsida and Synapsida*, *Memoirs*, American Museum of Natural History, vol. I, pt. VIII, Nov. 1903, p. 467 *et seq.*

[2] H. F. Osborn, *Reconsideration of the Evidence for a Common Dinosaur-Avian Stem in the Permian*, *American Naturalist*, vol. XXXIV, no. 406, Oct. 1900, pp. 777–799.

[3] G. Baur, *On the Phylogenetic Arrangement of the Sauropsida*, *Journal of Morphology*, vol. I, Sept. 1887, pp. 99–100.

[4] E. Fraas, *Die Meer-Crocodilier* (*Thalattosuchia*) *des oberen Jura*, *Paleontographica*, Bd. XLIX, Stuttgart, 1902.

were altogether no less than six orders of reptiles which had independently abandoned terrestrial life and acquired more or less perfect adaptation to aquatic life. Nature, limited in her resources of outfitting for aquatic life, fashioned so many of these animals into like form, it is small wonder that only within the last two years have we finally distinguished all the similarities of analogous habit from the similarities of real kinship.

The most conservative members of this second branch are the terrestrial, four-footed, persistently saurian or lizard-like forms, the tuateras and the true lizards; but from these types again there radiated off one of the marine orders (Mosasauria),[1] the limbless snakes (Ophidia), while the lizards themselves have in recent times diverged almost to the point of true ordinal separation.

The most highly specialized members of this second branch are, of course, the flying pterosaurs, of whose ancestry we know nothing. Also in a grand division by themselves there evolved the dinosaurs, distinctively terrestrial, ambulatory, originally carnivorous, and probably more or less bipedal animals. Not far from the stem of the dinosaurs was also the source of the birds, also distinguished by bipedalism.[2]

The working plan of creation becomes day by day more clear; it is that each group, given time and space, will not only be fruitful and multiply, but will diversify in the search for every form of food by every possible method. Specialization in the long run proves fatal; the most specialized branches die out; the members of the least specialized branches become the centres or stem forms of new radiations.

The Mammals of Four Continents

So it is among the mammals, in which these principles find new and beautiful illustrations, although our knowledge of the early phases is fragmentary in the extreme. Our sole light on the first phase, in fact, is that obtained from the two surviving monotremes of the Australian region; from this extremely reptilian and egg-laying monotreme phase it appears, although opinion is divided on this point, that before the Jurassic period (*i. e.*, already in the Trias) two branches were given off, the placental, from which sprang all the modernized mammals, and the marsupial.

The marsupials appear to have passed through an arboreal or tree-life condition, something similar to that seen in the modern opossum.

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[1] For the origin of the Mosasaurs see L. Dollo, *Les Ancêtres des Mosasauriens*, *Bulletin Scientifique de la France et de la Belgique*, t. 38, pp. 137–139.

F. Baron Nopcsa, *Origin of the Mosasaurs*, *Geological Magazine*, N. S. dec. IV, vol. X, no. 465, March, 1903, pp. 119–121.

S. W. Williston, *The Relationships and Habits of the Mosasaurs*, *Journal of Geology*, vol. XII, no. 1, Jan.–Feb. 1904.

[2] See note 2, p. 581.

The marsupials found their opportunity for unchecked adaptive radiation in Australia, and despite the disadvantage of starting from a specialized arboreal type (Huxley,[1] Dollo,[2] Bensley[3]), through the later Cretaceous and entire Tertiary a richly diversified fauna evolves, partly imitating the placentals and partly inventing new and more or less peculiar forms of mammals, such as the kangaroo.

The oldest placental radiation which is fully known is that which was first perceived in Europe and fully recognized by the discovery in 1880 of the basal Eocene mammals of North America — it may be called the Cretaceous radiation. These mammals[4] are distinctly antique, small-brained, clumsily built, diversified, imitative both of the marsupial and of the subsequent placental radiations; and our fuller knowledge of them after twenty-five years of research is at once satisfying and disappointing, satisfying because it gives us prototypes of the higher or modern mammals, disappointing because few if any of these prototypes connect with the modern mammals. This fauna is found in the Cretaceous and basal Eocene of Europe, North America, and possibly in Patagonian beds of South America (Ameghino),[5] and while giving rise to many dying-out branches, by theory it furnished the original spring from which the great radiations of modern mammals flowed. But practically again we await the direct connections and the removal of many difficulties in this theory. In fact, one of the great problems of the present day is to ascertain whether this radiation of Cretaceous mammals actually furnished the stock from which the modern mammals sprang, or whether there was also some other generalized source.

The Tertiary, or Age of Mammals, presents the picture of the dying out of these Cretaceous mammals in competition with the direct ancestors of the modern mammals.[6] I use the word modern advisedly, because even the small horses, tapirs, rhinoceroses, wolves, foxes, and other mammals of the early Tertiary are essentially modern in brain development and in the mechanics of the skeleton as compared with the small-brained, ill-formed, and awkward Cretaceous mammals.

Whatever the origin, two great facts have been established: first, the modern mammals suddenly appear in the Lower Eocene (as distinguished from the basal Eocene, in which the Cretaceous mammals are found), and second, they enjoy a more or less independent evolution and radiation on each of the four great continents. There thus arose the four peculiar or indigenous continental faunæ of South America, of North America, of Europe and Asia or Eurasia, and of Africa. Of these South America was by far the most isolated and unique in its animal life. North America and Eurasia were much the closest, and Africa acquired a halfway position between isolation and companionship with Eurasia.

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[1] T. H. Huxley, *On the Application of the Laws of Evolution to the Arrangement of the Vertebrata, and more particularly of the Mammalia*, *Proceedings* of the Zoölogical Society, London, pp. 649–662.

[2] L. Dollo, *Les Ancêtres des Marsupiaux étaient-ils arboricoles*, *Miscellanées Biologiques*, 1899, Paris, pp. 188–203; *Le Pied du Diprotodon et l'Origine arboricole des Marsupiaux*, *Bulletin Scientifique de la France et de la Belgique*, 1900, pp. 275–280.

[3] B. A. Bensley, *On the Evolution of the Australian Marsupialia*, *Transactions* of the Linnæan Society, London, 2d ser. Zoölogy, vol. IX, pt. 3, 4to, London, 1903.

[4] That is, the *Multituberculata*, *Creodonta*, *Tillodontia*, *Condylarthra*, *Amblypoda*.

[5] Fl. Ameghino, *Mammifères crétacés de l'Argentine*, *Bollettino* del Instituto Geografico Argentina, tomo XVIII, 1897, p. 117; *Notices Préliminaires sur des Mammifères Nouveaux des Terrains Crétacés de Patagonie*, *Boletin* de l'Academia Nacional de Ciencias de Cordoba, tomo XVII, 1902, pp. 5–68.

[6] H. F. Osborn, *Ten Years' Progress in the Mammalian Paleontology of North America*, *Comptes Rendus*, Congrès Internationale de Zoölogie, Bâle, 1905.

South America. The most surprising result of recent discovery is that the foreign element mingled with the early indigenous South American fauna is not at all North American, but Australian.[1] The wonderful variety of eight orders of indigenous rodents, hoofed animals, edentates, and other herbivores were preyed upon by carnivores of the marsupial radiation from Australia, which apparently came overland by way of Antarctica. There are possibly here also some South African foreigners. The South American radiation more or less closely imitated that of the northern hemisphere. Late in Tertiary times North America exchanged its animal products with South America, practically to the elimination of the latter.

Eurasia and North America. Each of these continents contained four orders of mammals in common with South America, namely, the Primates (monkeys), the Insectivores (moles and shrews), the Rodents (porcupines, mice, etc.), and the Edentates (armadillos, etc.). From some early Tertiary source North America, Eurasia, and Africa also acquired in common four great orders of mammals which are not found at all in the indigenous fauna of South America. These are the Carnivores (dogs, cats, etc.), the Artiodactyls (deer, bovines, camels, and pigs), the Perissodactyla (horses, rhinoceroses, and tapirs), and the Cheiroptera (bats). Migration and animal intercommunication between North America and Eurasia was very frequent. The history of these nine orders of mammals in North America and Eurasia developed as follows: Certain families indigenous to North America both evolved and remained here, others finally migrated into Europe and South America. Similarly Eurasia had its continuous evolution into forms which remained at home as well as into those which finally migrated into North America and even into South America.

Africa. The most astonishing and gratifying feature of recent paleontological progress has been the revelation of what was taking place in Africa at the same time (Andrews [1] and Beadnell). This discovery came with its quota of unthought-of forms, also with the representatives of three orders which it had been prophesied [2] would be found there, namely, the Proboscidea (elephants and mastodons), the Sirenia (manatees and dugongs), and the Hyracoidea (conies). The basis of this prophecy was the anomalous fact that these animals suddenly appeared in Europe in the Miocene and Pliocene fully formed and without any ancestral bearings; it was certain that they had evolved somewhere, and Africa seemed the most probable home, rather than the currently accepted unknown regions of Asia. Thus by a sudden bound paleontology gains the early Tertiary pedigree of the elephants and of two if not three other orders.

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[1] For a series of monographs on the South American fossil faunas, see *Reports* of the Princeton University Expeditions to Patagonia, 1896–99, 4to, Princeton, N. J. For the Australian element in the South American faunas see Moreno, *Note on the Discovery of Miolania . . . in Patagonia, Nature*, Aug. 24, 1899, p. 396; H. F. Osborn, *Science*, N. S., vol. XI, April 13, 1900, pp. 564–566. Sinclair, W. J., *The Marsupial Fauna of the Santa Cruz Beds, Proceedings* of the American Philosophical Society, vol. XLIX, 1905, pp. 73–81.

Africa in the early Tertiary, whether from the absence of land connections or from climatic barriers, was a very independent zoölogical region.[2] Some predatory Cretaceous mammals (Creodonta or primitive carnivores) found their way in there, also certain peculiar artiodactyls (Hyopotamids). Here also were two remarkable types of mammals (*Arsinoitherium*, *Barytherium*) which have no known affinities elsewhere, as well as the extremely aberrant Cetaceans or Zeuglodonts.

The Outlook

From all these continents we have, therefore, finally gathered the main history during the Tertiary period of eighteen orders of mammals. We have still to solve the origin of the cetaceans or whales, still to connect many of these orders which we call "modern" with their sources in the basal Eocene and Upper Cretaceous, still to follow the routes of travel which they took from continent to continent. Encouraged by the prodigious progress of the past twenty-five years, we are confident that twenty-five years more will see all the present problems of history solved, and judging by past experience we may look for the addition of as many new and no less important ones.

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[1] C. W. Andrews, in *Geological Magazine* for 1900, 1901, 1902, 1903, 1904, in *Annals and Magazine of Natural History*, 1903, p. 115; in *Proceedings* of the Zoölogical Society, London, 1902, p. 228; in *Proceedings* of the Royal Society, vol. 71, p. 443; in *Philosophical Transactions*, ser. B, vol. 196, 1903, p. 99; in *Publications* of the Survey Department, Cairo, Egypt.

[2] H. F. Osborn, *The Geological and Faunal Relations of Europe and America . . . and the Theory of the Successive Invasions of an African Fauna*, *Science*, N. S. vol. XI, no. 276, pp. 561–574, April 13, 1900.

SECTION D

PETROLOGY AND MINERALOGY

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PETROLOGY AND MINERALOGY

(*Hall* 9. *September* 22, 3 *p. m.*)

CHAIRMAN: DR. OLIVER C. FARRINGTON, Field Columbian Museum, Chicago.
SPEAKER: PROFESSOR F. ZIRKEL, University of Leipsic.

THE Chairman of the Section of Petrology and Mineralogy was Dr. Oliver C. Farrington, of the Field Columbian Museum, who opened the Section with the following remarks:

"This Section has met in the interests of the sciences of petrology and mineralogy. Although mineralogy is the older of these two sciences, it is quite likely that petrology will claim more of our attention to-day since its problems are at present the more pressing and perplexing. This is in accord, as well, with the usual human experience that a younger child requires more attention than an older one.

"In accordance with the uniform programme of the Sections of this Congress it is sought to have presented here one paper dealing with the relations of petrology and mineralogy to other sciences and one treating of the present problems of these sciences. We regret very much that Professor Brögger, who, it was hoped, would prepare the paper upon 'Present Problems,' finds it impossible to undertake the work, and hence we are deprived of the pleasure of seeing and hearing from him.

"The 'Relations of Petrology and Mineralogy to other Sciences' will be treated by Professor Zirkel of the University of Leipsic. It is with especial pleasure that we greet him, since we remember that it was the elder Zirkel who was in a sense the pioneer of petrology in America. It was no longer ago than 1876 that the Director of the Survey of the fortieth parallel of the United States, Clarence King, desiring a description of the rocks obtained by the Survey, found, as he states in his report, that 'the important study of petrology had suffered complete neglect in America,' and hence he felt obliged to 'turn to Europe for aid.' It was this description of the rocks of the fortieth parallel by Zirkel which was in a sense the pioneer publication in petrography in America, and it still remains a classic. We of America may take a just pride in the fact that it would no longer be necessary to 'turn to Europe for aid' in such an emergency, but that it would not be necessary is due in no small sense to the unselfish and earnest labors of Zirkel and his contemporaries in the instruction of American students and in the study of American rocks."

THE RELATIONS EXISTING BETWEEN PETROGRAPHY AND ITS RELATED SCIENCES

BY FERDINAND ZIRKEL

(*Translated from the German by Cleveland Abbe, Jr., Washington, D. C.*)

[**Ferdinand Zirkel**, Ordinary Professor of Mineralogy and Geology in the University of Leipsic, Director of the Mineralogical Museum and Institute. b. May 20, 1838, Bonn-on-the-Rhine, Germany. Ph.D. University of Bonn, 1861. Royal Privy Councilor; Professor, University of Lemberg, 1863–68; Kiel, 1868–70; Leipsic, since 1870. Member of the Academies of Science of Berlin, Vienna, Munich, Göttingen, Turin, Rome, Christiania, New York; Royal Society, London; Honorary Member, Royal Society, Edinburgh.]

Few other sciences have undergone such profound changes during the last third of the past century as has the science of petrography. The refined methods of investigation, especially the preparation of thin rock sections, the employment of the microscope, and the application of other optical instruments, to which are due in part the present status of the science, have been invented, improved, and made to bear fruit only within the past thirty or forty years. The resultant increase in number of known facts and their correlation by means of geological observations has been accompanied by increased efforts to deepen our insight into the causal connections and genetic relations between petrographic phenomena. During the same period there has been also a rapid increase in the number of investigators along petrographic lines. This increase is due in part to the inspiration and support of petrographic laboratories established during this period of time, and in part to the national geological surveys whose collections and activities have immeasurably increased the amount of study material. Petrographical literature, previously limited almost wholly to Germany, England, France, and Scandinavia, has also taken on a much broader international character. A number of excellent young students from the United States, after receiving training and inspiration by several years of European study, have returned to their native land, and by original independent research won for her a place in the front rank.

No science can exist wholly for itself alone, exerting neither a passive nor an active influence. Each science must make some use of the results acquired by allied branches of knowledge for the furthering of its own advancement, and again each must contribute from its own results toward the advancement of other sciences. Since the science of petrography deals with the materials composing the firm external crusts of the earth, *i. e.*, the rocks, there can be no doubt that the sciences most nearly related to it are mineralogy, geology, physics, and chemistry. These sciences, which enter most directly into the service of petrography, are certainly destined to become a part of it through peaceful assimilation, just as every rock used in the construction of a building thereby becomes a building-stone, no matter what other name it may go by.

We now come to two questions: first, What do the neighboring sciences contribute to the development of petrography? and second, What does petrography contribute from the range of its own experiences toward the understanding of phenomena or the solution of problems belonging to neighboring provinces? In reply to these questions it would appear that on the whole our science receives more help than it gives, although it is not nourished and supported by other sciences to the same extent as is that great complex of heterogeneous sciences known as modern geography.

With respect to the relationship of petrography and mineralogy, however, conditions are quite the opposite. Every one who has been actively engaged along both these lines of study during the past decade, and especially those who, like myself, have developed contemporaneously with the rapid modern growth of petrography, will admit that purely petrographic studies have been infinitely more fruitful to mineralogy than *vice versa.* It is true that as early as during the fifties there had been scattered, disconnected attempts to study isolated minerals by means of the microscope; but these attempts remained without further significance because of the indifference, skepticism, and lack of comprehension which then prevailed. General and methodical microscopic studies were first concerned with the thin sections of those minerals important as being constituents of rock-species, and whose recognition was, therefore, one of the chief problems of petrology (*Gesteinskunde*). Thus all these interpretations were undertaken rather in the service of petrography than of mineralogy. All those peculiarities of the rock-forming minerals which the petrologist was thus determining and studying with ever-increasing zeal, — the positions of their optic and elasticity axes, their coefficients of refraction and of absorption, their relative cohesive strengths, their twinning laws, and their finer structural conditions, the nature of their solid and fluid inclusions, the phenomena of alteration and weathering, their reconstruction into new epigenetic substances, — all this knowledge has been contributed to mineralogy proper. It was not until the necessity arose for studying the petrographic associations of many minerals that we obtained light on the history of their development. Until petrology included them in its province, how meager was our knowledge of titanite, sillimanite, cordierite, zoisite, tridymite, nepheline, leucite, mellilite, and many of the feldspars, of the members of the pyroxene-amphibole group! How poor the text-books on mineralogy would appear, if all of that material based on petrographic work, which now enriches and lends attractiveness to them, were to be withdrawn! That petrographic-geologic theory, by means of which Bunsen would explain the varied chemical compositions of the eruptive rocks, is reflected in Tschermak's ingenious and fruitful conception that the triclinic feldspars consist of a series of mixtures of two chemically different but isomorphous end-members.

As a matter of course, in all these mineralogic petrographic studies *physical* methods are continually employed. While it is true that the optical appliances of physics have become the common property of the petrographer, yet it must not be forgotten that the latter has also invented new instruments after special patterns and has made valuable improvements in others, all of which redounds to the advantage of general physics. A further service to physical science arose from the fact that a considerable portion of the laws of heat and optics had to be first investigated or verified by means of substances which belong to the mineral kingdom. Again the physical method of procedure used to separate heterogeneous mixtures by means of heavy solutions has been brought to yet greater perfection since its application to petrographic problems. The investigations which are endeavoring to apply the laws of mechanics in the study of rock-masses subject to deformation, torsion, or fracture are partly petrographic, but chiefly geologic in character.

We have long had lump *chemical* analyses of rocks, as well as partial analyses dealing with those rock-constituents dissolved or decomposed by acids, and those not attacked; and also analyses of the individual, isolated, rock-forming minerals. To be sure, all such analyses were at first considered as ornamental trimmings to the rock description, and they were frequently executed by rather inexperienced novices. For a while, also, the chemical analysis of rocks was neglected, because the rapidly increasing study of the carbon compounds seemed to be a more attractive and even lucrative field. At present the application of the methods of chemical analyses to the study of petrographic material is more general than ever before, and the undeniable significance of the results cannot be too strongly pointed out. Very properly the massive eruptive rocks and the crystalline schists continue to excite the most interest. Indeed, in recent years too much weight seems to be given to special chemical peculiarities if one is thereby induced to establish new and burdensome names for these rock-masses which are certainly non-stoichiometric in composition, merely on the basis of slight variations in the amounts of either the monovalent or the bivalent metals, or of both.

In recent years the United States Geological Survey also has made many very valuable individual contributions to the science. Among these are many hundreds of analyses executed with ever-increasing completeness and accuracy, which have shown that such supposedly rare substances as vanadium, barium, and strontium are present in nearly all the eruptive rocks, and that even molybdenum occurs with surprising frequency, although in very small quantities. In this connection W. F. Hillebrand is especially deserving of mention, his practical *Guide to the Analysis of the Silicate Rocks* being a perfect treasure-house of experience and practical hints. He very properly insists on the desirability of coöperation between the chemical and the microscopical study of rocks, now so commonly kept separate, and points out that if the examination of the thin section always preceded the chemical analysis the latter could be carried out with greater ease and exactness.

The literature of chemical petrography has recently been enriched by a truly monumental work, also executed with wonderful industry on this side the ocean. Henry Washington, following in the footsteps of Justus Roth, but with a more modern point of view, has succeeded in assembling and critically reviewing all the analyses of eruptive rocks and tufas which have been published during the sixteen years from 1884 to 1900. Beside the introductory remarks dealing with the selection of material, the amount of material, the measure of the degree of accuracy and completeness of the analyses, the sources of error, *et cetera*, the work is of primary importance as being the first attempt to appraise justly and impartially the relative values of the analyses. Adopting a method similar to that used in estimating the credit of a commercial business, he undertakes to arrange these analyses, according to their degree of accuracy and completeness, in five groups, designated in descending order as "excellent," "good," "fair," "poor," "bad." He has made a beginning most deserving of acknowledgment, and it is to be hoped that it will serve as a warning cry to analytical chemists.

In order to determine the composition of a rock-species the satisfactory chemico-petrographic analysis must show both the percentages of the various component materials as well as afford an insight into the position the rock occupies in certain chemical series. While a normal series is characterized by a steady increase and decrease in the materials, the peculiar ultra-members are especially noteworthy in this respect. Thus we have the great independent group of the Eruptives, which in spite of great basicity is almost wholly lacking in alumina and alkalies, although enormously rich in magnesia. Perhaps an even more striking case is that of a rock containing scarcely twenty per cent SiO_2, with almost all the remainder of its composition consisting of Al_2O_3, and yet the rock is a true Intrusive.

In recent years there have been many attempts to express the relationships of rocks by using simple formulated expressions for the chemical rock-composition. There have been also endeavors to show the relative position of an analysis by graphic methods which bring out the relative proportions of the individual components as shown by their percentages of the total weights calculated according to the individual molecular weight. Loewinson-Lessing, Pirsson, Michel-Levy, Mügge, Brögger, Becke, Iddings, Osann, have made special suggestions in this broad field of chemico-classificatory formulæ, graphics, and topics.

The second great aim of the chemical analysis of rocks is to prove the existence of changes in the substance of certain rock-material by comparing it with other material which has not undergone such alteration. Thus the methods of analytic chemistry have accumulated a great mass of knowledge concerning the regular course of simple weathering and of the complicated alterations caused by the universally active agencies of weathering aided by the carbonated and silica-bearing solutions which are the first products of that process. Our great master, Gustav Bischof, has rendered the immortal service of introducing order into our comprehension of this silent play of chemical relationship and of the mutual exchange of material within the rocks and strata of the earth.

But the science of chemistry must also come to our aid in explaining other more local transformations which take place in the minerals of the earth's crust. And first of all, regarding the changes in those regions where as the result of the intrusion of eruptive masses the bordering rock-strata have often been altered over broad areas into that changed condition known as contact metamorphism. As far as the effect of the active eruptive rock upon the passive country rock can be recognized in these aureole-like areas of metamorphism, — from the actual line of contact where the metamorphic energy is most intense, even to the extreme circumference where the last traces die out in the unaltered host, — the affected rock-mass is found changed according as it is more or less sensitive to such changes; but yet hundreds of localities widely scattered over the earth's surface show that the change in mineral content and even in rock-texture has always taken place along the same general lines. It is now a problem for the chemist to decide whether such a change represents simply the molecular rearrangement of materials already present in the host, or whether the latter has undergone an essential change in its chemical composition, having taken up materials which separated out from the intruding rock as it solidified. Long series of comparative analyses seemed to support the former explanation, at least for the case of the plutonic rocks. These analyses indicate that as a rule the phenomena of contact metamorphism take place without either addition or loss of material, that the active eruptive rock produces the phenomena of metamorphism simply through the agency of changes in pressure and temperature accompanying the intrusion quite independently of its own peculiar composition. It is true French investigators hold the contrary view, believing that even in the transformations in the usual contact metamorphism, — *e. g.*, of clay slate into hornfelt, (*Fruchtschiefer*, *Garbenschiefer*), — new materials contributed to the sub-strata play a part. Chemical analyses early proved this to be true for contacts of intrusive diabases. There can be no doubt, either, that when the host of certain granitic intrusives shows, beside the usual alterations, repeatedly recurring mineralizations with newly formed tourmaline, topaz, cassiterite, axinite, and fluorine mica, that the formation of these minerals, so often connected with fissures, must point to a fumarole-like exhalation of fluorine and boron vapors accompanying the eruption of the granite. In other words, they must prove that there took place an actual infusion of foreign chemical materials into the surrounding rocks.

There is another kind of rock metamorphism. The mountain-building forces have compressed, folded, and crushed the rocks over broad regions. Thus they have acquired a different and usually more schistose structure, while at the same time they have developed a new mineral composition. There now arises the important question what are the chemical characteristics of these products of pressure metamorphism. Based on insufficient material and limited to specially favorable hypothetical conditions, the law has been pronounced that even in cases of the most thorough transformations of structure and mineral composition there can have been no noteworthy chemical change. By means of a comprehensive series of analyses, Reinisch has shown this to be an erroneous generalization. He has shown that the granite orthoclase rocks and diabases, when subjected to pressure metamorphism, undergo a regular and very considerable chemical alteration. There may be so great a difference between the composition of the normal rock and the composition of the same rock after undergoing dynamic metamorphism that it is no longer possible to speak of the rock as being chemically unimpaired. This is not unnatural, since a greatly crushed rock offers a great number of new points to the attack of the subterranean water. It is thus no longer justifiable to attempt to determine, as was formerly done, the original rock by means of the chemical analysis of its representative among the products of dynamic metamorphism, for all resemblance to the original has been obliterated.

These examples show how indispensable in petrographic problems is the aid offered by chemical analysis. The obligation does not lie all upon the one side, however. Cases could be cited where chemistry has reason to make acknowledgments to petrography for having demanded increased refinement or broadening of existing methods. Charged with the problem of determining the presence of even those elements which occur in scarcely traceable quantities in the terrestrial rocks, the chemist was called upon to discover those particular reactions best suited to show the presence of those elements, most sharply to separate them from one another, and to determine them quantitatively with the greatest accuracy. Such investigations undertaken, as, for example, were those by Hillebrand, at the command of petrography for her own profit, have thus proved a benefit to the whole science of analytic chemistry. While serving petrography Gooch discovered the new methods for the separation of titanium, lithium, and boron; and the chemist owes to his inventive ingenuity the perforated platinum filter-cone and tubulated crucible used in making water determinations. The mineral riches of the Stassfurt rock-salt deposits inspired van 't Hoff to undertake long-continued and important researches into the conditions of equilibrium, the solubility curves, and conditions of formation of the hydrates, the double salts, and the products of double reactions.

In recent years the science of micro-chemistry has grown up and developed alongside the ordinary macro-chemistry. In this new science the eye, armed with the microscope, attempts to recognize both the changes produced in the subject under examination, and also the newly formed product of the chemical reaction. After applying the reagents to a very minute particle of the mineral, or drop of a solution, it is endeavored to secure by evaporation a product of the reaction which, though microscopic, shall be so characteristic optically and crystallographically, that it may serve to identify beyond doubt the presence of its particular elements in the original specimen. These special micro-chemical methods, which have proved most satisfactory for numerous elements, and are frequently employed, are now in the service of ordinary qualitative analysis. An historical review must, however, emphasize the fact that they were first introduced solely for the uses of petrography. It was Bořicky who, in 1877, in the course of his studies in lithology, hit upon the idea of treating mineral particles with hydrofluosilicic acid in order to obtain fluor-silicates of the alkalies, alkaline earths, etc., which betray the presence of suspected elements by distinct and characteristic crystal forms.

There is a constantly growing conviction that a large number of petrographic problems will find their explanation among the future results of the science called *physical chemistry*, a science which has won by its recent successes the right of actual independence, although its name suggests that it occupies an intermediate position. The following broad outline shows that its principles, laws, and methods may be most profitable to, and have already been in part applied in, petrographic fields.

It is peculiarly interesting that the concept of "solid solutions," which excited great interest in chemistry upon being put forward as something novel, had already long been accepted by petrographers as a matter of course. We have long known that while the lava magma is a molten solution of varying composition, and that the chemically identical, homogeneous, firm, amorphous product of its solidification, which forms as soon as the molecular mobility of the magma is lost, and before crystallization sets in, *i. e.*, the natural glass corresponding to the magma, — cannot be other than an under-cooled solidified solution.

We no longer assume the natural silicate fusions (*Silicatschmelzflüsse*) to consist of substances dissolved in a solvent of definite stoichiometric composition, but regard them as being, probably, mutually dissociated solutions. Speculations upon the nature of solvents, which have always been of most problematical origin, are thus rendered futile.

Bunsen already emphasized the fact that the same laws control crystallization from molten solutions as control crystallization from aqueous solutions. There can be no doubt that Gibb's Phase Rule for aqueous salt solutions also controls solidification from molten solutions. However, the presence of many compounds dissolved in the magma introduces complications which will make it difficult to apply the rule to the order of crystallization.

The order in which the individual constituents of a uniformly granular eruptive have solidified, or, to speak more accurately, the order in which they have begun to crystallize, is an old petrographic problem of the first rank. To-day no one may deny that this succession is normally controlled, as Lagorio has shown, by the character of the bases, and is not, as Rosenbusch believed, according to increasing acidity. The least soluble substances separate out first and the most soluble separate last. It has been shown experimentally that the descending order of solubility in molten silicate solutions is as follows: Iron oxide, magnesia, lime, soda, potash, and alumina, the last entering relatively late into the molecule of the various constituents, and finally silica itself. Yet there are hundreds of well-verified instances where the corresponding mineral series — iron ores, olivine, and rhombic pyroxene, monoclinic pyroxene, amphibole and biotite, anorthite, lime-soda feldspars, nepheline, albite, and ægirine, orthoclase, quartz — has not been adhered to, either through an inversion of order or by the contemporaneous crystallization of minerals which should have separated successively.

Two facts alone seem to be well established. First, in those silica-rich rocks which contain quartz, the latter mineral as a rule is among the last to solidify. Second, the minerals — apatite, zircon, rutile, titanite, ilmenite, and perofskite — containing those compounds, such as phosphoric, zirconic, and titanic acids, present in the magma in the smallest and even scarce traceable quantities, are the very first minerals to crystallize out, even though, in common with the ores, they sometimes show a not inconsiderable period of separation. It is to be doubted if the early solidification of these accessories is really due to their small proportionate amount, as is often believed, for since the solution seems to be diluted with reference to them, they should not crystallize out until quite late. Since it is, also, not permissible to adopt the rather drastic view that the magma strives first to rid itself of these foreign bodies, it is therefore preferable to assume that these minerals are especially difficult of solution in the silicate solution at the lower temperatures.

The causes of the variable behavior of the characteristic constituents as regards order of crystallization are still in large part unknown. The treatment of the problem is, however, made especially difficult, since both experimental and theoretical considerations have been accustomed to assume only two substances in solution, whereas a silicate-rock magma usually contains more than four substances in solution contemporaneously.

Attention is drawn to the fact that in certain solutions the range of temperature appropriate for the separation of one compound, *e. g.*, leucite, may be very limited, while, under otherwise the same conditions, the range appropriate to the separation of another compound, *e. g.*, augite, may be much greater. Thus, according to the temperature conditions, one and the same magma may yield up its augite now before, now after, its leucite. Furthermore, Meyerhoffer has shown that, according to the labile equilibrium, now *a* and now *b* may first crystallize out of the same slag.

The order of separation may be affected by yet another factor, viz., pressure. Since, according to the usual view, the rock-forming minerals contract as they crystallize out of their magma, then, as Sorby and Bunsen have shown, increased pressure must aid this contraction, *i. e.*, must accelerate crystallization. The concomitant shifting of the temperature of solidification (freezing-point) takes place unequally for different substances. Thus two substances which under ordinary atmospheric pressure have different freezing-points, may under somewhat greater pressure, and consequently more nearly equal melting-points, freeze at the same temperature, while under the influence of yet higher pressure the normally quicker-freezing substance may become the slower-freezing one. On the ground of such changes, then, the order of separation may be altered, as shown for example by the varying relations between the more easily fusible augite and the more difficultly fusible orthoclase.

According to Doelter, the rate of crystallization may also be of importance, in so far as the advantage (start) in separation given to the substance *a* by its lower solubility may be equaled or overbalanced by the tendency to more rapid crystallization possessed by the more readily soluble substance *b*. When such an overtaking does not take place, and this is said to be true sometimes, the explanation may be found in the varying degree of viscosity of the magmas and the corresponding changes in rate of crystallization. If increasing viscosity, *i. e.*, increasing internal friction, opposed the crystallization of *a* and *b* in equal degrees, the initial advantage of the former could not be so easily overcome by the latter, if at all.

Other physico-chemical questions in this province are as follows: Is the order of crystallization influenced by the relative amounts of constituents, and to what extent? What is the rôle of the as yet but little studied *Impfkrystalle?* Are certain uniformly fine-grained aggregates, consisting of two minerals mixed in definite proportions, the product of what Guthrie has termed "eutectic mixture," analogous to the cryohydrates? During the solidification of a magma, what rôle is played by the mineralizers, *les agents minéralisateurs*, those substances, in part of a gaseous nature, which seem, by their presence in the magma, to exert a purely catalytic influence upon the crystallization, *i. e.*, they seem to aid the latter process without either suffering change themselves or entering into the composition of the substances which are formed in their presence? Thanks to Iddings, we are somewhat better informed as to the causes of the frequently observed magmatic corrosion and resorption of already crystallized constituents by the remaining magma. Apparently we here have to do with a shifting of the condition of stability between the solid and fluid phases of the magma.

Physical chemistry will also render much-needed help in reaching the explanation of the differentiation of magmas. This widespread phenomenon, characteristic both of extensive eruptive masses and of broad dykes, consists in a splitting-up of the original magma into two submagmas, one of which is acid, predominatingly alkaline and rich in alumina, the other basic, rich in iron and magnesia silicates, but poor in alumina and alkalies. The former submagma almost always has a central position, the latter submagma appearing as a basic marginal facies at the periphery. These submagmas must have originated by diffusions acting in two opposite directions during the fluid condition of the original magma, and thus there arise two questions: First, What forces can have produced such a separation into two submagmas so diametrically opposite in character, collecting the bivalent metals with a small amount of silicon into one group, and assembling the monovalent metals with somewhat more silicon into the other group? Second, Why did the acid submagma assume a central and the basic submagma a peripheral position? There are several objections to the direct application of the law proposed by Soret and amplified by van't Hoff, which states that those constituents with which a solution is almost saturated tend to collect about the colder points.

One objection is that this law has been verified only for the case where there is but one substance dissolved in the solvent. Guy and Chaperon's law, that gravity coöperates to destroy the homogeneity of a solution, can have no application here, for in that case the heavier basic submagma should appear at the lower levels, and the lighter acid submagma in overlying higher levels, an arrangement which would not explain the contrast between centre and periphery. The explanation of the peripheral basicity as the result of the melting of the bordering country rock is not only difficult to understand, but contradicts innumerable facts, and furthermore denies that any magmatic differentiation takes place.

Brögger has contributed a distinct advance to our understanding of this problem by showing that, in special cases, it was definite stoichiometric combinations and not the isolated materials which moved in these opposing directions, the silica-poor, iron-magnesia-lime silicates having moved in the one direction and the silica-rich, alkali-alumina silicates in the other. Furthermore, he found that these stoichiometric combinations correspond to the minerals of the eruptive rocks where, as is well known, it is the rule to find the alkalies, aluminum, and calcium associated on the one hand, while on the other magnesium, iron, and calcium usually go together. Thus it comes about that the least soluble combinations are those which gravitate toward the cooling surface, and to this extent differentiation obeys the laws controlling the tendency to crystallize. Harker seems to hold similar views. We have here, certainly, an important exposition, but it merely recognizes a fact and offers no actual explanation of the same. There yet remains the unanswered question, What is the nature of that motive force, on account of which precisely the melanocratic pole has a peripheral position while the leucocratic pole has a central one? We know nothing about the difference in the diffusion constants of the respective [stoichiometric] combinations.

Contrasts similar to that between the centre and periphery of the same *massif* are found in a region where many so-called complementary dykes occur, acid and basic rocks being in close proximity. This association is explained as simply the result of fissure-filling by a differentiated plutonic magma precisely similar in origin to the one above referred to.

The question is an important one whether the fluid molten magma suffers decrease or increase in volume with its transition to the solid, crystalline condition. Gustav Bischof, Mallet, and David Forbes, as the result of experimental investigations, have expressed the opinion that the mass suffers a contraction of about one tenth, and the development of contraction fissures (cooling cracks) in the solid lava agrees with this. In the oft-cited experiments by Barus the frozen lava was remelted, and thereupon showed, in accordance with the above, an increase of volume.

The question has again attracted great attention because it plays such an important part in the theory of vulcanism newly proposed by Stübel. Stübel denies that the pressure of the contracting earth-crust upon the actually glowing interior produced the volcanic phenomena. He holds that the slowly freezing so-called "crust" retains imprisoned within it relatively small, nest-like reservoirs of glowing molten magma. The molten magma in these reservoirs escapes to the surface by eruptive vents (*Ausbruchskanal*), being forced out by the increase in volume which the magma undergoes in the course of its cooling. In view of previous results, he admits that the final result must be a contraction, but believes he may assume it to be most probable that in the course of the cooling the molten mass passes through a transitory phase of expansion or increase in volume. We have, however, absolutely no experimental knowledge of such a phase.

A further principle of physical-chemistry, which explains petrographic processes within the sedimentary rocks, is the tendency to reduce to a minimum the existent exposed surface (*Oberfläche*) of a number of contiguous identical individuals. It appears that equilibrium between a saturated solution and the bounding surface of a crystal in that solution is not established until the bounding surface is reduced to its minimum. If one moistens the powder of a soluble salt and then allows it to stand for some time, it is found that the mass assumes a distinctive crystalline structure, composed of large individuals, showing that a certain portion of the grains of the powder have increased their own dimensions at the expense of the remaining portion which may be said to have been consumed. In a corresponding way, and obedient to the same laws, it seems that recrystallization has produced similar effects in the structural character of those coarse-grained marbles which represent former very dense limestones. In this case it would seem that the small limestone grains, in the presence of carbonated water, possess the tendency to develop into larger individuals by mutual assimilation and a rearrangement of their molecules into parallel orientation. Moreover, we may thus come to understand why the older saline deposits are sometimes so coarse-grained, while deposits from salt lakes in recent times come down almost crypto-crystalline (*dicht*) in character; and in like manner the growth of the crystals of glacier ice, from the firn to the end of the glacier, may be explained.

Since petrography forms a part of *geology*, there is, of course, an intimate connection between the two sciences, they being mutually complementary. There can be no science of geology without petrography, nor can the science of petrography disregard discoveries made in other branches of geology, but it is not necessary to discuss in this place the relationship between these two branches in much detail, just as it would be scarcely requisite to set forth the relationship between paleontology and geology.

Thus modern petrography stands to-day in the midst of a circle of bordering sciences, and there is a mutual interchange of inspiration, acknowledgment of indebtedness, and instruction. If our science may not send to its neighbors the proud challenge *Do ut des* (I give in order that thou mayest give), yet she does not promise too much nor ask in vain when she makes the more modest request, *Da ut dem* (Give thou to me and I also will then give something).

SHORT PAPER

Professor William H. Hobbs, of the University of Wisconsin, read a paper on "Suggestions regarding a Petrographic Nomenclature, based on the Quantitative Classification," in which he said:

"The year 1903 was a remarkable one in the history of petrography. The chaotic condition in which petrographers have found the system of classification and of nomenclature was nowhere better illustrated than in 1897, at the International Congress of Geologists, in St. Petersburg. The largest and most representative body of petrographers ever assembled was there unable to fix upon any principles which could be utilized to improve the situation.

"With the close of the year 1903, the situation has materially changed, and the credit for this is almost entirely due to the work of five men. Without the work of the pioneer member of the company, Mr. W. F. Hillebrand, of the U. S. Geological Survey, the results could not have been secured, owing to a lack of adequate data upon which to construct a system. The large series of accurate analyses which were brought together and published in 1900, as Bulletin 168 of the U. S. Geological Survey, constitute the first adequate series of accurate analyses of igneous rocks. In 1903 appeared, after years of preparation, the three works which have so profoundly modified the situation. These are: *The Quantitative Classification of Igneous Rocks*, by Messrs. Cross, Iddings, Pirsson, and Washington; *The Chemical Composition of Igneous Rocks, expressed by means of diagrams*, by Mr. Iddings; and *The Chemical Analyses of Igneous Rocks*, published from 1894 to 1900, by Mr. Washington. There have since been published *The Superior Analyses of Igneous Rocks, from Roth's Tabellen*, published from 1869 to 1884, by Mr. Washington; and a most noteworthy addition to the list of analyses carried out by the Geological Survey."

After briefly indicating the work of the syndicate, the speaker criticised it to some extent as failing to meet the demands of science, and stated that this paper was intended more to call forth further discussion than to make a contribution to it.

SECTION E — PHYSIOGRAPHY

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(*Hall* 12, *September* 21, 10 *a. m.*)

Chairman: Mr. Henry Gannett, United States Geological Survey.
Speakers: Professor Albrecht Penck, University of Vienna.
Professor Israel C. Russell, University of Michigan.
Secretary: Professor John M. Clarke, Albany, N. Y.

THE RELATIONS OF PHYSIOGRAPHY TO THE OTHER SCIENCES

BY ALBRECHT PENCK

(*Translated from the German by Cleveland Abbe, Jr., Washington, D. C.*)

[**Albrecht Penck,** Professor of Geography, Imperial and Royal University of Vienna, since 1885.[1] **b.** Reudnitz, Leipsic, September 25, 1858. Ph.D. Leipsic, 1878; k.k. Hofrat, since 1903; Geologist, Geological Survey of Saxony, 1877–79; *ibid.* Geological Survey of Bavaria, 1881–82; Privat-docent of Geography, University of Munich, 1883-85. Member of Imperial Academy of Science, Vienna; Leopold Carl. Academy of Naturalists; Royal Academy of Padua; Honorary member of Natural History Society of Switzerland; Academy of Science, New York; and numerous scientific and learned societies. **Author of** *Die Vergletscherung der deutschen Alpen; Der deutsche Reich; Die Donau; Morphologie der Erdoberfläche; Friedrich Simony;* (with E. Brüchner: *Die Apennine Eisreitalter;*) and numerous works and articles on scientific subjects.]

The geographical sciences have not developed according to any definite, preconceived plan. They have developed and branched off from one another according as the division of labor and progress in research awoke demands for them. It is therefore vain to attempt to discriminate sharply between them from a philosophical standpoint. Such attempts would frequently result only in constraining them into programmes which did not at all correspond to their development. In order to understand the mutual relations of these sciences one must always adopt the standpoint of the historian. One must acquaint himself with their development, and study how, through the selection of certain problems or by the employment of certain methods of investigation, the work came to be divided up and finally resulted in the establishment of independent branches of knowledge. Only thus can one come to understand the ever-changing scope of the geographical sciences in the past, or discover the probable directions of their future development. In arranging a programme one could

[1] Professor of Geography in the University of Berlin since 1906.

not do better than endeavor to give expression to this course of development.

If now we seek the controlling points of view of the American and British investigators, as revealed by their studies, we are soon convinced that their conceptions of physiography are widely divergent. The British regard physiography as the science of natural processes, while the Americans consider it, essentially, as that part of what in Europe is called physical geography, which deals with the visible features of the continents. It is evidently from the latter standpoint that physiography stands by the side of cosmical physics, geophysics, and oceanography, as one of the eight earth-sciences on the programme of the International Congress of Arts and Science; and we shall here consider it from this point of view.

Physiography appears to me as *a part of geography*, that great mother-science from which so many members have branched off, at first as individual branches only, but soon developing into independent sciences. Physiography belongs to these latter. Its close relation to the mother-science is still shown by the European name, *i. e.*, physical geography, while the American name indicates that it is already becoming an independent branch by reason of its great literature.

In order to understand the exact position which physiography occupies it is necessary first to gain some appreciation of the aims and problems of geography. Scarcely any other science is the object of views so contradictory. To one geography is an agglomeration of sciences which are distinguished from one another by their methods, to another it is only a method applicable to the most widely differing sciences.

This difference of conception is due to the great age of the science. Geography was recognized as a science long before modern specialization brought forth the present geographical sciences, and at first it treated of problems that since have become the special fields of the one or the other of these. Increasing systematization has led to a sharp separation of most allied sciences from their mother-sciences, but in the case of geography new problems are constantly arising which serve to obliterate such separation. Very considerable portions of the earth's surfaces are still unknown, extensive regions are yet to be opened up, and there the geographical investigator meets problems which belong to the provinces of the auxiliary geographical sciences when they are encountered in the better-known regions of the globe. In the one case geographical investigations must be prosecuted in a different manner from those in the other case. Under the first circumstances the investigator must himself use the instruments of the auxiliary science, while under the other he may concentrate his attention upon a more limited field.

However various may be the demands which exploration may make upon the geographer, yet everywhere one problem is prominent which concerns him alone, and that is the earth's surface. The greatest of living German geographers has correctly defined geography as the science of the earth's surface.[1] Yet it is not always the earth's surface alone which occupies the most prominent place in geographical investigation. In civilized countries, where the division of labor is far advanced, that problem belongs to the domain of the auxiliary sciences of topography and cartography, while the true geographical problem is the study of the combinations of phenomena happening on the earth's surface.

The consideration of the various phenomena in their areal relations is the characteristic feature of all geographical investigation. Yet it is a great mistake to think that, for this reason, geography is only a method of investigation which may be applied to the most widely different sciences, for it does not treat of the areal relationships of *any* class of phenomena. Two groups of phenomena are especially prominent in its province. From the earliest times geography has dealt with the relations of the earth's surface to the distribution of man, thereby coming into touch with the science of man, and especially that branch called history. Thus we have come to call this side historical geography, a somewhat unfortunate designation, since its problems have a broader scope than that usually allotted to history. Its problems constitute an anthropogeography in a much broader sense of that word than the one ascribed to it by its inventor, Friedrich Ratzel. Furthermore, geography early began to study the relations between the earth's surface and mundane phenomena, considering both the organic and the inorganic phenomena which take place there. The researches of the biologists have made the largest contributions to our knowledge of the relations between the terrestrial surface and the organic phenomena from the standpoint of the latter, but the strictly geographical side of these problems has yet to be developed. In a similar way, the relation of the earth's surface to the natural forces at work upon it has grown clearer and clearer as the science of physics has advanced. Indeed, in recent years this relationship has acquired unusual complexity, chiefly as the result of English and American studies into the intimate reaction between the forms of the earth's surface and the forces acting upon them.

Physical geography, the study of the relation of the earth's surface to the earth-forces and to the earth as a whole, is thus seen to occupy a position between the geography of organisms in general, — biogeography in the widest sense of the term, or "ontography," as

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[1] Ferdinand Freiherr von Richthofen, *Aufgaben und Methoden der heutigen Geographie*, Leipzig, 1883, p. 3.

Davis happily terms it — and of man in particular on the one side, and the sciences which treat of the earth-whole in general on the other. Its problem reveals its scope. To the extent that the interaction of the earth's surface and the forces at work upon it grows clearer, so the old study of the forms of the earth's surface, which was long scarcely more than descriptive in character, rises into that important branch of physical geography called geomorphology. Topography and cartography form necessary members of this branch, for it is impossible to separate the descriptive treatment of a portion of the observational material from the science. *Physical geography* is the descriptive, genetic, and dynamic study of the earth's surface, and is most intimately related both to geodesy and geophysics, which treat of the whole earth, as well as to the sciences of geology, oceanography, and meteorology, which treat respectively of those portions of the earth called the lithosphere, hydrosphere, and atmosphere. The character of this relationship becomes clearer if we consider the special problems of this science.

However manifold in form the physical surface of the earth may appear to the eye of the observer, yet geometrically it is rather simply modeled. Aside from unimportant and very rare exceptions, this surface consists of combinations of slopes having various angles, but yet all dipping towards one and the same basal surface. This surface is that sensibly level one presented by the surface of the ocean, which geodesy considers the surface of the earth. The close touch between geodesy and physical geography is due to the fact that they both start from the same surface of reference, although seeking different ends. The geodesist endeavors to determine the form of the geoid of sea-level under the continents, while the geographer concerns himself with the variations from that surface which the firm crust of the earth presents. Both workers employ the same set of coördinates in their studies. Of these coördinates, those of latitude and longitude have long been considered as geographical, while the coördinate of elevation above sea-level, which is indispensable to the physiographer, has but rather recently been added to the others. Geography first assumed its present character only after the introduction of this third coördinate as a geographical measure. But while it suffices for the geodesist to determine with very great accuracy the geographical coördinates of relatively few fixed points, the geographer needs to know the positions of all the points of the earth's surface. This would be impossible were it not for the aid of *the geographical map*, by means of which the geographer can graphically represent the surface of the earth and show, exactly, the relative positions of places (or points) according to their differences in longitude, latitude, and elevation. The preparation of the geographic map is the purpose of geographic surveys, while the purpose of geodetic surveys is the determination of the surface of the geoid. The former, however, must be preceded by the latter; the geodesist supplies the frame, the topographer fills in the picture.

For these reasons geodesy is usually ranked above cartography, but the topographer then often fails to win the desirable sympathy with that science in whose service he really stands. This lack of sympathy is not rarely to be seen in Europe, where the geographic mapping of most of the countries lies in the hands of the army. Scholars cannot be too thankful for this peaceful service rendered by the army, but the fact cannot be concealed that the emphasizing of those features which are of military *importance* has not always advanced our knowledge of that which is geographically *true.* The employment of contour-lines, which are so indispensable for the physiographer, was long neglected in Europe because of the exclusive importance there ascribed to the surveying of the militarily significant inclinations of the slopes. Even to-day the map-maker runs the danger of using stereotyped, stencil-like methods, because he so often records forms whose nature and significance are unknown to him. And though it is often claimed that the topographer merely draws that which he sees, yet one forgets thereby that a specially trained observer sees much more than one not so trained. The mere mapping of the forms of the earth's surface does not lead to a deeper understanding of them. It is true that topographic surveying long since observed and made use of the fact that the surface of the earth is not only a surface whose every point may be represented in a projection by an individual point, but is a constantly downward-sloping surface throughout the greater portion of its extent. This recognition brought them the knowledge that the modeling of the earth's surface must have been chiefly accomplished by exogene forces, but it was attempted to refer the work to very great, and in part to catastrophic forces, rather than to slowly working causes. It is but a few years since that great floods and cataclysms played the same rôles in those theories of the earth's surface which were taught in many military courses as they did in the scientific literature of the eighteenth century.

Although it is very necessary, in the interests of exactness of survey of the surface forms, that the topographic work be under the control of the geodetic side, yet this subordination has not essentially increased our understanding of the forms. The greatest advances in method of cartographic representation of these forms have been made in those countries whose maps have been executed by technically well-educated engineers. The leading part played by Switzerland in the cartographic circles of Europe is due to this fact.

A true comprehension of the forms of the earth's surface must rest upon a genetic basis. It is only since we have accustomed ourselves to considering surface forms not merely as something existent, but as something that has developed, that we may speak of a geomorphology which forms a scientific branch of study under physical geography. There are two ways of considering the genetic character of surface forms: (1) One may consider the separate forces at work on the earth's surface and trace out the forms resulting from the activities of each; or (2) we may attempt to analyze the rich collection of forms already present. Both methods of consideration bring us into intimate relations with geology, for in either case the forms which are investigated touch upon changes which the earth's crust has suffered or is suffering. In the latter case, however, one enters the proper field of geology.

Geology early began to investigate more carefully the forces at work upon the earth's surface. Lyell was very instrumental in establishing the principle that the deposits resulting from the activities of these forces give the best key to an understanding of the rocks which make up the earth's crust, and in this way those deposits have been investigated. It would not have been possible to investigate these deposits, if their mode of origin had not been taken account of. The wide distribution of the phenomena of erosion was recognized, and notice even was taken of certain typical forms, but the process which resulted in those forms did not obtain further recognition. It remained for the newer science of geomorphology to do this. This science, in studying the development of streams, discovered the *sequence of forms* which accompanies that development. A stream running down an existing slope cuts a furrow which we designate as a *consequent form*, because it follows the slope already present. Scarcely is this furrow cut than its steep walls begin to assume more gradual slopes, and the surface waters develop new furrows down these new slopes. These new furrows, coming after the original furrow, we call *subsequent forms*. Their development follows other rules than those controlling the consequent forms. The latter develop upon existing or original slopes, the former take their origin on slopes of later development, whose courses are essentially dependent upon rock-character. Professor W. M. Davis has specially studied these developments, and has shown how the subsequent forms adjust themselves to the character of the earth's crust, and more especially to its structure. He has shown how a gradual adjustment is brought about between the watercourses originally consequent upon the existent slopes, and the internal mountain structure. The first position of the original surface, forming the surface of departure (*ausgangsfläche*) for later development, was the result of unequal elevations of the earth's crust. The modeling processes of erosion transform this surface into a *surface of adjustment*, which offers to further denudation greater and greater resistance, until it finally becomes a *surface of maximum resistance.* Plains of denudation (peneplains) seem to be such surfaces. The transformation of the original surface into the surface of adjustment goes on much more rapidly than does the reduction of the latter into surfaces of denudation (peneplains) of maximum resistance. The original form approaches the last form according to the law of asymptotes. For this reason we do not find any plains of denudation actually appearing as perfect planes; they are only almost-plane, and appear as peneplains.

However rich in results for genetic morphology may have been the observations of the forces at work on the earth's surface, this has not sufficed to clear up our understanding of all these forms, for all the forces at work are not visible at the earth's surface. Some act too slowly, others, as glaciers, hide their processes from our observation. The analytical method of study of forms thus remains the only available one. This method yields excellent results as soon as we compare the forms with their contents, as soon as we bring them into relation with the crust of the earth, and learn to compare the surface features with the internal structure, or tectonic conditions. In this case geomorphology is working upon a geological foundation just as the topographer works from a geodetic foundation, and where this necessary foundation is lacking, physiography must supply it, just as in an unknown country it must also supply geodetic work. Topography is often subordinated to geodesy in civilized countries, and similarly there are cases where geomorphology is to be considered solely as a branch of geology. It is easy to understand how those excellent investigators to whom we are indebted for the first idea of the geological structure of the earth's crust readily came to explain the earth's surface forms by the aid of the knowledge they had already gained of its structure. As they found evidences in the structure of the earth's crust of great dislocations apparently the result of violent forces, they thought that the surface features of the land should also be explained by violent crustal movements. It was long before students became emancipated from this conception of violent catastrophes. It was long before the idea became thoroughly incorporated that the forms of the earth's surface are the results of the gradual and mutual reaction of endogene and exogene processes. Even to-day wide differences of opinion prevail as to the relative importance of each of these classes of processes. The student of tectonic phenomena, who recognizes in the stratigraphic structure of the earth's crust most magnificent disturbances, finds so many cases where the external form has been influenced by the internal structure, that he is inclined to explain the physiognomy of the earth's surface as primarily the result of the structure. The topographer, on the other hand, is inclined to explain the surface features as the result of exogene processes because he is solely concerned with the survey of the surface.

The divergence of these two views is due to the fact that the relation between internal structure and superficial form is most complicated. There are not a few localities where the latter is most closely dependent upon the former; but there are also very many places where such a relation is unfortunately absent. The highest mountain range of the earth, the Himalaya, consists of strata which have been most tremendously compressed, yet in Belgium we find regions of scarcely less complicated structure which present an almost plane surface. In the Alps may be found regions of the most complicated structure lying close beside others of the very simplest, yet the latter do not appear to be on that account less mountainous in character. In illustration one needs only to recall the south Tyrolean Dolomites and compare them with the Glarner Alps. The picture of the face of the earth drawn by Edward Suess is widely at variance with that presented by a geographical map. *The morphologic point of view of the physiographer does not coincide with the tectonic one of the geologist.* It would be a mistake to attempt to subordinate the one to the other. One must accustom himself to recognize the fact that he is here dealing with different conceptions of equal rank, and that one should not and cannot supplant the other, but rather must be mutually enriching. The way in which this mutual enrichment is to come about has been especially developed in the United States. Here topography and geology are not so hostile to one another as in most European countries. They are not in the hands of different Government Bureaus, but are both fostered in one and the same institution, the Geological Survey. And if the activity of the topographer is separated from that of the geologist, yet a lively intercourse exists between both as result of the association in work. In the arid regions of the far West, where the mountain structure is not hidden by dense vegetation, the map-maker may easily recognize the relation of the topographic surface to the stratigraphic structure, and the geologist sharpens his perception of the forms of the earth's surface, since he must often do some topographic mapping. Geomorphology owes its more recent advances, in no small degree, to the far West. G. K. Gilbert, as result of his work there, has established a series of fundamental principles upon which others, especially W. M. Davis, have based further advances. The greatest service performed by Davis consists in the systematization of the complicated relation between the internal structure and the surface forms whose causes had already been explained in large part. Those portions of the earth's surface where a direct dependence of the surface forms upon the structure is recognizable, are young, and those where such a dependence is entirely lacking are old. The sequence in development, when elaborated in greater detail, is called by Davis the geographic cycle. It leads us from the stage of youth, where the elevated surface still predominates, through the stage of maturity, with its surface of adjustment, to the stage of old age with its surface of maximum resistance.

The dependence of the variety of forms presented by a land surface upon the history of that surface was in the early days recognized only with great hesitancy, but to-day, on the basis of the geographical cycle, may be stated with a certain amount of assurance. In the Rhenish Schiefergebirge, in Western Germany, we have a mountain system of Alpine structure and of plateau-like expression. The region has passed through at least one complete geographical cycle; the inequalities produced by crustal movements were almost completely planed off before it was again furrowed with valleys. The latter process implies a reëlevation to have taken place, a change which was not produced by a folding of the strata, but rather in this case by a broad elevation of the whole region. If we imagine this elevation to continue, then the valleys will cut in deeper, and the interstream areas become more and more portions of the valley slopes. An instant in geographic time will come when the plateau surface will disappear entirely and be replaced by mountain ridges whose constancy of elevation over long distances will be the only indication of the former plateau condition. The constancy of elevation of mountain summits is one of the most striking facts which characterize many widely differing mountain ranges of the earth. If, on account of its ambiguity, we do not venture to connect this feature in general with an earlier complete denudation of the region, followed by more recent reëlevation, yet in certain cases, as has already been demonstrated for a number of mountain ranges, this interpretation does hold good.[1] The Alps, which have heretofore served as an example of a mountain range which has originated by horizontal compression, may be shown to have last undergone a vertical elevation in their western portion.

In our opinion the foldings, which are so important for the tectonic structure of the crust, play a considerably smaller part in determining the physiognomy of the earth's surface than do the vertical movements of the crust. In building the great highlands of the earth, the latter agencies have decidedly the chief rôle. It is true that this fact has been proven, so far, only for the highlands of North America. In the Colorado Plateau of Arizona great fault blocks of nearly horizontal strata predominate. The great elevated masses have nothing to do with folding, and one speaks involuntarily of a broad elevation, since here extensive masses have been brought above sea-level. From a physiogeographic standpoint we cannot determine whether this elevation is identical with a centrifugal movement of the mass with reference to the earth as a whole; the answer to this question lies in the hands of geodesy and geophysics, which alone may with certainty determine the degree of mobility possessed by the different levels of the earth's crust.

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[1] Davis, Bailey Willis, and de Martonne brought a number of such cases to the attention of the eighth International Geographical Congress, Washington session.

The latter are certainly not rigid, but so long as we do not know the degree of contraction suffered by the terrestrial sphere during the course of geologic time, we never go further in geomorphological considerations of elevations and depressions than to refer them to sea-level, since the position of this surface determines all physiographic work upon the earth's surface. From the geomorphological standpoint one cannot say more than that the physical surface of the earth, however mobile it may be with reference to sea-level, possesses certain peculiarities which we cannot assume it to have acquired during present times alone. Among these peculiarities the one of most significance is its geometric characteristic as a complex of slopes. This has a physical basis in the small strength of the rocks as compared to the attraction of gravity. Even where we see steep projections, these show themselves to be transitory, they fall down and form slopes entirely without the coöperation of running water. We may, therefore, consider that it is impossible that there ever have been such overhanging rock-masses at the earth's surface as might correspond to the recumbent folds exhibited in mountain-masses. Such bold stratigraphic folds can only have originated at considerable depths. The processes of folding, whose products we meet with in folded mountain ranges, appear to us to be deep-seated, a conclusion already reached twenty years ago by G. K. Gilbert also. No doubt these folds were represented on the surface by other results which must have been superficial and could not have extended down to an unlimited depth, for a stratigraphic folding of the rocks indicates simply a decrease in area which is not connected with a decrease in volume, since rocks are but slightly compressible. The rocks must spread themselves out upward or downward to correspond with the compression that they suffer by folding, and since this redistribution of mass has taken place only in moderate amount, the processes of folding must have been limited to comparatively thin layers.

A second important peculiarity of the land surface is that its greater features present a surface of isostatic equilibrium. Measurements of the length of a geographical degree and pendulum observation have long since shown that the elevated masses upon the earth's surface are compensated by decrease of mass below them. The whole surface of the earth is found to be in an equilibrium similar to that which would characterize the surface if it consisted of a number of floating blocks of different densities, the less dense not sinking as deeply as the more dense. Dutton has characterized this condition by the name isostasy. It reveals an intimate relation between the greater forms of the earth's surface and the density of the subjacent masses. We know nothing of the more immediate cause of this relation, but we have no reason to doubt that it has always obtained. At first glance the theory of isostasy appears as a powerful support for the frequently voiced doctrine of the permanence of the continents, for it suggests the idea that those masses which to-day are light and, therefore, stand high, have always been so. But it is difficult to conceive how masses that once lay deeply buried, and were, therefore, heavy, now stand high and appear light. The Colorado Plateau of Arizona gives us an example of this. It once formed the sea-floor, and to-day is a highland of horizontal strata. We can only explain its elevated position by assuming that the masses lying under the crust have there suffered a shifting, that the foundation upon which the rock crust floats has changed. The regular change in the character of the products of successive volcanic eruptions which has been proved to occur in many places on the earth's surface seems to argue in favor of the existence of such magmatic movements.

Since shiftings of the magma lead to changes in the relative elevations of different portions of the earth's surface, they may also lead indirectly to independent movements of the upper portions of the earth's crust. Such movements must take place if high-lying crustal blocks come to rest alongside of low-lying blocks. If one portion of the crust is brought into such a position that it slopes steeply down to a neighboring, lower-lying one, as the result of vertical dislocation, then horizontal movements will be set up in the crust just as they are in a mass of soil which has been heaped up steeper than its normal angle of repose. E. Reyer many years ago compared the foldings of the earth's strata with the phenomena which appear where great slidings have taken place. In fact, there is no lack of evidence that vertical movements have preceded the horizontal ones. Many folded districts of the earth's crust, where the strata exhibit the multiple phenomena of compression rather than the rarer regular folding, originated in districts of subsidence in which enormous masses of sediments were deposited contemporaneously with the shrinking. In such cases it appears that the mountain-making elevation did not immediately succeed the compression of the strata. The Appalachians were folded long before they appeared as mountains. In other cases, also, as, for example, the Alps, the elevatory processes have followed the compression. It is conceivable that a recent change in the subcrustal masses has caused the formerly sunken land to rise again.

One can also imagine that a deep-seated, continuous folding may express itself at the surface as a low, dome-like elevation. In any case it is certain that we must not interpret all crustal movements as the direct results of a single, universal process. One needs only recall the close association of vertical crustal movements and the areas of ancient glaciation, in order to have an illustration of the fact that phenomena of elevation and subsidence may result from physical changes in the uppermost layers of the earth's crust. One must choose from among several explanations, and this can only be done with certainty when both structure and diastrophic conditions are clearly in mind, so that the whole geographic history of the mountain system may be passed in review.

We shall content ourselves with the foregoing suggestions. They are meant to point out directions in which the genetic study of the forms of the earth's surface may yield results of significance to geophysics and to the study of the earth's development, as soon as it is founded upon a broad basis and brought into sympathy with all its neighboring sciences. But without doubt the richest results are to be looked for from the recognition and appreciation of the forces at work upon the earth's surface, the more detailed study of which belongs to the dynamic problems of physiogeography.

All the movements which take place upon the surface of the earth, the winds, rivers, surf, and glaciers, all stand in intimate relationship with one another, all are dependent upon the forms of the earth's surface and react upon them. This mutual reaction extends also to the organic life of the globe, and although it is not the task of physiogeography, but of biogeography, to investigate the distribution of organic forms, yet a physiogeographic study cannot overlook the influence which the association of biologic processes exerts upon the forms of the earth's surface. Examples of this influence are found in the protection of the earth's surface against erosion by the cover of vegetation, and in the widespread coöperation of organisms for the formation of sediment, *e. g.*, the work of the reef-building corals. It is, however, precisely these biologic communities of geomorphologic significance, which combine with the peculiarities of surface and soil and the activities of the water and the atmosphere, to determine the physical expression of the lands. It is the general investigation of these communities which teaches most clearly which portions of the neighboring sciences belong also to physiogeography. The latter must take from each enough to gain an understanding of the physical features as a whole of the different countries in order to understand the local interlocking of the different phenomena in their "causal nexus." Physiography does not attempt to found laws within the domain of these sciences, but takes already established laws from these sciences and applies them. The result is the discovery of definite relation between these other processes and the surface forms of the earth; and of certain rules according to which the surface forms perform definite functions. In these facts originates that comparison of the earth's surface to an organic being which has so often been made since the time of Karl Ritter. In order to understand this functional significance of the various portions of the earth's surface, we may take climate as an illustrative example.

The waters are collected in the great hollows of the earth's surface, and their uninterrupted surface presents strikingly to the eye, not only the contrast between a smooth, level surface, and the physical surface of the earth, but also a contrast due to two different kinds of surfaces which are differently affected by the warming influence of the sun. We refer to contrast between continental climate and marine climate, but the sea climate is not characteristic of every portion of the ocean surface, and the continental climate does not distinguish all portions of the land. The surface must possess a certain amplitude in order to exert a climatic influence. The small islands in the ocean and the majority of the lakes on the continents have the same climate as their greater surrounding region. The different thermal behavior of the land areas and the water areas disturbs the regular distribution of atmospheric pressure which would otherwise characterize a rotating spheroid with a homogeneous surface character, and calls into existence, in addition to the dynamic wind system, a system of terrestrial winds ranging from the small land and sea breezes to the great monsoons. In this case, the relative positions of the areas, as well as their extent, exerts a dynamic influence. The monsoons blow far out beyond the boundaries of the Asiatic continent, and reach even beyond its great island neighbors. Finally, the vertical extent of the land-mass becomes a controlling element. In ascending we notice a regular decrease of temperature, which goes on more rapidly if the elevation is needle-shaped than if it covers a greater area. Although the wind may blow for a considerable distance over low, flat land without losing much of its moisture content, yet it surrenders the latter quickly where a mountain range compels it to ascend. How different are the climates of Europe and western North America! yet that is only because the winds blow against mountain ranges whose axes have different directions. In the one case the oceanic climate, carried by the winds, makes itself felt far inland; in the other case, the continental climate reaches far westward under the lee of the Rocky Mountains. In meteorological processes the *size* and *relative positions* of the forms of the earth's surface are not less significant than their geographical latitude. Both coöperate to determine the climate of the individual regions of the earth.

The organic world of the land's surface is most intimately dependent upon the latter's climate. It is true that a definite flora and fauna does not characterize every climate; but the relationship is unmistakable from the quantitative point of view. Almost everywhere on the earth's surface we find a dense covering of vegetation corresponding to a certain quantity of light, warmth, and moisture, and this covering becomes thinner, in proportion as the warmth or the moisture of the country becomes less. The plant formations mirror the most widely varying combination of climatic elements, and since the latter are dependent upon the extent and size of the forms of the earth's surface, so these latter geographical facts may be traced in the varying density of the vegetal covering of the lands.

Finally, we find that there is the very closest relation between the climate and the minuter modeling of the earth's surface. All the modeling forces which work upon the latter are under climatic influence; the running water and the powerful masses of ice of the glaciers are both products of climate, and the universally present wind can work most effectively where an arid climate causes the absence of the protective covering of plants. Where a single river, such as the Colorado, is eroding, the valley forms are different from those where their slopes are regularly moistened, so that the débris creeps downward. Thus we find that climate, the density of the vegetation, and the finer features of relief of any country bear intimate mutual relationship, and are dependent upon the distribution and mass of the greater land forms, which exert a far-reaching influence.

This close relation between climate, density of vegetation and the finer land forms finds expression in a definite physiographic correlation, more or less completely corresponding to the position of any district. Very simple consideration of the subject shows us that different grades of development may exist. Imagine a land area emerging from the ocean. As soon as it appears above the surface of the water, it will acquire its appropriate climate, but a certain period of time must elapse before its appropriate covering of vegetation will develop, for it is only the density and not the existence of this cover that is dependent upon the climate. The development of such a cover presumes that germs and seeds shall reach the new land area. If other land areas lie in its neighborhood, this transfer will take place quickly, as, for example, we see the island of Krakatoa already clothing itself again in vegetation, after the fearful explosion of 1883. On the other hand, if other land areas are widely removed from the new one, a very long period of time may elapse before it receives the elements of its appropriate flora. Yet, from the geological point of view, this reclothing takes place in a rather short time. This is illustrated by the lonely group of the Kerguelen Islands, which during the glacial period was wholly covered with ice, and has developed since that time a flora peculiar to itself.

Further, the new land, almost as soon as it has appeared above the waters, begins to be sculptured by the exogene processes. These processes may, in the one case, be hindered by reason of the rapidly extending cover of vegetation; on the other hand, they may be able to work for a long time undisturbed by any hindrance. In general, the exogene processes require a much greater interval of time to subdue a land surface than do the plants in order to cover it with a dense cloak of vegetation. The Kerguelen still show the surface relief imposed upon them by their glaciation, and running water has not yet been able to remodel them. The Alps, which furnish us with a splendid example of the adjustment of vegetational covering to the zones of elevation, still betray in all their features the glaciation to which they were subjected during the glacial period. Thus we find that the morphologic adjustment of a country to its structure and its climate, which indicates that it has reached the mature stage of the geographic cycle, succeeds to the physiogeographical stage of maturity by a very considerable interval of time, and in order to produce a complete physiographic correlation between the two, it requires an amount of time which must be measured by geologic units. This correlation, however, will not be reached in the same way under all conditions. In many cases aqueous erosion works so vigorously that it destroys an existing dense covering of plants, producing ravines and gorges which remain almost barren of vegetation because of their steep walls, and must give, even to a non-geographical observer, the impression of a disturbing attack upon an otherwise harmonious set of conditions.

Simple as physiogeographic correlation is, in its systematic relationships, yet it leads to an extraordinarily large number of individual cases which call for regional consideration. It forms the chief approach to the physical science of the land. The latter, for its part, cannot leave out of consideration the human element, since Man revolutionizes the plant covering of the land, controls the rivers, and influences the relief of the land by means of his roads and settlements. Thus regional physiogeography is closely related to biogeography.

The total natural features of any region we have seen to depend, not only upon its own peculiarities and absolute position, but also to a controlling degree upon its position with relation to other land areas. Fundamental changes in any land make themselves felt far beyond its boundaries. If a lowland along the boundaries of a continent become submerged below the sea, as may result from a very slight crustal movement, then the influence of the oceanic climate reaches much farther inland. This is excellently shown by the climatic advantages which Europe draws from the presence of the North Sea and the Baltic. Alterations in the lands disturb the physiogeographic correlation in their vicinity, and for a long time we were inclined to ascribe all changes of climate to telluric processes. The studies of the glacial period no longer allow us to retain these theories. But there remains the attractive physiogeographic problem to determine the number of possible changes of climate which may result from geomorphologic causes.

If, on the one hand, changes in the distribution and mass of the great forms of the earth's surface produce far-reaching results in the natural features of the surrounding lands, and if the finer modeling of broad areas of land may be wholly changed because they have been transferred from a region of dry, continental climate into that of the oceanic climate without having changed their position with reference to the earth as a whole, on the other hand, the forces at work on the earth's surface also exert an undeniable influence upon the greater features in the surficial forms of our planet. The destroying power of flowing water works most vigorously where the greatest inequalities exist, not only because the water here has the greatest distance to fall through, but because the great increase in precipitation resulting from those inequalities produces a greater mass of running water. Climatic controls result in climatic divisions or boundaries. Bays become silted up by the rivers, promontories are worn away by the attack of the ocean; thus the horizontal arrangement of the land areas is disturbed, which is an extraordinarily important factor from the physiogeographic standpoint. Under the influence of the exogene forces all the abrupt contrasts of form disappear, and at the same time the causes for the yet sharper contrasts in the organic phenomena of the lands are removed. In this respect also it will become possible to establish a sequence in development similar to the sequence in morphologic development, and to place alongside of the geographic cycle of Davis a physiogeographic cycle. The final stage of this physiogeographic cycle would present a complete adjustment between the forms of the earth's surface and all those exogene processes which are active upon it.

If, now, we review the earth's surface, whether from the morphologic or from the physiographic standpoint, it is clear that taken as a whole it is very far removed from the stage of old age. Everywhere we notice traces of young crustal movements which are repeatedly and abruptly disturbing that adjustment which the exogene forces are constantly endeavoring to establish. Although we may readily observe the theater of action of the exogene forces, and easily recognize their distribution over broad areas, we are still in the depths of darkness as regards knowledge of the distribution of the disturbing endogene forces. It has been repeatedly attempted to find some sort of relation between these forces and the earth as a whole. The French investigators more especially have referred sometimes to a dodecahedron and sometimes to a tetrahedron which should express the ground-plan of the earth's crust. The location of mountain ranges along both flanks of the continents of North and South America has given rise to an attempt to establish a relation between the distribution of mountain ranges and the outlines of the continents. Great weight has also been laid upon the occurrence of volcanoes along the edges of the continental blocks. Edward Suess, who has undertaken the correlation of the different portions of the crust, has emphasized the relations existing between folded mountain ranges and the massive rocks which stand in front of them. Although studies along the latter line have yielded some very pretty isolated results, yet it is not safe to assume that these various attempts have revealed to us the ground-plan of crustal movements with respect to the degree of elevation or depression and the strike of the zone of compression. Indeed, it seems as though the analytic method heretofore pursued will not accomplish anything until we are able to deduce a mental picture of the sequence of processes in the development of the zones of compression. The contrast between the structure of such zones and the structure of the bordering regions invites the application of such a method.

In this respect we may hope for considerable enlightenment as the result of further investigations in geophysics. It is easy to see that the modeling of the earth's surface influences the radiation of heat by the earth quite as strongly as it does the warming of the earth by absorption of external heat. The water collected in the great basins of the ocean reacts upon the warm earth body as a cooling apparatus on a large scale, while the elevation of the land performs the functions of a protecting cover, now thicker, now thinner, which tends to prevent the loss of heat. Systematic investigations, on islands as well as mainland, into the geothermal gradients would give us a basis for quantitative knowledge regarding the influence of the surface features upon the radiation of heat by the earth. Just as the surface isotherms, which Humboldt first drew for us, gave us the first clear conception of the climate of the earth, so must we look to geographic presentation of the isogeotherms for a reliable understanding of the intracrustal distribution of heat. These would serve as a basis for further investigations, provided geophysical investigations continue to clarify our understanding of the conditions prevailing in the abysmal masses. The problem of the crustal movements of the earth can only be solved, if we may obtain in addition to its complicated composition the relation existing between the earth's crust and the overlying and underlying masses. This can only take place if physiogeography, geology, and geophysics coöperate as heartily in this line of investigation as have astronomy, meteorology, and physiogeography in the study of the exogene processes.

Whatever the solution of this problem may be, there will yet remain a further question. When we compare the total amount of erosion going on over the land with the total amount of rock which was formed at the expense of preëxistent continents, we find that the latter is far greater than the former. According to the present intensity of erosion and denudation, it would require an incomprehensibly large number of millions of years in order to produce a volume of rock equal to that now comprised in the sedimentary series. Estimates of the time elapsed since the earth, under present physiogeographic conditions, has been the theater of the processes to-day active upon the earth's surface, lead us by other routes to the same conclusion. The influences of the sun's rays upon the exterior of our planet have been felt for an incomparably long time, and we cannot assert that there is any sensible decrease of their intensity. Yet the sun's energy cannot be inexhaustible. Here we find a lack of harmony, according to the present state of our knowledge, between cause and effect, which is much in need of explanation. Questions brought up by the physiogeographic method of studying the earth are appealing, not only to astronomy, but to astrophysics.

Thus our point of view passes from the earth's surface to the earth as a whole, and from the earth as a whole to the sun, just as soon as we begin to compare the phenomena which are taking place upon our planet with the work performed by him. The broader the circle to which we turn with questions, the greater the number of problems which present themselves. We are thus more and more strongly compelled to acknowledge that the key to success lies in an organized coöperation among the different sciences, and any hard-and-fast barring-off of the one from the other, or even the contemptuous disdain of one by another, will have evil results. It is true that they have different refinements of method, but all problems do not permit of a mathematical treatment, and it is also true that at times the one may make such a marked advance that it grows over the heads of the others and is able from its more advanced standpoint to point out the direction along which the others must develop. But in the long run they must all advance evenly together as long as they stand on the firm foundation of their individual fields of observation. Physiogeography has such a field of observation in the land surface, since it considers that as the surface upon which light and heat fall from without, and through which the warmth of the earth's body must pass from within outwards.

The position occupied by geography among the other sciences has been the subject of many discussions during the past decade; and there is, especially in German, a rather large literature on the subject. It was not my intention, in the above address, to refer to all the expressions of opinion on the subject, nor do the works listed below by any means always hold the same position as the one established in that address. They are here brought together without any intention of being a complete list, but merely with the purpose to furnish the reader a means of orienting himself.

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PHYSIOGRAPHIC PROBLEMS OF TO-DAY

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In looking ahead and endeavoring to see in what ways our knowledge of the earth's surface can be increased, the fact should be borne in mind that physiography is one of the younger of the sciences. In truth, the new geography, or physiography, as it has been christened, is of such recent birth that its limits and its relationship to other sciences are as yet, in part, indefinite. Accepting the conservative view, that physiographers should confine their studies to the earth's surface, but have freedom to investigate the causes producing changes of that surface, whether coming from without or arising from forces at work within the earth, my task is to suggest ways in which man's knowledge of his dwelling-place may be enlarged.

Inheritances

Although the scientific study of the earth's surface can with sufficient accuracy be said to be less than a century old, and to have attained the greater part of its growth during the past half-century, the fact must be freely admitted that, preceding the recognition of physiography as one of the sisterhood of sciences, there was a long period of preparation, during which man's physical environment, and the many changes to which it is subject, attracted attention and awakened interest. The more or less general and diffuse descriptions of the earth's surface embraced under the term "physical geography," when vivified by the idea of evolution, became the more definite and concrete physiography of to-day. Physiography from this point of view may perhaps be justly designated as scientific physical geography. New thoughts grafted on the previously vigorous stem have borne rich fruits, but in many instances inherit much of their flavor from the original trunk. One of the important duties of the physiographer is to select all that is of value from the inheritance that has come to him, whether of fact, or theory, or suggestion, and give it a place in his systematically classified records.

In the physical geographies on our library shelves, in books of travel, in transactions of learned societies, etc., pertaining to the era preceding the time when physical geography became a science, there are numerous records of facts, concealed, perhaps, in part in dreary cosmogonies and exuberant theories, which in many instances are of exceptional value because, in part, of the date at which they were observed. One of the leading ideas in scientific geographical study is the recognition of the wide-reaching principle that changes are everywhere in progress. Many, if not all, of the changes referred to have an orderly sequence, and constitute what may be suggestively termed life-histories. In writing the biographies of various features of the earth's surface the observations made a century, or many centuries, ago have a peculiar, and in some instances an almost priceless value, because of the light they furnish as to the sequence of events. In this and yet other ways, the records left by past generations of geographical explorers contain valuable legacies. In attempting to winnow the wheat from the chaff of physical geography, the physiographer should avoid the conceit of youth, and fully recognize the work of the bold and hardy pioneers who blazed the way for the more critical and better-equipped investigators who came later.

Nomenclature

One of the reasons for the slow growth of knowledge concerning the earth's surface during the centuries that have passed was the fact that the objects which claimed attention were, to a great extent, designated by terms derived from popular usage. The language of geography, in large part of remote antiquity, was adopted from the parlance of sailors, hunters, and others in the humbler walks of life, and retained its original looseness of meaning. The change from geographical description to scientific analysis, which marked the birth of physiography, necessitated greater precision in the use of words. This change is not yet complete, and physiography is still hampered in its growth and usefulness by a lack of concrete terms in which tersely and accurately to state its results. In the nomenclature of physiography to-day the words inherited from physical geography by far outnumber the technical terms since introduced, and to a large extent still retain the indefiniteness and lack of precision that characterize the multiple sources from which they were adopted. One of the pressing duties of the scientific student of the earth's surface, and one which on account of its many difficulties may well be reckoned among the physiographic problems of to-day, is the giving of fixed and precise meanings to the words employed in describing and classifying the features of the earth's surface. A scientific physiographical nomenclature is of importance, not only to the special students of the earth's surface, but through them to communities and patrons. The diverse interpretations that have been given to such seemingly simple terms as "shore," "lake," "river," "hill," "mountain," "divide," etc., as is well known, have led to misunderstandings, litigations, international disputes, and even threatened to bring on war between highly civilized nations. A duty which physiographers owe, not only to their science in order that its continued advancement may be assured, but to communities in payment for the terms borrowed from them, as well as for the general good, is a systematic effort to define accurately the words and terms now used to designate the features of the earth's surface. Careful attention needs to be given also to the coinage of new terms when their need is definitely assured. An appropriate task for a group of physiographers would be the preparation of a descriptive geographical dictionary, suited to the wants of both the specialist and the layman.

While considering the advantages of a language of science, its disadvantages should also be recognized.

The histories of all sciences show that, as they became more and more precise, and as their nomenclature grew so as to meet their internal requirements more and more completely, they at the same time, on account of the very precision and accuracy of their language, became more and more circumscribed and farther and farther removed from the great mass of humanity for whose use and benefit they exist. Not only this, but a science dealing with facts of vast public importance and filled with instructive and entertaining matter — nay, in itself even poetic and as fascinating as the pages of a story-book — has, in not a few instances, been rendered difficult to understand, and even repellent to the general reader, by a bristling array of esoteric terms built about it like an abatis.

Between the two extremes, — on the one hand, a science without words in which to speak concisely and accurately, the condition in which the physiographer finds himself at the present time; and, on the other hand, a science with a language so technical and abstruse that it seems a foreign tongue to the uninitiated, — is there not a happy mean? Such a much-to-be-desired end seems to be within the grasp of the physiographer. By giving precision to and defining the bounds of words inherited from physical geography, and adding to the list such terms as are strictly essential in the interest of economy of time and space, or for accuracy, — such contributions, so far as practicable, to be chosen from the language of every-day life, — it would seem as if a nomenclature could be formulated which would at the same time meet the requirements of the scientific student and enable the general reader of average intelligence to receive instruction and inspiration from the talks and writings of the especially qualified interpreters of nature.

Exploration

Physiography, to a great extent, is still in the descriptive stage of its development, but the descriptions demanded are such as discriminate and select the essential, or suggestive, from the confusing wealth of secondary details frequently present. The records should also include comparisons between the objects described and analogous topographic or other physiographic features, and, within safe and reasonable limits, be accompanied by explanations of their origin and life-histories.

One of the important functions of physiography, as a more mature growth of physical geography, is to continue and render more complete the exploration of the earth's surface and to conduct resurveys where necessary. Geographical exploration has, as is well known, been carried on vigorously, although spasmodically, in the past, and the areas marked "unknown" on our globes have become smaller and smaller, and more and more isolated. The more critical physiographic studies, however, which have for their object not only the description of coast-lines, mountain ranges, plains, etc., but a search for the records of their birth, the discovery of their mode of development, and their assignment to a definite place in the complex whole, termed man's environment, has progressed but slowly. In this stricter sense, the unknown areas on the earth's surface embrace regions of continental extent. It is this latter method of geographical exploration and survey which now demands chief attention.

The terms "exploration" and "survey" are here used advisedly, as two divisions of physiographic field-work may justly be recognized. These are: first, travel in which physiographic observations are incidental to other aims, or perhaps the leading purpose in view, as during a physiographic reconnoissance; and, second, detailed surveys and critical study of definite areas or of concrete problems. Each of these subdivisions of the great task of making known the beauties and harmonies of man's dwelling-place has its special functions.

From the observant traveler we expect comprehensive and graphic descriptions of the regions visited, rendered terse by the use of well-chosen terms, in which the more conspicuous elements of relief, and other physiographic features, and their relation to life, shall be clearly and forcibly presented. In order to render this service, the traveler should not only be familiar with the broader conclusions and fundamental principles of physiography, but skilled in the use of its nomenclature. The chief contribution to the science of the earth's surface demanded of the explorer of new lands is a careful record of facts. When a journey becomes a reconnoissance, with physiography as its leading feature, it is not only an advance into a more or less unknown region, but an excursion into the realm of ideas as well. It is during such explorations, when one's mind is stimulated by new impressions, that hypotheses spring into existence with greatest exuberance. While most of these springtime growths are doomed to wither in the more intense heat of subsequent discussion, their spontaneity, and the fact that the mind, when not oppressed by a multitude of details, grasps significant facts almost by intuition, make the suggestions of the explorer of peculiar value.

The detailed work of physiographic surveys falls into two groups: namely, the study of definite areas, and the investigation of specific problems. In each of these related methods the desirability of recording facts by graphic methods is apparent. The demand for accurate maps as an aid to both areal physiography and the study of groups of specific forms, or the functions of concrete processes, needs no more than a word at this time. With the growth of physiography the time has come when the work of the individual explorer, who from force of circumstances endeavors to follow many of the paths he finds leading into the unknown, is replaced to a large extent by well-organized and well-equipped scientific expeditions. It is from such systematically planned campaigns, in which the physiographer and representatives of other sciences mutually aid each other, that the greatest additions to man's knowledge of the earth's surface are to be expected. The most extensive of the unexplored or but little-known portions of the surface of the lithosphere, in which a rich harvest awaits the properly equipped expedition, are the sea-floor and the north and south polar regions. As is well known, splendid advances have been made in each of these fields, but, as seems evident, much more remains to be accomplished.

In the branch of physiography appropriately termed "oceanography" the problems in view are the contour of the sea-floor, or its mountains and deeps, plains and plateaus, the manner in which each inequality of surface came into existence, and the various ways it is being modified. In both of these directions the interests of the physiographer merge with those of the biologist and the geologist. One phase of the study of the ocean's floor which demands recognition is that the topographic forms there present are such as have been produced almost entirely by constructional and diastrophic agencies, free from complications due to erosion which so frequently obscure the result of like agencies on the land. For an answer to the question: What would have been the topography of land areas, had there been no subaërial decay and denudation? the topography of submarine regions furnishes at least a partial answer. The sounding-line in the Caribbean region has furnished examples of topography due, as it seems, mainly to differential movements of blocks of the earth's crust bounded by faults, which have not been modified by subaërial denudation. In a similar way, as is to be expected, a survey of other portions of the at present water-covered surface of the lithosphere will supplement our knowledge of thee merged portions of the same rock-envelope, and assist in an important way in the deciphering of their histories.

In the Arctic and Antarctic regions, where all is unknown, systematic research may be expected not only to extend many branches of physiographic study, but to bring to the front as yet unsuspected problems.

The larger of the unexplored regions of the earth, however, are not the only portions of our field of study that demand attention. New ideas, new principles, and fresh hypotheses make an unknown country of the most familiar landscape. The definite formulation of the base-level idea, the suggestive and far-reaching principle embraced in the term "geographic cycle," the planetesimal hypothesis as to the origin of the earth, etc., furnish new and commanding points of view, or, as they may be termed, primary stations in the physiographic survey of the earth's surface, by means of which previous local surveys may be correlated and corrected.

In the search for problems, the unraveling of which may be expected to advance the scientific study of the earth's surface, the writings of travelers, the pregnant suggestions of those who make reconnoissances into the realm of unknown facts and of unrecognized ideas, as well as the precise and accurate pictures of portions of the earth's surface presented on the maps of the topographer and the charts of the oceanographer, point the way to still greater advancements, and furnish inspiration to those who follow.

Fundamental Concepts

While physiography deals with the surface features of the earth, the fact that in those features is expressed to a great extent the effects of movements originating deep within the earth leads the student of continents and oceans to ask of the geologist and the physicist puzzling questions as to the changes that are taking place in the great central mass of our planet, and even in reference to the origin of the earth itself. So intimately are the various threads of nature-study interwoven that the full significance of many of the surface features of the earth cannot be grasped and their genesis explained until the nature and mode of action of the forces within the earth which produce surface changes are understood.

The growth of physiography up to the present time has been largely influenced by the far-reaching ideas of Laplace and others in reference to the nebular origin of the solar system. In all of the questions respecting secular changes of land areas in reference to the surface level of the ocean, the origin of corrugated and of block mountains, the fundamental nature of volcanoes, etc., the controlling idea has been that the earth has cooled from a state of fusion, and is still shrinking on account of the dissipation into space of its internal heat.

With the recent presentation of the planetesimal hypothesis by Professor Chamberlin, a radically different point of view is furnished from which to study the internal condition of the earth. The new hypothesis — which has for its main thesis the building of a planet by the gathering together of cold, rigid, meteoric bodies, and the compression and consequent heating of the growing globe by reason of gravitational contraction — is suggestive, and seems so well grounded on facts and demonstrated physical and chemical laws that it bids fair not only to revolutionize geology, but to necessitate profound changes in methods of study respecting the larger features of the earth's surface. One of the several considerations which make the planetesimal hypothesis appeal forcibly to the inquiring mind is that it employs an agency now in operation, namely, the process of earth-growth through the incoming of meteoric bodies from space; and for this reason is welcomed by uniformitarians, since it is in harmony with their understanding of a fundamental law of nature.

In many, if not all, questions respecting the origin of the atmosphere, the ocean, continents, mountains, and volcanoes, and the secular, and to a marked extent, in certain instances, the daily changes they experience, it is evident that the planetesimal hypothesis necessitates a revision, or at least a review, of some of the fundamental conceptions held by physiographers. The objection will perhaps be advanced that to make such a radical change of front on the basis of a young and as yet untried hypothesis is not wise. The reply is that the older hypothesis has been tried and to a marked extent found wanting, and that the new conception of the mode of origin of the earth demands consideration, not only as affecting a large group of basement principles of interest to the physiographer, but with the view of testing the planetesimal hypothesis itself by physiographic standards.

The problems interlocked with the mode of origin of the earth, in which the physiographer shares an interest with the geologist, are the rate at which the earth's mass is now being increased owing to the ingathering of planetesimal, and the chemical and physical and perhaps life-conditions of the incoming bodies; the temperature of the earth's interior and the surface changes to be expected from its increase or diminution; the results of gravitational contraction in reference to movement in the earth's crust; the extrusion of gases and vapors from the earth's interior, and the resultant changes in progress in the volume and composition of the atmosphere and hydrosphere. In these and still other fundamental conceptions of the primary causes of many of the changes in progress on the earth's surface the planetesimal hypothesis seemingly furnishes the corner-stone of a broader physiography than has as yet been framed.

Ideal Physiographic Types

During the descriptive stage of the study of biology the relationships among plants and animals were the chief end in view, and as a result of the conditions confronted, a systematic classification of animate forms under species, genera, families, etc., was formulated, which has been of infinite assistance during the more philosophical investigations that followed. Biological classification was facilitated, as learned later, by the fact that with the evolution of species there was concurrent extinction of species. As the tree of life grew, its branches became more and more widely separated.

Throughout the many changes the surface features of the earth have experienced, there has also been development, not of the same grade, but analogous to that recognized in the realm of life; but the process of extinction has been far less complete than in the organic kingdom, and the connecting links between the various groups of topographic and other physiographic forms produced have persisted, and to a conspicuous extent still exist. The task of arranging the infinitely varied features of the earth's surface in orderly sequence, or systematic physiography, is thus far more difficult than the similar task which the flora and fauna of the earth present.

The utility of classification is fully recognized by physiographers, and various attempts have been made from time to time to meet the demand, but thus far without producing a generally acceptable result. Remembering that a scheme of classification of topographic and related forms is to be considered as a means for attaining a higher end, namely, the history of the evolution of the surface features of the earth, and should be of the nature of a table of contents to a systematically written treatise, the task of preparing such an index becomes of fundamental importance to the physiographer. Since extinction of species among physiographic features has probably not occurred, and connected series of forms which grade one into another confront us, the practical lesson taught by the success of schemes of biological classification seems to be that ideal physiographic types should be chosen correlative with species among plants and animals.

By "ideal physiographic types" is meant complete synthetic examples of topographic and other physiographic forms, which will serve the rôle of well-defined species in the study of the surface features of the earth. Ideal types may be likened to composite photographs. They should combine critical studies of many actual forms, within a chosen range, and in addition be ideally perfect representatives of the results reached by specific agencies operating under the most favorable conditions. Like the idealized personalities of history and religions, the types of physiographic forms might well be more perfect than any actual example. When such idealized types shall have been chosen after careful study, described with care, and illustrated by means of diagrams, maps, pictures, models, etc., a comparison with them of actual examples on any portion of the earth for the purpose of identification and classification would be practicable. A well-arranged catalogue of ideal types would be an analytical table of contents to the history of the evolution of the features of the earth's surface, and constitute a scheme of physiographic classification.

In illustration of what is meant by an idealized physiographic type: We find in nature a great variety of alluvial deposits, now designated as alluvial cones or alluvial fans. They present a wide range and infinite gradations in size, shape, composition, structure, angle of slope, degree of completeness, stage of growth or decadence, etc. Complications also arise because of the association and intergrowth of such alluvial deposits with other topographic forms. In constructing the ideal type the characteristics of many of the most perfect actual alluvial cones, aided by a study of the essential features of similar artificial structures, should be combined in an ideally perfect and representative example which would serve as the type of its species. All actual examples might be compared with such a type, their specific and generic relations determined, and their individual variations noted. In like manner, other topographic forms, ranging from the more concrete species — such as constructional plains, cinder-cones, sea-cliffs, river-terraces, etc., to the more complex forms, as, for example, mountain ranges, mountain systems, and yet larger earth-features — could be represented by ideally perfect examples free from accidental and secondary complexities and accessories.

While individual examples of idealized topographic and other features of the earth's surface would serve as species, their arrangement under genera, families, etc., offers another problem, in which relationship or genesis should be the controlling idea.

The selection of idealized physiographic types, as just suggested, has for its chief purpose the reduction of endless complexities and intergradations to practicable limits. It is a method of artificial selection so governed that, while no link in the chain of evolution need be lost to view, certain links are chosen to represent their nearest of kin and serve as types. A danger to be marked by a conspicuous signal, in case this plan for aiding physiographic study is put in practice, is that it may tend toward empty ritualism. To give the idealized types chosen for convenience of classification an appropriate atmosphere, the fact that changes are constantly in progress — that mountains come and go even as the clouds of the air form and re-form — should be ever present in the mind.

The process of evolution without concurrent extinction which characterizes the development of physiographic features finds expression also in related departments of nature, as, for example, in petrography, where, as is well known, it has greatly delayed the framing of a serviceable and logical system of classification. Indeed, the principle referred to may be said to be one of the chief distinctions between the organic and the inorganic kingdoms of nature.

The Primary Features of the Earth's Surface

The primary features of the earth's surface may consistently be defined as those resulting from the growth and internal changes of the lithosphere, while the modifications of relief resulting from the action of agencies which derive their energy from without the earth may be termed secondary features. The primary or major characteristics of the earth's surface, so far as now known, may be ranked in three groups, in accord with the agency by which they were principally produced; namely, diastrophic, plutonic, and volcanic physiographic features. Each of the groups presents many as yet unsolved problems.

Diastrophic Features. Under this perhaps unwelcome term are included a large class of elevations, depressions, corrugations, faults, etc., in the surface portion of the lithosphere due to movements within its mass. The causes of the changes which produced these results are as yet obscure, and, although a fruitful source of more or less romantic hypotheses, may in general terms be referred to the effects of the cooling and consequent shrinking of a heated globe, or, under the terms of the planetesimal hpyothesis, reckoned in part among the results of gravitational condensation. However obscure the fundamental cause, the results in view are real, and among the larger of the earth's features with which the physiographer deals. They are the greater of the quarry-blocks, so to speak, which have been wrought by denuding agencies into an infinite variety of sculptured forms. Included in the list, as the evidence in hand seems to indicate, are continental platforms, oceanic basins, corrugated and block mountains, and many less mighty elements in the marvelously varied surface of the lithosphere. Not only the study of the shapes of these features of the earth's surface, but the movements they are still experiencing, and their transformations through the action of denuding agencies, claim the attention of the physiographer. While it may be said that the investigation of the method by which the primary relief of the lithosphere has been produced falls to the lot of the geologist or the geophysicist, the physiographer is also interested in the many profound problems involved. The geologist and physiographer here find a common field for exploration, and can mutually assist each other. The task of the physiographer is to describe and classify the elements in the relief of the lithosphere due to diastrophic agencies, discriminate them from deformations due to other causes, and restore so far as practicable the forms that have been defaced by erosion. He can in this way assist the geologist by presenting him with the results of diastrophism free from accessories. With pure examples of the forms produced, the geologist will be better able to discover the causes and their mode of action, which have produced the observed results.

Although much has been accomplished in the way of determining which elements in the relief of the lithosphere are due to diastrophic agencies, only a small part of the difficulties to be overcome have been met. The aim in view is the attaining of a knowledge of what would have been the shape and surface features of the solid earth, had there been no modifications by internal causes except diastrophism, and no changes in relief by erosion or other surface agencies. Included in this branch of physiography is the shape of the earth itself, in the study of which the physiographer becomes a geodesist. The earth's shape, and its primary surface features due to diastrophism, form the logical basis for physiographic study, in which ideal types of topographic forms declare their usefulness. In the geographical museums of the future, at the head of the long series of models of physiographic types illustrating the species, genera, families, etc., of the earth's surface features, should be placed ideal examples of the most typical elements of relief due to diastrophism.

Physiographers cannot rest content with the study of the shape of the lithosphere and of its surface relief, in which so much of the history of the earth is recorded, and refrain from searching for the deeper meanings these facts suggest, but must have freedom to invade the province of the geologist, the astronomer, the physicist, the chemist, and other subdivisions of the science of the cosmos, in search of truths bearing on their special line of work. This is particularly true in connection with the special department of physiography in hand, in which many of the branches of the river of knowledge have their sources.

Plutonic Features. Intimately associated with the irregularities of the earth's surface due to a decrease in its volume, and, as our reasoning tells us, dependent primarily on the same cause and at present only partially differentiated from them, are surface elevations and depressions, produced by the migration of portions of the earth's central magma from the deep interior toward or to the surface. A convenient but arbitrary subdivision of the matter forced outward from the earth's interior is in vogue among geologists, and rocks of plutonic and of volcanic origin are recognized. To the physiographer the distinction referred to is more suggestive than it appears from the point of view of the geologist, since the recognition of differences between topographic forms produced by the injection of fluid or plastic magmas into the cooled, rigid outer portion of the earth, and topographic forms resulting from the extrusion of similar matter at the surface, is of genetic significance.

The simile was used above between the quarry-blocks taken to the studio of the sculptor and the portions of the earth's surface brought by diastrophic movements within the sphere of influence of denuding agencies. There are two other primary classes of physiographic quarry-blocks; one produced by intrusions of highly heated plastic or fluid magmas into the earth's crust, which cause upheavals of the surface above them, and the other due to extrusions of similar material at the surface, as during volcanic eruptions. The first of these two series of earth-features has received much less attention from physiographers than the second series.

Surface elevations due to local intrusions are well illustrated by the reconstructed forms of the Henry Mountains and the similar information in hand concerning several other regions. The topographic forms referred to have a conspicuous vertical measure in comparison with their breadth of base, and their prominence gained for them earlier recognition than in the case of related, and in part far more important, plutonic changes. It is to be remembered, however, that every intrusion of a magma into the earth's crust is, theoretically at least, accompanied by a change in the relief of the surface above. What surface changes accompany the lateral movements in the rocks invaded by a dike has eluded search and seemingly escaped conjecture. The surface changes produced by an extensive horizontal injection of a magma, as when intruded sheets are found in stratified terranes, is a matter of inference rather than of observation. Intrusive sheets are numerous, and the surface changes in topography, and consequently of drainage, that accompanied their production must have been important, but definite examples are wanting. Critical studies are needed in this connection, both by physiographers and by geologists, in order that the widely extended movements which have been observed in the surface of the lithosphere may be referred to their proper cause. How do we know, for example, that the many recorded changes in the relation of the land to sea-level may not in part be due to the injection of magmas into the earth's crust, instead of diastrophic movements, as commonly supposed. The activity of volcanoes at the present day is warrant for the hypothesis that the concurrent process of sub-surface injection is still in progress, and is to-day producing changes in the geography of the earth's surface.

Of still more importance to the physiographer than the surface changes known, or legitimately inferred, to have resulted from the formation of dikes, laccoliths, and intruded sheets are the elevations and possibly concurrent depressions of the surface of the lithosphere caused by still greater migrations of portions of the earth's central magma outward and into or beneath the rigid surface rind. Concerning these *regional intrusions*, as they may be termed, the geologist has furnished suggestive information. We are told, for example, that the granitic rocks from which the visible portion of the Bitter Root Mountains in Idaho have been sculptured are intrusive. The now deeply dissected granitic core of this mountain range measures not less than three hundred miles in length and from fifty to over one hundred miles in width. The area occupied by intrusive granitic rocks in the Sierra Nevada is seemingly still greater than in the case just cited, and other regional intrusions of even mightier dimensions are known in the vast region of crystalline rocks in Canada and elsewhere. The covers of sedimentary or other material which formerly roofed these vast intrusions in the instances now open for study have for the most part been removed by denudation, although instructive remnants of metamorphosed terranes occurring as inliers in the granitic areas sometimes persist and reveal something of the nature of original domes of which they formed a part.

The surface changes in relief produced by the migration of magmas measuring thousands, and in many instances, as we seem justified in concluding, tens of thousands, of cubic miles, from deep within the earth outward, but failing to reach the surface, must be reckoned as of major physiographic importance. The very magnitude of the features of the earth's surface due to such intrusions has served to conceal their significance. We look in vain in our treatises on physiography for so much as a mention of them. Perhaps the excuse will be offered that the modifications in relief referred to are commonly grouped with the results produced by diastrophic agencies; but, if so considered, a differentiation seems necessary, and the significance of the topographic forms resulting from intrusions of various kinds clearly recognized.

In our dreamed-of museum of ideal physiographic types, mighty domes raised by regional intrusions, broad uplifts with perhaps sharply defined boundaries, elevated by relatively thin intruded sheets, as well as steep-sided domes with relatively small bases, concealing laccoliths, and the still smaller covers arching over plutonic plugs, will demand a place in the group of type examples of primary unsculptured elements in the relief of the lithosphere.

Volcanic Features. Elevations on the surface of the lithosphere due to the presence of material extruded from volcanic vents have long been recognized, but the specific, or, as perhaps may be consistently claimed, generic, differences among them have only recently claimed attention. Of primary importance in the classification of topographic forms of volcanic origin is the fact that volcanoes are both constructive and destructive in their action. Among the results of constructive action are included the changes produced by effusive, fragmental-solid, and massive-solid eruptions, each of which has furnished a wide range of primary topographic forms. The catalogue of recognized types includes lava plains and plateaus, cinder and lapilli cones, lava cones and domes, lapilli and dust plains, together with many minor structures, such as "spatter-cones," "lava-deltas," "lava-gutters," "lava-levees," and the various surface details of lava-streams due to the flow of still mobile magmas beneath a stiffened crust which ranged in physical consistency from a highly plastic to a rigid and brittle condition. With these more familiar forms are to be included also the results of massive-solid extrusions, of which the "obelisks" of Mont Pelée are the most striking examples.

Our present list of destructional topographic forms due to volcanic eruptions includes decapitated cinder, lapilli, and lava cones, and subsided and broken lava-domes, calderas, crater-rings, etc., together with cones of various kinds breached by outflowing lavas; and, as minor features, the floated blocks sometimes carried on lava-streams, or the *moraines of lava flows,* as they may suggestively be termed, the subsided and broken roofs of lava-tunnels, etc.

The interesting contributions made during the past decade to the list of topographic forms resulting from the action of volcanic agencies are highly suggestive, and warrant the belief that still more numerous and equally important results in the same direction will reward more extended and more careful search. The progress of physiography would evidently be accelerated by a systematic review and a more definite classification of the topographic forms, both constructional and destructional, known to have resulted from volcanic agencies, and a more critical selection of types to serve as species than has as yet been attempted. From such a catalogue something of the underlying principles governing the many ways in which the relief of the earth's surface has been modified, and is still being changed through the agency of volcanoes, would make themselves manifest, and predictions rendered possible which would facilitate further study. The analogy between lava-streams and rivers, on the one hand, and glaciers, on the other, suggests interesting and instructive methods for considering the entire question of the movements of liquids and solids on the earth's surface.

While the topographic changes produced by volcanic agencies are of chief interest to the physiographer, they lead him to profound speculations in reference to the nature of the forces to which they are due, the source and previous condition of the matter extruded during eruptions, and the study of the existing relations between the earth's interior and its surface. The great, and as yet but partially answered, questions: Whence the heat manifest during volcanic eruptions? What is the source of the energy which forces lava to rise from deep within the earth through volcanic conduits to where it is added to the surface, perhaps ten to twenty thousand feet above sea-level? and, What is the source or sources of the steam discharged in such vast quantities during eruptions of even minor intensity? are of as great interest to the physiographer as they are to the geologist, and furnish another illustration of the unity of nature-study. From the new point of view furnished by the author of the planetesimal hypothesis, the many questions the physiographer is asking concerning volcanoes and fissure or regional eruptions are rendered still more numerous by the suggestion that these fiery fountains are the sources from which the ocean and all the surface waters of the earth have been supplied. This startling revelation, as it seems, makes a still more urgent demand than had previously been felt for quantitative measures of the vapor discharged from volcanic vents. Nor is this all; with the steam of volcanoes is mingled various gases, and the mode of origin of the earth's atmosphere, as well as the changes it is now undergoing, is a theme in which the physiographer is profoundly interested.

Volcanic mountains are numbered among the most awe-inspiring of topographic forms; the solid additions which volcanoes make to the surface of the lithosphere are in view, and the contributions to the atmosphere of vapors and gases from the same sources are tangible facts; but another phase of the great problem is also of interest to the physiographer, namely, what changes take place in the rigid outer shell of the earth by reason of such transfers of vast volumes of material as are known to have occurred from deep within the earth to its surface. The magmas which have been caused to migrate and come to rest for a time, either as intrusions within the earth's outer shell, or as extrusions on its surface, are measurable in millions of cubic miles. In connection with the profound questions concerning the formation of folds and fractures in the earth's crust, an agency is thus suggested comparable in importance with loss of heat, as under the nebular hypothesis, or with gravitational compression, as explained by the planetesimal hypothesis. In the many discussions that have appeared as to the adequacy of earth contraction to account for the origin of mountains of the Appalachian type, I have been able to find but one mention of the rôle played by the transfer of matter from deep within the earth outward, and in part its extrusion at the surface, in causing folds in the crust from beneath which it was derived. Problems of fundamental importance are outlined by the considerations under review.

To the immediate question, What is the best plan for enlarging our knowledge of the physiography of volcanoes? the reply seems pertinent: Press on with the study of both active and dormant or extinct examples. In this connection it should be remembered that, while the individual volcanoes and volcanic mountains which have been critically studied can be enumerated on the fingers of one's hands, those which are practically unknown number many thousands. The fact that Mont Pelée and La Soufrière of St. Vincent during their recent periods of activity furnished examples of at least two important phases of volcanic eruptions not previously recognized is an assurance of rich returns when other eruptions are critically investigated.

While it is difficult to formulate the precise questions, so numerous are they, to be asked of volcanoes, whether active, dormant, or dead, and in various stages of decay and dissolution, it is plain that all the facts that can be learned concerning them should be classified and put on record, and their more obvious bearings on the fundamental questions concerning the condition of the earth's interior, and the changes there taking place, pointed out. In this connection — and as is true in all branches of research — the fact may be recalled that energy expended in discovering, classifying, and recording facts decreases the time and force necessary for the framing of multiple hypotheses. With an abundance of well-classified and pertinent observations in hand, the nature of the thread on which the gems of truth should be strung usually declares itself.

Résumé. On a previous page of this essay the desirability was suggested of recognizing ideal types with the aid of which the multitudinous surface features of the earth could be classified and studied. Thus far we have considered the elements in the relief of the earth's surface which have resulted from changes within its mass. We term them primary physiographic features, because their birth precedes the modifications of the lithosphere due to agencies acting externally. They are (1) the topographic forms resulting from contraction on account of cooling, or of condensation owing to growth in mass; (2) the surface changes produced by intrusions of magmas into the earth's outer shell; and (3) the results of volcanic eruption. Among the more important idealized models in our future physiographic museum there should be displayed continental platforms, oceanic basins, corrugated mountains, block mountains, domes of various and some of vast dimensions upraised by intrusions, volcanic cones, lava-plateaus, etc. These are the major physiographic types, or the larger monoliths from which the rock-hewn temples of the earth have been sculptured by forces acting on the surface of the lithosphere and deriving their energy mainly from the sun. Resulting from surface changes come a vast array of both constructive and destructive physiographic features, which may consistently be termed secondary. Under secondary features may be included also relational topographic forms, such as islands in water, in glaciers, and in lava-fields. In the study of the primary features of the earth's surface the work of the physiographer is most intimately linked with that of the geologist, but, on passing to the secondary feature, the influence of life becomes apparent, and the relation of man to nature is in the end the leading theme.

Secondary Physiographic Features

The most familiar features of land areas, as is well known, are those which owe their existence to the work of moving agencies resident on the earth's surface, such as the wind, streams, glaciers, waves, currents, etc. The forces at work are set in motion by energy derived from without the earth, and the material worked upon is brought within the range of their activities by forces resident within the earth which cause deformations of, or additions to, its surface. The earth-born primary physiographic features are thus modified by sun-derived forces, and a vast array of secondary modifications of relief are produced which give variety and beauty, particularly to those portions of the lithosphere which are exposed for a time to the air. The study of secondary physiographic features has produced a rich and abundant harvest, especially during the last quarter of a century, and the returns are still coming in at a seemingly accelerated rate.

The themes for study are here mainly the various processes of erosion and deposition of the material forming the outer film of the lithosphere, and the characteristics of the destructive and constructive topographic forms produced. With the knowledge gained concerning the changes now in progress on the ocean's shore, in the forest, by the riverside, on the snow-clad and glacier-covered mountains, etc., the physiographer seeks to decipher the records made in similar situations during the past. Two groups of problems are in sight in this connection; one is concerned with observing, classifying, and recording the changes now in progress; and the other has for its chief aim the translation, in terms of the agencies now at work, of the records left by past changes. We find that to-day the same area is being inscribed perhaps in several different ways. The surface of the earth, like an ancient manuscript, is frequently written upon in different directions; and with different characters. It is the duty of the physiographer to translate this ancient palimpsest, and deduce from it the history of the development of the features of the earth's surface. It has been said that "geology is the geography of the past," but to the physiographer this formula has a yet deeper meaning. There is a physiography of the past, of venerable antiquity, which has begun to receive attention. Ancient land surfaces, buried during geological eras beneath terranes which were deposited upon them, have here and there been exposed once more to the light of the sun, owing to the removal by erosion of their protecting coverings. In northern Michigan, for example, one may gaze on the veritable hills and valleys which were fashioned by the wind, rain, and streams of pre-Potsdam days of sunshine and shower. These *fossil landscapes* invite special study, not only on account of their poetic suggestiveness, but as furnishing evidence, supplementary to that afforded by organic records, ripple-marks, shrinkage-cracks, etc., as to the oneness of nature's processes throughout eons of time. The consideration of past physiographic conditions, the tracing of former geographic cycles, the study of the concurrent development of primary and secondary physiographic features, the causes and effects of past climatic changes, and the influences of these and still other events of former ages on the present expression of the face of nature, offer not only a fascinating, but a far extended field for research.

One especially important development of the study of past physiographic conditions, and the manner in which they merge with the present phase of the same history, is the connection between the life of the earth and its control by physical environment. The present and past distribution of floras and faunas affords important data supplementary to those recorded by abandoned stream-channels, glacier-scorings, elevated and depressed shore-lines, desiccated lake-basins, and other physical evidences of former geographic changes.

In the excursions into the domain of the unknown, here suggested, the physiographer seeks the companionship and counsel of both the geologist and the biologist.

Physiography and Life

In the study of the relation between physiography and the present state of development of living organisms on the earth, it is convenient and logical to recognize two great subdivisions: the one, the control exerted by physiographic features on the distribution of plants and animals; and the other, the reaction of life on its physical environment, and the modification in the relief of the lithosphere and the geography of its surface thus produced. Although man is embraced in each of these categories, there are sufficient reasons for considering his relations to his environment separately from those of the lower forms of life.

The dependence of life on its physical environment has received much attention from botanists and zoölogists, and is perhaps the leading thesis now claiming their attention. So important is this branch of study that a name, "ecology," has been coined by which to designate it. The phase of nature-study thus made prominent pertains to the marvelously delicate adjustment that has been found to exist between the distribution of life and the nature of the region it inhabits. Among the interesting themes involved are topographic relief, degree of comminution and disintegration of the surface blanket of rock-waste, depth and freedom of penetration of water and air into the life-sustaining film of the earth's surface, and the concurrent changes in life with variations in these and other physical conditions. In this most fascinating branch of study the ecologist borrows freely of the physiographer, and makes payment in peat-bogs, living vegetable dams in streams, organic acids serviceable for rock disintegration and decay, deposits of calcium carbonate and silica in lakes and about springs, vast incipient coal-beds in the tundras of the far north, and numerous other ways.

From the physiographic point of view, however, the many and intricate ways in which life leads to modifications in the features of the lithosphere are of more direct interest than studies in ecology. Much has been accomplished in this direction, but it is evident that as yet but partially explored paths leading through the borderland between biology and physiography remain to be critically examined.

In connection with the changes in progress on the earth's surface, due to the influence of organic agencies, and the application of that knowledge in interpreting past changes, the study of the influences exerted by the lowest forms of life in both the botanical and the zoölogical scale seems most promising to the physiographer.

The secretion of calcium carbonate and silica by one-celled organisms, as is well known, has led to the accumulation of vast deposits like the oozes on the sea-floor, beds of diatomaceous earth, deposits about hot springs, the so-called marl of fresh-water lakes, etc. A review of the several ways in which such accumulations are formed, and an extension of the search in various directions, give promise that other and equally wonderful results flowing from the activities of the lowest form of life will be discovered. The mode of deposition of iron, and perhaps of manganese, the generation of hydrocarbons, the origin of extensive sheets of seemingly non-fossiliferous limestone and dolomite, the method by which the beautiful onyx marbles are laid down, film on film, the nature of the chert so abundant in many terranes and so conspicuous in the surface waste of extensive regions, and other equally important deposits which exert a profound and frequently controlling influence on topographic forms, seemingly demand study with the hypothesis in mind that they owe their origin to the vital action of low forms of plants or animals. Not only the concentration of mineral matter by one-celled organisms, but the part played by similar organisms in the comprehensive processes of denudation, also invites renewed attention. Many of the organisms in question do not secrete hard parts, and hence are incapable of directly aiding in the concentration of inorganic solids on the surface of the lithosphere. If not assisting in the building of physiographic structures, the suspicion is warrantable that they are engaged in sapping their foundations. The wide distribution of one-celled organisms, — and, indeed, as one may say, their omnipresence on the earth's surface, — and their seeming independence, as a class, to differences in temperature, light, and humidity, enable them to exert an unseen and silent influence, not suspected until some cumulative and conspicuous result is reached. The importance of bacteria in promoting decay, and in consequence the formation of acids which take a leading part in the solution and redeposition of mineral substances, the rôle played by certain legions of the invisible hosts in secreting nitrogen from the air and thus aiding vegetable growth, and perhaps to be held accountable also for the concentration of nitrates in cavern earths, the part others play in fermentation, and the diseases produced in plants and animals by both bacteria and protozoa, render it evident that an energy of primary importance to the physiographer is furnished by these the lowest of living forms. Physiographers were given a new point of view when Darwin explained the part played by the humble earthworms in modifying the earth's surface. As it seems, still other advances in our knowledge of the changes in progress in the vast laboratory in which we live may be gained by studying the ways in which organisms far lower in the scale of organization than the earthworms are supplying material for the building of mountains or assisting in the leveling of plains.

In brief, a review of the interrelations of physiography and life shows that from the lofty snow-fields reddened by *Protococcus*, to the bottom of the ocean, the surface of the lithosphere is nearly everywhere enveloped in a film teeming with life. In part the vital forces at work are reconcentrating material and adding to the solid framework of the globe, and in part, but less obviously, aiding in rock decay and disintegration. Throughout this vast, complex cycle of changes new physiographic features are appearing, others disappearing, and one and all, to a greater or less degree, are undergoing modifications. The wide extent of the changes in progress, and their known importance in certain instances, are justification for the belief that the physiographer as well as the ecologist will find many problems of fundamental importance to his science in the interrelations of life and physiographic conditions.

Physiography and Man

Go forth, subdue and replenish the earth, is the language of Scripture. The observed results show that, while man strives to bend nature to his will, he himself is a plastic organism that is moulded by the many and complex external forces with which it comes in contact. Here again two groups of themes present themselves to the physiographer: one embracing the influences of environment on man; and the other, the changes in the features of the earth's surface, brought about by human agencies. In the first the physiographer can aid the anthropologist, the historian, the socialist, etc.; and in the second, which is more definitely a part of his own specialty, he searches for suggestive facts throughout the entire domain of human activities. It is in these two directions that the student of the earth's surface finds the most difficult and the most instructive of the problems in which he takes delight, and the richest rewards for his efforts.

The control exerted by physiographical environment on human development is so subtle, so concealed beneath seemingly accidental circumstances, and its importance so obscured by psychological conditions, that its recognition has been of slow growth. The countless adjustments of both the individual man and of groups of men in communities, nations, and races, to physical conditions, is so familiar that the sequence of causes leading to observed results passes as a matter of course, and to a great extent fails to excite comment. The due recognition of the influence of physiographic environment on history is now coming to the front, and, as is evident, the rewriting of history, and especially the history of industry, from the point of view of the physiographer, is one of the great tasks of the future. The problems in this broad field are countless, and the end in view is similar to those embraced in dynamical physiography, namely, the study of the various ways in which man is now influenced by his physical environment, with the view of interpreting the records of similar changes in the past and of predicting future results. Or more definitely formulated: peoples have reached a high degree of culture under certain multiple conditions of environment, while other peoples, exposed to other combinations of conditions, have remained stationary, or retrograded and become degenerate. What are the essential conditions in control in the one case or the other?

Can predictions be made as to what the results of a given combination of physical conditions on a given community will be, in spite of that other and still more mobile, and as yet but little understood, group of conditions embraced under the term *psychology?* Many profound questions, in the solution of which the physiographer unites his efforts with those of the student of the humanities, present themselves for study during the century that is yet young.

Within the broader questions just suggested are many others that are more concrete and definite, and of vital importance to mankind, which can be conveniently grouped under the term *economic physiography.* The problems which here present themselves share their chief interests with the engineer. They relate to plans for transportation in all of its various forms, drainage, irrigation, water-supply, sanitation, choice of municipal locations, control of river-floods, selection of cities for homes, farms, vineyards, factories, etc. In every branch of industry a critical knowledge of the physical conditions, both favorable and adverse to the economic ends in view, and of the limitations of the daily, seasonal, and secular changes they experience, is of primary commercial importance. Although the money-value of truth should be a secondary consideration to the truth-seekers, a critical study of the influence of environment on industry is as truly a matter of scientific research as any of the less complex and less directly utilitarian branches of physiography.

The reaction of human activities on physiographic features presents two great groups of problems. These embrace, on the one hand, the far-reaching and frequently cumulative effects of man's interference with the delicate adjustment reached in natural conditions before his influence became manifest; and, on the other hand, the effects of such changes on man's welfare.

A change, amounting to but little less than a revolution in the long-established processes by which the features of the earth's surface are modified and developed, accompanied the advancement of man from a state of barbarism to one of civilization, and is most strikingly illustrated when men skilled in the arts migrate to a previously unoccupied region. This new factor in the earth's history demands conspicuous changes in the methods of study usually employed by physiographers, and makes prominent a series of investigations, the full significance of which is as yet obscure. The wholesale destruction of forests, drainage of marshes, diversion of streams, building of restraining levees along river banks, tillage of land, abandonment of regions once under cultivation; the introduction of domestic animals in large numbers into arid regions, and the consequent modification, and frequently the destruction, of the natural vegetal covering of the soil; and many other sweeping changes incident to man's industrial development, are fraught with consequences most significant to the student of nature, and of profound import to the future of the human race. From the point of view of the physiographer, the ultimate result of these great changes in the surface conditions of the earth can to a great extent be expressed in one word, and that word is *desolation*. In view of the suicidal lack of forethought manifest in the activities of peoples, and, as experience shows, increasing in many directions in destructiveness with industrial progress, the problems that confront the physiographer are not only what far-reaching changes in the surface condition of the land result therefrom, but how the ruin wrought can be repaired, and how human advancement can be continued and its deleterious consequences on the fundamental conditions to which it owes its birth and development be avoided or lessened. Considerations which lessen the horrors of the regions crossed by industrial armies are that nature, no matter how severely torn, has great recuperative power and tends to heal her wounds; and also that man, through the science of agriculture particularly, although greatly modifying natural conditions, is able to reconstruct his environment, and, so long as intelligent care is exercised, adjust it to his peculiar needs.

In the relations of physiography to man, as the above hasty sketch is intended to show, the themes for research are many and important. As a suggestive summary, they include the review of history with the aid that modernized physical geography furnishes; the recognition of a strong undercurrent due to inorganic conditions in the political, social, and industrial development of peoples; the incorporation of physiographic laws into the formulas used by the engineer in all of his far-reaching plans; the calling of a halt in the wanton destruction of the beauties of nature, and the providing of a check on the greed of man which casts a baneful shadow on future generations. Great as are the results to be expected from a better knowledge of the mode of origin of the earth, its deformation by internal changes, and the removal and redeposition of material by forces resident on its surface, the combined results of all these studies culminate in the relation of man to his environment.

SECTION F — GEOGRAPHY

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(*Hall* 11, *September* 22, 3 *p. m.*)

CHAIRMAN: PROFESSOR ISRAEL C. RUSSELL, University of Michigan.
SPEAKERS: HUGH ROBERT MILL, Director of the British Rainfall Organization, London.
H. YULE OLDHAM, King's College, Cambridge.
SECRETARY: R. D. SALISBURY, University of Chicago.

THE PRESENT PROBLEMS OF GEOGRAPHY

BY HUGH ROBERT MILL

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THE present problems of a science may, I hope, be viewed as those problems the solution of which at the present time is most urgent and appears most promising. Were present problems held to include the whole penumbra of our ignorance, I at least have neither the desire nor the competence to discourse upon them. So much has been written on the problems of geography in recent years that a detailed summary of the existing literature would be a ponderous work, and afford much dull and contradictory reading. I cannot even attempt to associate different views of the problems of geography with the names of their leading exponents, though, perhaps, if I were to do so, I should quote with almost entire approval the masterly address recently delivered to the American Association for the Advancement of Science by Professor W. M. Davis.

Believing that every geographer should approach such a question as this by the avenue of his own experience, I offer a frankly personal opinion, the outcome of such study, research, and intercourse with kindred workers as have been possible to me during the last twenty years. The views I hold may not be representative of European, perhaps not even of British, geographical opinion, except in so far as they are the result of assimilating, more or less consciously, the writings and teachings of geographical leaders in all countries, retaining congenial factors, and modifying or rejecting those which were foreign to the workings of my own partially instructed mind.

The history of every branch of science teaches that time works changes in the nature and the value of the problems of the hour. In successive ages the waves of existing knowledge make inroads upon the shores of ignorance at different points. For one generation they seem to have been setting, with all their force, against some one selected point; in the next they are encroaching elsewhere, the former problem left, it may be, imperfectly solved; but gradually the area of the unknown is being reduced on every side, however irregularly.

In the beginning of geography, the problem before all others was the figure of the Earth. Scientific progress, not in geography alone, but in all science, depended on the discovery of the truth as to form. No sooner was the sphericity of the Earth established than two fresh problems sprang to the front, neither of them new, for both existed from the first, — the fixing of position and the measurement of the size of the Earth. Geography, and science as a whole, progressed by the failures, as well as by the successes, of the pioneers who struggled for centuries with these problems. Latitude was a simple matter, theoretically no problem at all, but a direct deduction from the Earth's form, though its determination was practically delayed by difficulties of a mechanical kind. The problem of the longitude was far more serious, and bulks largely in the history of science. Pending their solution, the estimates of size were rough guesses; had these been more accurate, it is doubtful if Columbus could have persuaded any sane sailor to accompany him on his westward voyage to India, the coast of which he was not surprised to find so near to Spain as the Caribbean Sea.

After latitude could be fixed to a nicety, and longitude worked out in certain circumstances with nearly equal accuracy, the size of the Earth was determined within a small limit of error, and the problem of geography shifted to detailed discovery. This phase lasted so long that even now it hardly excites surprise to see an article, or to open a volume, on the history of geography, which turns out to be a narrative of the progress of discovery. Perhaps British geographers, more than others, were prone to this error, and for a time the country foremost in modern discovery ran some risk of falling to the rear in real geography.

It is not so paradoxical as it seems to say that the chief problem of geography at present is the definition of geography. Some learned men have said within living memory, and many have thought, that geography is not a science at all, that it is without unity, without a central theory, that it is a mere agglomerate of scraps of miscellaneous information regarding matters which are dealt with scientifically by astronomers, geologists, botanists, anthropologists, and others. Geography is not so circumstanced. Although its true position has only recently been recovered from oblivion, it is a science, and one of long standing.

I have said before,[1] and I may repeat, because I can say it no better, that modern geography has developed by a recognizable continuity of change from century to century. I am inclined to give more weight than others have done to the remarkable treatise of Dr. Nathanael Carpenter, of Exeter College, Oxford, published in 1625, as a stage in the growth of geographical thought and theory. The striking feature of Carpenter's book is the practical assertion of the claims of common sense in dealing with questions which superstition and tradition had previously influenced. Varenius, who died at the age of twenty-eight, published in 1650 a single small volume, which is a model of conciseness of expression and logical arrangement well worthy even now of literal translation into English. From several points in its arrangement I am inclined to believe that he was influenced by Carpenter's work. So highly was Varenius's book thought of at the time that Sir Isaac Newton brought out an annotated Latin edition at Cambridge in 1672. The opening definition as rendered in the English translation of 1733 (a work largely spoiled by stupid notes and interpolations) runs:

"Geography is that part of *mixed mathematics* which explains the state of the Earth and of its parts, depending on quantity, viz., its figure, place, magnitude, and motion with the celestial appearances, etc. By some it is taken in too limited a sense, for a bare description of the several countries; and by others too extensively who, along with such a description, would have their political constitution."

Varenius produced a framework of physical geography capable of including new facts of discovery as they arose; and it is no wonder that his work, although but a part, ruled unchallenged as the standard text-book of pure geography for more than a century. He laid stress on the causes and effects of phenomena, as well as the mere fact of their occurrence, and he clearly recognized the influence upon different distributions of the vertical relief of the land. He did not treat of human relations in geography, but, under protest, gave a scheme for discussing them as a concession to popular demands.

As Isaac Newton, the mathematician, had turned his attention to geography at Cambridge in the earlier part of the eighteenth century, so Immanuel Kant, the philosopher, lectured on the same subject at Königsberg in the later part. The science of geography he considered to be fundamentally physical, but physical geography formed the introduction and key to all other possible geographies, of which he enumerated five: *mathematical*, concerned with the form, size, and movements of the Earth and its place in the solar system; *moral*, taking account of the customs and characters of mankind according to their physical surroundings; *political*, concerning the divisions of the land into the territories of organized governments; *mercantile*, or, as we now call it, commercial geography; and *theological*, which took account of the distribution of religions. It is not so much the cleavage of geography into five branches, all springing from physical geography like the fingers from a hand, which is worthy of remark, but rather the recognition of the interaction of the conditions of physical geography with all other geographical conditions. The scheme of geography thus acquired unity and flexibility such as it had not previously attained, but Kant's views have never received wide recognition. If his geographical lectures have been translated, no English or French edition has come under my notice; and such currency as they obtained in Germany was checked by the more concrete and brilliant work of Humboldt, and the teleological system elaborated in overwhelming detail by Ritter.

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[1] *British Association Reports* — Presidential Address in Section E. Glasgow, 1901.

Ritter's views were substantially those of Paley. The world, he found, fitted its inhabitants so well that it was obviously made for them down to the minutest detail. The theory was one peculiarly acceptable in the early decades of the nineteenth century, and it had the immensely important result of leading men to view the Earth as a great unit, with all its parts coördinated to one end. It gave a philosophical, we may even say a theological, character to the study of geography.

Kant had also pointed to unity, but from another side, that of evolution. It was not until after Charles Darwin had fully restored the doctrine of evolution to modern thought that it was forced upon thinking men that the fitness of the Earth to its inhabitants might result, not from its being made for them, but from their having been shaped by it. The influence of terrestrial environment upon the life of a people may have been exaggerated by some writers, — by Buckle, in his *History of Civilization*, for example, — but it is certain that this influence is a potent one. The relation between the forms of the solid crust of the Earth and all the other phenomena of the surface constitutes the very essence of geography.

It is a fact that many branches of the study of the Earth's surface which were included in the cosmography of the sixteenth century, the physiography of Linnæus, the physical geography of Humboldt, and perhaps even the *Erdkunde* of Ritter, have been elaborated by specialists into studies which, for their full comprehension, require the whole attention of the student; but it does not follow that these specializations fully occupy the place of geography, for that place is to coördinate and correlate all the special facts concerned, so that they may throw light on the plan and the processes of the Earth and its inhabitants. This was clear to Carpenter in 1625, though it has been almost forgotten since.

The principles of geography on which its claims to status as a science rest are generally agreed upon by modern geographers, though with such variations as arise from differences of standpoint and of mental process. The evolutionary idea is unifying geography, as it has unified biology, and the whole complicated subject may be presented as the result of continuous progressive change brought about and guided by the influence of external conditions. It is impossible to discuss the present problems of geography without once more recapitulating the permanent principles.

The science of geography is, of course, based on the mathematical properties of a rotating sphere; but there is force in Kant's classification, which subordinated mathematical to physical geography. The vertical relief of the Earth's crust shows us the grand and fundamental contrast between the oceanic hollow and the continental ridges; and the hydrosphere is so guided by gravitation as to fill the hollow and rise upon the slopes of the ridges to a height depending on its volume, thus introducing the great superficial separation into land and sea. The movements of the water of the ocean are guided in every particular by the relief of the sea-bed and the configuration of the coast-lines. Even the distribution of the atmosphere over the Earth's surface is affected by the relief of the crust, the direction and force of the winds being largely dominated by the form of the land over which they blow. The different physical constitution of land, water, and air, especially the great difference between the specific heat and conductivity or diathermancy of the three, causes changes in the distribution of the sun's heat, and as a result the simple climatic zones and rhythmic seasons of the mathematical sphere are distorted out of all their primitive simplicity. The whole irregular distribution of rainfall and aridity, of permanent, seasonal, and variable winds, of sea climate and land climate, is the resultant of the guiding action of land forms on the air and water currents, disturbed in this way from their primitive theoretical circulation. So far we see the surface forms of the Earth, themselves largely the result of the action of climatic forces, and constantly undergoing change in a definite direction, control the two great systems of fluid circulation. These in turn control the distribution of plants and animals, in conjunction with the direct action of surface relief, the natural regions and belts of climate dictating the distribution of living creatures. A more complicated state of things is found when the combined physical and biological environment is studied in its incidence on the distribution of the human race, the areas of human settlement, and the lines of human communications. The complication arises partly from the fact that each of the successive earlier environments acts both independently and concurrently; but the difficulty is in greater degree due to the circumstance that man alone among animals is capable of reacting on his environment and deliberately modifying the conditions which control him.

I have said before, and I repeat now, that the glory of geography as a science, the fascination of geography as a study, and the value of geography in practical affairs, arise from the recognition of this unifying influence of surface relief in controlling, though in the higher developments rather by suggestion than dictation, the incidence of every mobile distribution on the Earth's surface. I am inclined, in the light of these views, to put forward a definition of geography which I think may be accepted in principle, if not in phrase, by most of the class called by Professor Davis "mature geographers."

It runs, *Geography is the science which deals with the forms of relief of the Earth's crust, and with the influence which these forms exercise on the distribution of all other phenomena.*

The old pigeon-hole view of human knowledge is now happily discredited and recognized as useless, save perhaps by some Rip Van Winkles of science, who concern themselves more with names than things, and would cheerfully misconceive the facts of nature to fit the framework of their accepted theories. High specialization is necessary to progress, but only as a phase of a working life, not as the whole purpose of a whole man.

It is convenient and often profitable for a man of science to have a recognized label, but it seems to me that important advances are to be made by cultivating those corners of the field of knowledge which lie between the patches where the labeled specialists toil in recognized and respected supremacy. It has been so habitual to classify the man of science by what he works in that it requires something of an effort to see that the way in which he works is of greater determinative importance. Thus the scientific geographer is apt to find no place in the stereotyped classification, and his work may be lost sight of on that account. Should he dwell on latitude and longitude, the astronomer smiles pityingly; if he looks at rocks, the geologist claims that department; if he turns to plants, the botanist, with the ecologist behind him, is ready to warn him off; and so with other specialists. But the mature geographer seeks none of the territory, and hankers after none of the goldfields, belonging to other recognized investigators. He works with the material they have already elaborated, and carries the process a step farther, like the goldsmith handling the finished products of the metallurgist and the miner.

The present problems of geography seem to me to be of two kinds: the first, minor and preliminary, the completion of the unsolved and partially solved problems of the past; the second, ultimate and essential, dealing with the great problem on the solution of which the whole future of the science rests.

The residual problems inherited from the past represent the work which should have been done by our predecessors, but, not having been done at the right time, remains now to bar our progress. It has to do only with ascertaining and accurately recording facts, and involves infinite labor, but comparatively little geographical thought.

To begin with, the ground should be cleared by wiping off the globe the words *terra incognita*. Such unknown parts of the Earth now cling about the poles alone, and that they should even do this is something of a disgrace. If common terrestrial globes were pivoted on equatorial points, so that the polar areas were not covered with brass mountings, the sight of the bare patches would perhaps have been so galling to the pride of humanity that they would long since have been filled in in detail. Again and again, and never more splendidly than in recent years, polar explorers have shown courage and perseverance, and have cheerfully encountered hardships enough to have enabled them to reach the poles, and they would have done so, not once, but many times, were it not for the want of money. Of course, all polar explorers have not been competent for the task they undertook, but most of the leaders, if they had had more powerful ships, more coal, more stores, more dogs — and sometimes if they had had fewer men — could have solved these perennial problems of exploration. With a competent man in command, — and competent men abound, — a sufficiency of money is all that is required. A million dollars judiciously spent would open the way to the north pole, a few millions would reach the south pole; but far more than this has been spent in vain, because the money was doled out in small sums at long intervals, sometimes to explorers with no real call to the quest, and working in accordance with no scientific plan.

The grand journeys over the polar ice of Nansen, Peary, and Cagni in the north, and of Scott and his company in the south, promise well for an early solution of this particular problem.

The other residual problems of exploration and survey are in the same case. If those who control money saw it to be their duty to solve them, they would all be solved, not in a year, but in due time. Though a great deal of exploration remains to do, the day of the ignorant explorer is done. The person who penetrates a little-known country in search of adventures or sport, or in order to go where no one of his color or creed had been before, is, from the geographical point of view, a useless wanderer; and if he be a harmless wanderer, the true explorer who may follow in his footsteps is uncommonly fortunate. Exploration now requires, not the pioneer, but the surveyor and the student.

The map of the world ought to be completed, and it is the duty and, I believe, the interest, of every country to complete at least that portion which includes its own territory. An imperial policy which ignores such an imperial responsibility is a thing of words, and not of deeds. Unsurveyed and unmapped territory is a danger, as well as a disgrace, to the country possessing it, and it would hardly be too much to say that boundary disputes would be unknown if new lands were mapped before their mineral wealth is discovered. The degree of detail required in any survey depends upon the importance of the region. The desideratum is not a large-scale map of every uninhabited island, but a map of the whole Earth's surface on the same scale, which for the present may be a small one, and might very well be that of 1: 1,000,000 proposed by Professor Penck, and now being carried into effect for the surveyed portions of the land. Such a map ought to include sub-aqueous as well as sub-aërial features, and when completed it would form a solid basis for the full discussion of many problems which at present can only be touched upon in a detached and unsatisfactory manner. The first problem which it would solve is the measurement of the volume of the oceanic waters and of the emergent land, so that the mean depth of the oceans and the mean heights of the continents might be exactly determined. This would involve, besides the horizontal surveys, a vertical survey of considerable accuracy. At sea the vertical element is easily found, and the depths measured by surveying and exploring vessels in recent years are very accurate. They must, however, be made much more numerous. On land, outside the trigonometrically surveyed and spirit-leveled countries, the vertical features are still most unsatisfactorily delineated. Barometric determinations, even when made with mercurial barometers or boiling-point thermometers, are uncertain at the best, while when made with aneroids they afford only the roughest approximations to the truth. Where leveling is impracticable, angular measurements of prominent heights, at least, should be insisted on as an absolute necessity in every survey.

When a map of the whole surface of the Earth on the scale of 1:1,000.000 is completed, we may consider the residual problems as solved. This is far from being the case as yet, and in the present circumstances the most useful work that the geographical societies of the world could do would be to secure the completion of explorational surveys to that scale. The system of instruction for travelers established by the Royal Geographical Society has equipped a large number of explorers and colonial officials as expert surveyors, and the result is now being felt in every quarter of the globe. This is not the highest geographical work, but merely preliminary and preparatory; yet progress is checked, if not barred, until it is accomplished. The map of one to a million is not to be viewed as an end in itself; nevertheless, its completion will mark an era, the accomplishment of the small-scale survey of the globe, and permit of fresh advances.

Money could solve the last of the problems of exploration, but when we come to problems of the second category we enter a region of pure science, where money becomes a minor consideration. The acquisition of knowledge is a simple process, for which multitudes have a natural aptitude; but the coördination of knowledge and its advancement are very different matters. The difference is more marked in the case of geography than in geology or chemistry or physics, for, in English-speaking countries at least, the training of geographers is in its infancy, whilst that of the exponents of other sciences is highly developed. Hence it happens that before any actual problem in geography can be attacked, the man who is to deal with it must be prepared on purpose for the task, and he must have determination enough to stick to an unpopular subject with little encouragement in the present and small prospects for the future. Such men are not very easily found.

If they can be found, the problems they should be set to solve are at hand and waiting. We know enough about the relations of mobile distributions to fixed environments to feel satisfied that the relations are real and of importance; but we do not yet know enough to determine exactly what the relations are and the degree in which they apply to particular cases. It is the province of geography to find this out, and to reduce to a quantitative form the rather vague qualitative suggestions that have been put forward. The problem is multiform and manifold, applying to a vast range of phenomena, and those who have surveyed it are often inclined to sigh for a Kepler or a Newton to arise and call order from the chaos.

A vast amount of material lies before the geographer with which to work, even though, as has been explained, much more is needed before the data can be looked upon as complete. After seeing that the missing facts are in course of being supplied, the great thing is to work and to direct the work of others towards the proper comprehension of the facts and their bearings. This involves as much the checking and discouragement of work in wrong or useless directions as the help and encouragement of well-directed efforts.

The first element of geography is the configuration of the crust of the Earth, and our knowledge is already ripe for a systematic classification of the forms of the crust, and for a definite terminology by which to describe them. For some reason, not easy to discover, geographical terms, with the exception of those handed down from antiquity, have not, as a rule, been taken from the Greek, like other scientific terms. They have usually been formulated in the language of the author who has introduced them. For this reason they retain a national color, and, absurd as it may seem to scientific reflection, national or linguistic feeling is sometimes a bar to their general adoption. A more serious difficulty is that different languages favor different modes of thought, and thus lead to different methods of classification. The clearness and definiteness of French conduces to the use of simple names, and the recognition of definite features distinguished by clear differences. The facility for constructing compound words presented by German lends itself to the recognition of composite types and transition forms, the introduction of which often tangles a classification in an almost unmanageable complexity. English stands intermediate between those languages, less precise, perhaps, than French, certainly less adaptable than German, and English terminologies often reflect this character. The best way out of the difficulty seems to be to endeavor to arrive at a general understanding as to a few broad types of land-form which are recognized by every one as separate and fundamental, and then to settle equivalent terms in each important language by an international committee, the finding of which would have to be ratified by the national geographical societies. These terms need not necessarily be identical, nor even translated literally from one language into another, but their equivalence as descriptive of the same form should be absolute. A recent international committee appointed for the nomenclature of the forms of sub-oceanic relief put forward certain suggestions in this direction which might well be adapted to the forms of sub-aërial relief as well. But there are strong-willed geographers who will recognize no authority as binding, and who will not, I fear, ever conform to any scheme which might threaten their liberty to call things as they please.

Personally, I would go very far to obtain uniformity and agreement on essential points, but the only way to do so seems to be to arrive by general agreement at a classification that is as brief, simple, and essential as possible.

It is necessary to classify land-forms according to their resemblances and differences, so that similar forms may be readily described, wherever they may be. The fixed forms of the crust are the foundation of all geography, the ultimate condition underlying every distribution, the guiding or controlling resistance in every strictly geographical change. The question of place-names is altogether subordinate. It is convenient that every place should have a name, and desirable that the name should be philologically good, but the national boards of geographic names, geographical societies, and survey departments see to that, and do their work well. The question of terminology is far more difficult, and, I think, more pressing.

The grand problem of geography I take to be the demonstration and quantitative proof of the control exercised by the forms of the

Earth's crust upon the distribution of everything upon the surface or in contact with it which is free to move or to be moved. It is a great problem, the full solution of which must be long delayed, but every part of it is a-bud with minor problems of detail, alike in nature, but differing widely in degree. These minor problems claim our attention first, and are so numerous that one fears to attempt their enumeration because of the risk of distracting attention from the main issue. Geography was defined long ago as the science of distribution; but the old idea was statical distribution, the laying-down on maps of where things are; now we see that we ought to go farther, and discuss also how the things came there, why they remain there, whether they are in transit, and, if so, how their path is determined. We are learning to look on distribution from its dynamical side, the earth with all its activities being viewed as a machine at work. The geographer, as an independent investigator, has to deal only with matters touching or affected by the crust of the Earth; his subject is limited to a part only of the economy of the Kosmos, a fact that sometimes seems to be in danger of being forgotten.

The quantitative relationships of crustal control have to be worked out for different areas with different degrees of detail. A great deal has been done already, and the material for much more has been collected in a form fit for use. The first step in commencing such a discussion is the accurate mapping of all available data — each kind by itself — for the particular area. On the national, and almost continental scale, this is done better in the United States Census Reports than in any other works known to me. An adequate discussion of all that is shown in the maps accompanying these Reports, and in those of the Coast and Geodetic Survey, the Geological Surveys, and the Department of Agriculture, would be almost an ideal geographical description. The material provided in such rich profusion by the Federal and State Governments is being used in American universities with an originality and thoroughness that has developed the conception of geography and advanced its scientific position. American geographers more than others have grasped the dynamic idea of geography, and realized that the central problem is the elucidation of the control or guidance exercised by fixed forms on mobile distributions.

Detailed work in the same direction has been done by many European geographers, whose works are too well known to require citation; but the geographical treatment of statistics has not been taken up adequately by public departments in the countries east of the Atlantic. To touch only on the instance most familiar to me, with the exception of the maps of the Admiralty, Ordnance, and Geological Surveys, which cannot be surpassed, the maps issued by British Government Departments in illustration of their reports are rarely more than diagrams delimiting the areas dealt with, but not depicting the distributions. This is the more regrettable because the accuracy and completeness of the statistics in the reports are inferior to none, and superior to most work of a similar character in other countries. As frequently happens, private enterprise has stepped in where official action is wanting, and it is a pleasure to the geographer to turn to the recent maps of Mr. J. G. Bartholomew, especially the volume of his great Physical Atlas, the Atlas of Scotland published some years ago, and the Atlas of England and Wales, which has just left the press. Both of the latter works contain general maps based on statistics that have not been subjected to cartographic treatment before, and attention may be drawn in particular to the singularly effective and suggestive mapping of density of population. Another work similar in scope, and no less creditable to its compilers, is the Atlas of Finland, prepared by the active and enlightened Geographical Society of Helsingfors. In Germany, France, and Russia, also, examples may be found of good work of this kind, sufficient to whet the desire for the complete and systematic treatment of each country on the same lines.

It seems to me that the most useful application of youthful enthusiasm in geography, such as breaks forth in the doctorial theses of German universities, and is solicited in the programme of the Research Department of the Royal Geographical Society, would be towards the detailed comparison of the distribution of the various conditions dealt with statistically in Government Reports with the topographical map of selected areas. The work would, of course, not stop with the maps, for these, when completed, should be tested and revised as fully as possible on the ground, since geography, be the scale large or small, is not advanced by maps alone.

Such small portions of the coördination of existing surveys are, at the best, no more than fragments of a complete scheme, but they show what can be done with existing surveys and actual statistics, and indicate where these may be appropriately reinforced by new work. I have treated a special case of this kind pretty fully, in papers to which it is only necessary to refer.[1] One section of the scheme outlined and exemplified in these papers is the distribution of rainfall viewed in relation to the configuration of the land; and with the active assistance of nearly four thousand observers in the British Isles, I feel that there is some prospect, though it may lie far in the future, of ultimate results from that study.

The system of botanical surveys now being carried on with signal success in many countries is in some ways even more interesting. It includes the mapping of plant associations and the discussion of their relation to altitude, configuration, soil, and climate. Such phenomena are comparatively simple, and the influence of the various modifications of geographical control is capable of being discovered. I need only mention the similar problems in animal distribution, both on land and in the sea, to the elucidation of which many able workers are devoting themselves.

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[1] *Geographical Journal*, VII (1896), 345–364; XV (1900), 205–226, 353–377.

Difficulties increase when the more complicated conditions of human activity are taken into account. The study of the geographical causes determining, or assisting to determine, the sites of towns, the lines of roads and railways, the boundaries of countries, the seats of industries, and the course of trade, is full of fascination and promise. It has yielded interesting results in many hands; above all, in the hands of the leading exponent of anthropogeography, the late Professor Ratzel, of Leipzig, whose sudden death last month is a grievous loss to geographical science. Had he lived, he might have carried the lines of thought, which he developed so far, to their logical conclusion in the formulation of general laws of universal application; but that task devolves on his disciples.

Separate efforts in small and isolated areas are valuable, but a much wider basis is necessary before general principles that are more than hypotheses can be deduced. For this purpose there must be organized coöperation, international if possible, but, in the present condition of things, more probably on a national footing for each country. To be effective, the work would have to be on a larger scale, and to be continued for a longer time, than is likely to appeal to an individual or a voluntary association. One experienced geographer could direct an army of workers, whose task would be to collect materials on a properly thought-out plan, and from these materials the director of the work could before long begin to produce results, probably not sensational, but accurate and definite, which is far better. The director of such a piece of work must be free to disregard the views of the collectors of the facts with which he deals, if, as may very well happen, these views are at variance with scientific principles.

A complete geographical description should commence with a full account of the configuration of the selected area, and in this I lay less stress than some geographers feel it necessary to do upon the history of the origin of surface features. The features themselves control mobile distributions by their form, irrespective of the way in which that form was produced, and, although considerations of origin are often useful and always interesting, they are apt to become purely geological. The second point to discuss is the nature of the actual surface, noting the distribution of such geological formations as volcanic rocks, clays, limestones, sandstones, and economic minerals, the consistency and composition of the rocks being the points to which attention is directed, the geological order or age an entirely subordinate matter. To this must be added a description of the climate as due to latitude, and modified by altitude, exposure, and configuration; then the distribution of wild and cultivated plants in relation to their physical environment, and of the industries depending on them and on other natural resources. As the conditions increase in complexity, historical considerations may have to be called in to aid those of the actual facts of to-day. The lines of roads and railways, for example, are usually in agreement with the configuration of the localities they serve; but anomalies sometimes occur, the explanation of which can only be found by referring to the past. The more transitory features of a country may have acted differently at different times in affording facilities or interposing barriers to communication. The existence of forests long since destroyed, of marshes long since drained, of mineral deposits long since worked out, or of famous shrines long since discredited and forgotten, account for many apparent exceptions to the rules of geographical control. In long-settled countries the mobile distributions do not always respond immediately to a change of environment. A town may cease to grow when the causes that called it into existence cease to operate, but it may remain as a monument to former importance, and not wither away. As one ascends in the geographical system, the mobility of the distributions which have to be dealt with increases, the control of crust-forms upon them diminishes, and non-geographical influences come more and more into play. It may even be that causes altogether outside of geographical control account for the persistence of worn-out towns, the choice of sites for new settlements, or the fate of existing industries. If this be really so, I think it happens rarely, and is temporary. Geographical domination, supreme in simple conditions of life, may be modified into geographical suggestion; but in all stable groupings or continuous movements of mankind the control of the land on the people will surely assert itself. How? and to what degree? are the questions to which the modern geographer must seek an answer.

A special danger always menaces the few exponents of modes of study which are not yet accepted as of equal worth with those of the long-recognized sciences. It is the Nemesis of the temptation to adopt a plausible and probably true hypothesis as the demonstrated truth, and to proclaim broad and attractive generalizations on the strength of individual cases. Geographers have, perhaps, fallen into the error of claiming more than they can absolutely prove in the effort to assert their proper position; but the fault lies mainly at other doors. In geography it is not always easy to obtain exact demonstrations or to apply the test of accordance with fact to an attractive hypothesis; and it is necessary to be on guard against treating such speculations as if they were truths. The methods of journalism, even of the best journalism, are to be absolutely discouraged in science. The new is not necessarily truer or better than the old simply because it is new, and we must remember that time alone tests theories. It is a danger to become too popular. The scientific study of geography should be carried on with as many safeguards of routine verification, and patient repetition, and it may be within as high a fence of technical terminology, as, say, physiology, if the proper results are to be obtained. Unfortunately, the idea is prevalent that geography is an easy subject, capable of being expounded and exhausted in a few popular lectures. I regret to see the growing tendency amongst teachers of geography to deprecate the acquisition of facts, to shorten and "simplify" all chains of reasoning, to generalize over the heads of clamant exceptions, and even to use figures, not as the ultimate expression of exact knowledge, but merely as illustrations of relative magnitude. I quite allow that all this may be legitimate and laudable in the early stages of elementary education, but it should never pass beyond, and every vestige of such a system of evading difficulties should be purged from the mind of the aspirant to research.

The facts available for the advancement of geographical science are neither so well known nor so easily accessible as they should be. Much has been done towards the indexing of the current literature of all sciences, and geography is peculiarly fortunate in possessing the exhaustive annual volumes of the *Bibliotheca Geographica*, published by the Berlin Geographical Society, the carefully selected annual bibliography of the *Annales de Géographie*, the critical and systematic chronicles of the *Geographische Jahrbuch*, and the punctual monthly lists and reviews of the *Geographical Journal* and *Petermanns Mitteilungen*, not to speak of the work of the *International Catalogue of Scientific Literature*. A great desideratum is an increase in the number of critical bibliographies of special subjects and particular regions, prepared so carefully as to relieve the student from the necessity of looking up any paper without being sure that it is the one he requires to consult, and to save him from the weary labor of groping through many volumes for fragmentary clues. In addition to the sources of information usually catalogued in one or other of the publications cited, there exist in every country numbers of Government Reports and quantities of periodical statistics too valuable to deserve their usual fate of being compiled, printed, stored away, and forgotten. There is scope for a great deal of hard but very useful and permanently valuable work, in throwing all these open to working geographers by providing analytical indexes. This would make it easier to discuss current Government statistics with the highest degree of precision, and to compare past with present distributions. All such statistics should be subject to a cartographical treatment no less rigidly accurate than the ordinary arithmetical processes.

The ultimate problem of geography may perhaps be taken as the determination of the influence of the surface forms of the earth on the mental processes of its inhabitants. But a host of minor problems must be solved in cutting the steps by which that culmination may be reached. Let us first find, if possible, what is the true relation between the elevation, slope, and exposure of land and climate; then the exact influence of elevation, slope, soil, exposure, and climate on vegetation; then the relation between all these and agriculture, mining, manufactures, trade, transport, the sites of towns, the political associations of peoples, and the prosperity of nations. After that we may consider whether it is possible to reduce to a formula, or even to a proposition, the relation between the poetry or the religion of a people and their physical surroundings. The chemist Chenevix wrote a book in two volumes a hundred years ago to demonstrate the inferiority of a particular nation, against one of whom he bore a personal grudge, and he was bold enough to attempt to justify the formula $C=f\lambda$, where C represented civilization, λ the latitude, and f a function so delicately adjusted as to make the value of C negative on one side of a channel twenty miles wide and positive on the other! We cannot hope to arrive by any scientific process at so definite a formula, but the only way of getting there at all is by forging the links in a chain of cause and effect as unbroken as that which led from the "House that Jack built" to "the priest all shaven and shorn."

The last of the problems of geography on which I intend to touch is that of the training of geographers. So far as elementary instruction in geography is concerned, I have nothing to say, except that it was bad, it is better, and it seems likely that it will be very good. But between geography as part of the education of a child and geography as the whole life-work of a man there is a gulf as wide as that between nursery rhymes and the plays of Shakespeare. The training of an elementary teacher in geography should be more thorough and more advanced than that of a child, but it need not be of a different order. The teacher, whose special function is teaching, must, like the child, accept the facts of geography from the authorities who are responsible for them. Although the two gifts are sometimes happily combined, an excellent teacher may make but a poor investigator.

A would-be geographer has at present adequate scope for training in very few universities outside Germany and Austria. Great advances have been made in the United States, but it is only here and there amongst the universities that steps have been taken to secure men of the first rank as professors, who are not only channels of instruction, but masters of research as well. In the United Kingdom there are lecturers on geography at several universities and many colleges; and, although they have done good work, the system adopted fails, in my opinion, on a practical point, — the lecturers are so inadequately paid that they cannot afford to give their whole time or their undivided attention to the subject with which they are charged. In such conditions progress cannot be rapid, and research is almost impossible. The absence of any well-paid posts, by attaining which a geographer would be placed in a position equivalent to that of a successful chemist or mathematician or botanist, kills ambition. The man with his income to make cannot afford to give himself wholly to such a study, however great his predilection for it. The man with as much money as he needs rarely chooses " to scorn delights and live laborious days;" and — with some bright exceptions — he has a tendency, when he turns to science at all, to study it rather for his own satisfaction than for the advance of the subject or the help of his fellows. We want some adequate inducement for solid scientific workers, well trained in general culture, and fitted to come to the front in any path they may select; to devote their whole attention — and the whole attention of such men is a tremendous engine — to the problems of geography. The laborer is worthy of his hire, and the services of the most capable men cannot reasonably be expected if remuneration equivalent to that offered to men of equal competence in other subjects is not available. At a few American and several German universities such men can receive instruction from professors who are masters of the science, free to undertake research themselves, and to initiate their students into the methods of research, — the best training of all. If the time should come when there are, perhaps, a dozen highly paid professorships in English-speaking countries, several dozen aspirants will be found, including, we may hope, a few more gifted than their masters, all qualifying for the positions, stimulated by rivalry, and full of the promise of progress. This is not an end, but the means to an end. Rapid progress is impossible without the stimulus of the intercourse of keenly interested and equally instructed minds. Geography, like other sciences, has to fight its way through battles of controversy, and smooth its path by wise compromises and judicious concessions, before its essential theory can be established and universally accepted. We can already see, though somewhat dimly, the great principles on which it depends, and they are becoming clearer year by year. As they are being recognized, they may be applied in a provisional way to current problems of practical life. The world is not yet so fully dominated by the highest civilization, nor so completely settled, as to deprive geographers of an opportunity of showing how the settlement and development of new lands can best be carried out in the light of the permanent relationships between land and people discovered by the study of the state of matters of long-settled areas at the present day and in the past.[1]

The practical politician, unfortunately, thinks little of geographical principles, and hitherto he has usually neglected them utterly. Many burning questions that have disturbed the good relations and retarded the progress of nations, even when they did not burst into the conflagration of war, would never have got alight had the consequences of some apparently trifling neglect, or some careless action, been understood beforehand as clearly by the man of affairs as by the student of geographical principles. Perhaps, when geography has obtained the status in the world of learning to which its ideals and achievements entitle it, the geographer may even be invited, when the occasion demands, to assist by his advice in saving his country from extravagance or disaster.

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[1] For a development of this suggestion see the author's *New Lands*, London, Charles Griffin, 1901.

THE RELATIVE VALUE OF GEOGRAPHICAL POSITION

BY HENRY YULE OLDHAM

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THERE is a factor involved in the consideration of many geographical problems which is too commonly overlooked. That factor is the element of time. Familiarity tempers human judgment, and the constant obtrusion of the more obvious naturally induces oblivion with respect to the remoter aspects of a case.

The New World looms so large in modern life that it is difficult to remember that till comparatively recent times in the history of mankind it was practically non-existent.

Ever since there has been an atmosphere surrounding the globe, there have probably been steady easterly winds in the tropical regions, with stronger but less regular westerly ones in the temperate climes. The sea, in essence a vast body of cold water, with a shallow upper layer of warm, in obedience to the working of the winds shows a tendency in the tropics to a heaping-up of the warm surface water on the eastern sides of continents towards which the winds blow, with an upwelling of the colder lower layers on the western sides, where the wind blows off the shore; while in the temperate zones, where the winds are reversed, the positions of warm and cold water are naturally also reversed.

The temperature of the sea has a marked effect on the life of the organisms which dwell in it, while its influence on the atmosphere above produces notable climatic effects on the land. Thus the warmer water on the eastern shores in the tropics conduces to the growth of coral reefs, which are as markedly present on the eastern coasts of Australia, Africa, and America, as they are conspicuous by their absence from the western. Similarly the contrast in the temperate zone, between the warm, moist climate of British Columbia and the frozen wastes of Labrador, is no less striking than the difference between the climates of western Europe and eastern Siberia.

Were there any new continents to be discovered, one could predict that their western shores would be warm and wet in temperate regions, their eastern ones in the tropics.

These are some of the salient and constant factors in geography.

Such also are the distribution of land and water, the configuration of the continents, their streams and mountains, their valleys and plains, which have changed but little in historical times; but familiarity with these features tends to forgetfulness of the fact that, like actors on a stage, their appearance in the theatre of history has been gradual, and that, though their actual positions have remained unchanged, their relative positions have varied through the ages, and moreover that they are often destined to play more parts than one.

A sea like the Mediterranean may at one time be the centre of commercial activity, and then become a backwater, while commerce streams along an ocean route round Africa. A few centuries pass, and the cutting of the Suez Canal, coupled with the development of steam navigation, restores it to its ancient and honorable estate as a highway of communication with the East, and the great cities on its shores, like Venice and Genoa, after a long period of decay, begin to resume their pristine vigor.

Since long before the beginning of human life, stores of gold and coal and other minerals have lain in the bosom of the earth; but their development as the sites of great centres of population has in most cases been essentially linked with the element of time. The rapid growth of Johannesburg into the position of the largest city of South Africa would have been as impossible without the recent discovery of the cyanide process, as was the development of the great coal-fields — the most striking factor in the shifting of great masses of population in modern times — until the invention of the application of steam power to machinery.

The great forces of nature show little tendency to change and may be usefully applied to the elucidation of many geographical problems, as for example in the case of the early voyages round the Cape of Good Hope. Bartholomew Dias, the first discoverer of the cape, encountering adverse westerly winds on his return voyage, must as assuredly have been there in our northern summer months, when the shifting of the trade-wind system brings the cape into the influence of the westerly winds, as was Vasco da Gama there in our winter months, when easterly gales prevail. Similarly the voyage of Odysseus may be largely elucidated with the help of a modern manual of sailing directions for the Mediterranean. But it is into the fluctuating fortunes of individual districts that the time-factor chiefly enters and demands a nice discrimination between the meaning of actual and relative geographical position.

Potentiality precedes performance. An island may be long the home of a hardy race of mariners before a field adequate to the display of their abilities is unveiled. The site of a great city may seem to have been predestined for centuries, before the opportunity for its development arises. Just as the obstacle presented by the lack of light in the short winter days of northern latitudes has been overcome by the invention of modern artificial illuminants, so the present barrier of unhealthiness to the development of the tropics may be removed by the discoveries of medical science. A place of business on the outskirts of a city is at a great disadvantage compared with one situated in the centre, but the expansion of the town may in course of time completely reverse their relative positions, without the smallest variation in their actual sites being made.

An interesting example on a small scale is presented by the fortunes of a famous English school. In the reign of Queen Elizabeth, a Warwickshire lad, Laurence Sheriffe by name, left his boyhood's home at Rugby to win fame and fortune in London. Mindful of his early days, and wishful to help succeeding generations, he left by will two fields in the neighborhood of Rugby, to provide the means for obtaining the aid of, if possible, a Master of Arts to teach the boys of his native town. By a fortunate inspiration, in a codicil to his will, two fields in the neighborhood of London were substituted for the original pair in the neighborhood of Rugby. At the time the two portions of land were probably of nearly equal value, but, though their actual position has never changed, their relative positions have undergone a revolution. The two fields near Rugby remain two country fields of little worth; the two near London in the reign of Queen Elizabeth are now in the heart of the great metropolis, and produce a princely revenue, on which the fortunes of the school at Rugby have been raised.

An example on a larger scale is offered by the history of England. It is a truism, often repeated, that the British Islands lie almost in the centre of the land-masses of the globe, an unrivaled position for wide-reaching empire and dominion. But this relative position is only one of recent growth. Five hundred years ago, a short period in the history of man, England was in a position of isolation on the very outskirts of the then known world. Shut in by the pathless barrier of the Atlantic on the west, and the untrodden wastes of Africa in the south, the only outlook of Europe was towards the east. Then was the Mediterranean, as its name implies, literally in the centre of the earth, the great scene of maritime activity. The principal nautical charts of the early fifteenth century, the Italian *portolani*, admirably reflect this state of affairs. The major part of the map is occupied by the Mediterranean, whose shores are studded with ports; a few of these on the west of Africa as far as the latitude of the Canaries, and several on the west of Europe as far as Flanders, indicate the limits of ordinary navigation. England, with only a few ports, chiefly on the south coast, is in the extreme corner of the map, separated by a long and hazardous sea-voyage from the great centre of activity. Under such conditions the central situation of Italy gave it a predominant position, and Venice and Genoa became the natural foci of commercial power.

It was only natural, also, that Italy should prove the birthplace of the great pioneers of geographical exploration. From Venice came Marco Polo, the great explorer of Cathay, who first to Western eyes unveiled the wonders of the East, and through whom Venice learned "to hold the treasures of the gorgeous East in fee;" from Genoa, Columbus, the pioneer of Western exploration, who sought, but failed, to find a western route to the Indies, and in his failure won a greater fame by the revelation of the road to a new and unsuspected world; while Florence saw the birth of Amerigo Vespucci, the scholarly explorer, who first realized that this new world was totally distinct from Asia, and so led to his name being inseparably linked with it. Cadamosto of Venice, sometimes called the Marco Polo of West Africa, the Cabots and Verrazano, pioneers of Western exploration for England and France, were likewise Italians.

The trade with the East, the home of silks and spices, — some once almost worth their weight in gold, — was till recent times the prize of the world's commerce. It was the fertilizing streams of Eastern commerce, pouring into the Mediterranean by various routes, but mainly up the Red Sea, which nourished Genoa and Venice. But gradually round the Levantine shores there spread, eventually from Cairo to Constantinople, the Turks, an alien race of alien religion; and Turkish dues, exacted on the inevitable land transit across Egypt from the Red Sea, proved a serious and increasing charge on the profits of this commerce.

In the latter part of the fifteenth century a merchant of Venice, writing to the King of Portugal, said that the greatest trade of Venice was with India, which came by way of Alexandria, whence the Turk derived great profit; he could not say where India was, but it was an affair for a great prince to undertake to find it, for if successful he would be exalted in riches and grandeur above all others.

The necessity of finding an ocean highway to the East, which would obviate the need of any land-break, with all its consequent expenses, had, however, been anticipated at an earlier period. The natural direction in which to seek such a route was round Africa. No one knew whether this were possible, or even if Africa had a southern end, but it was probable, and, indeed, the impartial record of an incredulous historian, Herodotus, had handed down the tradition of a Phenician circumnavigation of the continent six hundred years before the beginning of the Christian Era.

For the quest of a route round Africa, the relative position of the Iberian Peninsula at the time foreshadowed the preëminence of Spain or Portugal. That the initiative came from Portugal was partly due to the fact that that country had freed itself from the Moors, before the Christian reconquest of Spain was complete, but chiefly to the birth of one of those remarkable personalities that leave a permanent mark on the history of mankind.

In A. D. 1415, when just of age, Prince Henry of Portugal, third surviving son of the reigning king, distinguished himself so preëminently at the capture of Ceuta that he was offered the dignity of knighthood before his elder brothers, an added honor which he modestly declined. The fame of his attainments brought brilliant offers from other countries, but all were refused. Accepting the governorship of the southern province of his native land, he settled at Sagres near Cape St. Vincent, and practically devoted the rest of his life to one great idea, — the unveiling of the coast of Africa, in pursuance of the search for an ocean highway to the East. It is not easy to realize the difficulties that checked the work: the terrors of the unknown, the superstitions of his sailors, which long prevented their penetrating beyond the latitude of the Canaries, then the farthest limit known along the western coast of Africa. His indomitable persistence, however, prevailed. Gradually the inhospitable edge of the Sahara was passed, and the rich region of Senegambia discovered, so that, ere his death in 1460, Cape Verde, the westernmost point of the continent, had been rounded, and a district a little beyond the Gambia reached, while the island groups of the Madeiras, the Azores, and the Cape Verdes had been added to his country's dominions.

Compared with the long stretch of the African coasts, this may seem but a small achievement, but in itself is an indication of the initial difficulties to be overcome.

The first step had been taken; the rest was comparatively easy. As an example of the far-reaching designs of Prince Henry, it might be noted that at an early stage he obtained from Rome Papal bulls granting to Portugal all countries found, not in the possession of a Christian monarch, *usque ad Indos*. And so, after his death, the quest for the Indies was resumed, and gradually the long eastern trend of the Guinea Coast explored, and then the still longer southern stretch, until, after a little more than twenty-five years had elapsed, an end to Africa was found, and a cape hard-by happily named the Cape of Good Hope. The way seemed clear, and ten years later was proved to be so, when the gallant Vasco da Gama led the first expedition along a continuous ocean highway from Europe to India.

The first shot fired by the Portuguese on the Malabar Coast of India was the signal for the downfall of the commercial supremacy of Venice, and for three and a half centuries the great trade with the East was diverted, for some time to the exclusive benefit of Portugal, from its normal and ancient route up the Red Sea into a new Atlantic path, until the old order was restored by the cutting of the Suez Canal.

The century-long quest of the Portuguese to find this way round Africa was not likely to pass without some rival routes being advocated, and one there was which had a classic flavor.

To reach the East by sailing west was a natural corollary to the demonstration that the world was globular. Many of the Greek geographers had spoken of it, and though the famous Eratosthenes — who in the third century B. C. had measured the size of the earth with greater accuracy than any one attained to until quite modern times — had dismissed the scheme as impracticable owing to the extent of intervening ocean, the later Ptolemy, with restricted ideas as to the size of the earth and exaggerated notions of the extent of Asia, made it appear but a short voyage from the west of Europe westward to the east of Asia. This scheme, first mooted about the middle of the fifteenth century by Paul Toscanelli, an astronomer of Florence, won little sympathy from the Portuguese, who were rightly committed to the African route, but found an ardent advocate in Columbus.

After long waiting, in 1492, the consolidation of Spain, accomplished by the eviction of the Moors from their last stronghold in Granada, gave Columbus his opportunity, and in the service of Spain, the second of the two countries occupying the favorably situated Iberian Peninsula, he set out on his famous voyage, as the pioneer of Western exploration. A short voyage of less than five weeks from the Canaries, helped by the favoring trade-winds, revealed land, where land was anticipated. Asia had apparently been reached at the first attempt, by the easiest of voyages, and the name West Indies perpetuates the blunder to this day.

Other voyages quickly followed, and presently the great wonder of a new and unsuspected world was revealed, lying like a great barrier to the immediate object of the Western quest, but instinct with the greatest possibilities. A new route to an old world had not been found, but the path to a vast new continent, hitherto undreamed of, had been laid bare.

Twenty years after Columbus's first voyage, the sea that lay beyond the New World was first beheld by Nunez de Balboa,

> "When with eager eyes
> He star'd at the Pacific, and all his men
> Look'd at each other with a wild surmise —
> Silent, upon a peak in Darien."

But for all Keats's fine imagination, the marvel of the Pacific was as unsuspected as had been the existence of the New World. America was supposed to lie close up to Asia and only separated from it by a narrow sea.

It was nearly ten years later that Magellan, a native of Portugal in the service of Spain, found a passage through the straits which bear his name, at the southern end of the barrier continent, and, after a voyage of unrivaled difficulties, revealed the vast extent of the Pacific Ocean, which covers nearly half the whole surface of the globe. In this notable voyage, — the most notable, as a contemporary chronicler quaintly remarks, since that of the patriarch Noah, — the East Indies, where the leader lost his life, were reached by a western route, while one ship out of five completed the circumnavigation of the globe with a handful of men, who on their return crawled as humble penitents in sackcloth and ashes through the streets of Seville, because, having unconsciously lost a day in the voyage, they found that they had been keeping the fasts and festivals of their Church on the wrong dates.

With the unveiling of the Atlantic in the fifteenth century, — the conversion of what had been a pathless barrier into a great field for maritime activity, — a new era begins, the medieval Mediterranean epoch closes, and the modern oceanic period succeeds. The relative value of the position of the Iberian Peninsula for carrying out this great work was so preëminent that for some time Portugal and Spain were suffered to proceed unrivaled and unchecked. Indeed, by mutual agreement, a line of demarkation was drawn from north to south, about through the mouth of the Amazon, by which the whole undiscovered portions of the world were divided into two hemispheres, an eastern one for Portugal, a western one for Spain. But the very success which had been won wrought a revolution in the relative positions of the other lands in western Europe. England and France were equally well placed for undertaking western voyages.

It was the King of France who, in the sixteenth century, is said to have ironically invited Portugal and Spain to produce the will of our father Adam which constituted them his sole heirs. It was England, however, which mainly profited by the great change. Our island race of bold and skillful navigators had been only waiting for the opportunity of a field adequate to the display of latent powers. The time had come, and with the reign of Queen Elizabeth in the second half of the sixteenth century begins the expansion of England.

Sir John Hawkins was one of the first to dispute the exclusive right of Spain to traffic with the West Indies. Sir Francis Drake, the first to rival Magellan as a circumnavigator of the globe, was the most brilliant leader in the long struggle for the mastery of the sea which led up to the great tragedy of the Spanish Armada. Sir Walter Ralegh, no less an organizer of exploration than an explorer himself, by his attempts to colonize Virginia laid the foundation for the Anglo-Saxon dominion of North America.

Ralegh, Drake, and Hawkins, with most of their associates, were all Devon men, and this was only to be expected, for the position of Devon at the southwest corner of the land bears the same relation to the rest of England as in the earlier work the Iberian Peninsula bore to the rest of Europe, giving the Devon men for the time a positive advantage in the voyages undertaken to the famous cry of Westward, Ho! The period of their activity is ever recalled by the happy rhyme, which couples the dashing Drake with the famous Virgin Queen —

> "Oh! Nature, to old England still
> Continue these mistakes;
> Still give us for our Kings such Queens,
> And for our Dux such Drakes."

It was an English merchant, resident in Spain, who first suggested that, if feasible, a polar passage to Cathay would prove the shortest route, shorter than either the Portuguese path round Africa or the Spanish one across the Pacific, and that England was most favorably placed for undertaking the attempt to find one.

Attempts were accordingly made to discover a northeast passage, but soon a rival was found in the Dutch, who were equally well placed for such an undertaking. That the passage should eventually be completed long afterwards by Sweden is appropriate, when the position of that country is remembered.

It is, however, rather with the long search for a northwest passage that our countrymen are associated. From the time of Sir Martin Frobisher, the Columbus of the scheme, Davis of the Straits, and Baffin of the Bay, their names have been written largely on the map of North America, until the last link was forged with the life of Sir John Franklin.

When once England had ceased to lie on the outskirts of the known world, and had by the course of events become the centre of the land-masses of the globe, the path was clear to supremacy in maritime affairs. That the brilliant achievements of the sixteenth century were not continued in the seventeenth was due to internal political conditions. A century that saw the unhappy introduction of the Stuart dynasty, and its collapse after all the horrors of civil war, was not favorable to external development. A period of internal commotion is not adapted to external activity. Consequently, it is rather with the Dutch that the honors of exploration in the seventeenth century must rest. Boldly disputing the monopoly of the Cape route to the East Indies, they obtained a footing among those islands, and from that vantage-point prosecuted the unveiling of the great adjacent continent of Australia.

Unfortunately the region of New Holland, as they called it, which was first discovered, was mainly the arid western parts, and even when the continent was circumnavigated by Tasman, the fertile eastern coast was entirely missed. Hence, for the Dutch, Australia remained a region of possible future colonization, rather than one to be readily exploited.

A whole century was destined to pass before the fertile eastern shore was to be revealed, and then by a sailor of another nation, the English Captain Cook, who, after sailing in and out round the islands of New Zealand, of which Tasman had only seen a fragment, explored the whole of the east of Australia, and so opened the road to its colonization by a different nation from the Dutch.

The name of Captain Cook serves as a reminder that the eighteenth century saw a revival of maritime activity in England. It is with the great Pacific Ocean that his name is inseparably connected; east and west, north and south he penetrated to its utmost limits, revealing much of its wealth of islands, and finally sinking to rest in its waters, slain, like his great predecessor Magellan, in a petty skirmish, while endeavoring to protect his men.

Cook was the last of the great oceanic explorers. After him sailors were left, like Alexander, sighing for new worlds to conquer.

The nineteenth century, save for attempts to penetrate the polar fastnesses, has been mainly concerned with the exploration of the interior of continents, in which representatives of many nations have been engaged, for none have had special advantages of position.

The development of steam navigation has largely served to annihilate distance, and has destroyed much of the relative value of position, which gave some countries an advantage in earlier times, under other conditions.

One interesting result has been a revival of the early Italian eminence in exploration, the Duke of the Abruzzi's expedition having penetrated to the "Farthest North" yet reached, while in the recent attack on the Antarctic there has been a striking combination among a large number of countries.

Finally, the fact that the great Universal Exposition is held this year at St. Louis, where we are assembled, in the heart of North America, suggests a reflection on a change in relative position, which has affected many districts at different epochs, owing to a tendency for the spread of civilization to follow the course of the sun in its westerly path, — as Wordsworth puts it, "Stepping westward seem to be a kind of heavenly destiny." To the Assyrians of old, Europe itself was the West — *Ereb;* Moorish names in Portugal and Morocco represent the West of a later period, while Cape Finisterre similarly records the limit of the land. In the New World, the same phenomenon repeats itself, the centre of gravity in the distribution of its population moves steadily to the west, and the name of the "Far West" is losing its earlier significance. Already for some time the waves of civilization have reached the far Pacific shore.

One thought remains. The Middle Ages might fittingly be described as a Mediterranean epoch. Then followed the Atlantic period of modern times. The problems of the future seem largely bound up with the Pacific, and, indeed, signs are not wanting that we are entering on a new era.

SECTION G — OCEANOGRAPHY

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(*Hall* 8, *September* 21, 3 *p. m.*)

Chairman: Rear-Admiral John R. Bartlett, United States Navy.
Speakers: Sir John Murray, K.C.B., F.R.S., Edinburgh.
Professor K. Mitsukuri, University of Tokio.

THE RELATION OF OCEANOGRAPHY TO THE OTHER SCIENCES

BY SIR JOHN MURRAY

[**Sir John Murray**, Naturalist. **b.** Coburg, Ontario, Canada, March 3, 1841. K.C.B., Knight of the Prussian Order Pour le Mérite, F.R.S., LL.D., D.Sc., Ph.D.; Cuvier Prize, Institut de France; Humboldt Medal, Gesellschaft für Erdkunde, Berlin; Royal Medal, Royal Society; Founders' Medal, R.G.S.; Neill and Makdougall-Brisbane Medals, Royal Society of Edinburgh; Cullum Medal, American Geographical Society; Clarke Medal, Royal Society of New South Wales; Lütke Medal, Imperial Russian Society of Geography. Visited the Arctic Regions, 1868; one of the naturalists with H.M.S. *Challenger* during exploration of physical and biological conditions of great ocean basins, 1872–76; first assistant of staff appointed to undertake publication of scientific results of *Challenger* Expedition, 1876–82; appointed editor, 1882; took part in *Knight Errant* and *Triton* expeditions. **Author of** *A Summary of the Scientific Results of the Challenger Expedition*, and of numerous papers on subjects connected with geography, geology, oceanography, marine biology, and limnology; joint-author of *The Narrative of the Cruise of the Challenger;* and the *Report on Deep-Sea Deposits*. **Editor of** the *Report on the Scientific Results of the Challenger Expedition*.]

Within the past half-century our knowledge of the ocean has been very greatly extended by the explorations of scientific men belonging to nearly every civilized country. The depth of the ocean, the temperature, the composition, and the circulation of ocean waters, the nature and distribution of oceanic organisms and of marine deposits over the floor of the ocean, are now all known in their broad general outlines. We are at last in a position to indicate, and to speculate concerning, the relations of oceanography to the other and older sciences.

We now know that the greatest depth of the ocean below sea-level exceeds the height of the highest mountain above the sea-level. If Mount Everest, the highest mountain peak in the world, were placed in the Nero Deep in the North Pacific, where a depth of 31,600 feet has been recorded, its summit would be submerged by about 2600 feet, and if placed in the Nares Deep of the North Atlantic, where 28,000 feet have been recorded, it would form a small islet 1000 feet above the waves.[1] We now know about eighty-six areas in the ocean where there are depths exceeding three geographical miles (3000 fathoms). These areas, in which depths greater than 3000 fathoms have been recorded, have been called *deeps*, and a distinctive name, like *Nero Deep* and *Nares Deep*, has been given to each one of them. On the other hand, there are in the ocean basins numerous cones, rising in some instances above sea-level and forming coral and volcanic islands, or rising it may be to a few hundreds of feet below the sea-level. These elevated cones rising from the ocean's floor seem for the most part to be of volcanic origin; when they do not rise to the sea-level they are covered with a white mantle of carbonate of lime shells, mostly of plankton organisms where their summits are submerged half a mile or more. Disregarding these elevations and depressions, which are after all of small extent, it may be said that the general level of the bed of the great ocean basins is submerged about two and a half geographical miles beneath the general level of the surface of the continents. Were the ocean waters run off the globe, the solid surface of the sphere would appear like two great irregular plains, one of which — the continental areas — would be elevated nearly three miles above the other, — the floor of the great ocean basins; this is the fundamental geographical fact. In comparison with the size of our globe, this may seem a very small matter; still, it is important to inquire whether or not this great superficial appearance of the solid crust is part of its original structure, or has been brought about by agencies at work since the first crust was formed over the globe's incandescent surface, or since the first precipitation of water on the surface of our planet.

Geodesists tell us that their observations point to a deficiency of matter beneath the continental areas, and it seems possible that oceanographical researches may give some hint as to how this deficiency of matter may be accounted for. It is probable that most of the chlorine and sulphur now in combination in the ocean were carried down from the atmosphere with the first falls of rain on the surface of the primitive crust, in which we may suppose that all the silica was combined with bases, such as the alkalies, lime, magnesia, iron, manganese, and alumina. At a high temperature silicic acid (SiO_2) has a great affinity for bases, but at a low temperature it is replaced by carbonic acid (CO_2), which resembles silicic acid in many of its properties; geological History might indeed be represented as a continuous struggle between these two radicals for the possession of bases, At a high temperature SiO_2 is successful, while at a low temperature the victory rests with CO_2. In all the ordinary disintegrating processes at work on the surface of the earth since the first precipitation of rain, carbonic acid has been replacing silica from its bases; a large part of the silica thus set free goes to form the hydrated variety of silica, like opal, or ultimately free quartz. The bases are thus continually being leached out of the emerged rocks of the continents, and carried away to the ocean in solution, or in a colloid condition, the result being the ultimate deposition of the greater part of the heavier materials in the abysmal regions of the ocean and an accumulation of the lighter refractory quartz on or near the continental areas.

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[1] Greatest depth in the Pacific (Nero Deep) = 5269 fathoms; greatest depth in the Atlantic (Nares Deep) = 4662 fathoms.

A detailed study of marine deposits shows that, while quartz-sand forms generally the largest part of the deposits close to the continents, quartz-sand is, on the other hand, almost wholly absent from the abysmal regions of the ocean far from land, except where the sea surface is affected by icebergs. The average chemical composition of terrigenous deposits near land and of continental rocks shows about 68 per cent of free and combined silica. On the other hand, the average chemical composition of abysmal deposits shows only 36 per cent of silica. Continental rocks have an average specific gravity of 2.5. The abysmal deposits now forming on the floor of the ocean would make up rocks with a specific gravity of over 3.1. The superficial layers of the earth's crust on the continents must be, therefore, considered specifically lighter than the superficial layers of the earth's crust below the waters of the ocean.

Everywhere along continental slopes and in inclosed seas we find a series of strata being formed in all respects comparable with the stratified rocks of the geological series. Glauconite is to be found now forming on nearly all continental slopes, but it is not met with in the deep-sea deposits far from land. This same mineral is found in the same form as in recent deposits throughout the whole series of stratified rocks from the pre-Cambrian down to the present time, so that it is legitimate to assume that rocks in which it occurs were laid down close to continental land. Phosphatic nodules are very intimately associated with glauconitic deposits, and have the same distribution in the present seas; they are found along the submerged slopes of continental land, and are very rarely met with in deep water far from continental shores. There is a similar association of phosphatic nodules and glauconite throughout the whole geological series of past ages. These phosphatic and glauconitic rocks are now forming especially where ocean currents from different sources and of different temperatures meet, as, for instance, off the United States coasts in the Atlantic and Pacific, off the Cape of Good Hope, off eastern Australia, off Patagonia, and off Japan. In these areas there is a vast destruction of life, owing to sudden changes of temperature of the sea-water. The organisms in the cold current are killed from a sudden rise of temperature, the animals in the warm current from a sudden lowering of the temperature. When the tile-fish was nearly exterminated off the United States coasts in 1882, it was estimated that over hundreds of square miles there was a layer of dead marine fishes and other animals on the bottom six feet in depth. This vast destruction of marine organisms points clearly to the source of the phosphate in these deposits, and we obtain a hint as to the conditions under which greensand and similar rocks were laid down in past ages. Generally it may be said that in the terrigenous deposits along continental shores we have rocks now in process of formation which resemble closely the stratified rocks of the continents, so that these rocks may all be said to have been formed in varying depths in inclosed seas or along the continental slopes within two or three hundred miles from land.

It is quite different when we turn to the marine deposits now in process of formation towards the central portions of the great ocean basins. No geologist has yet been able to produce a specimen of a stratified rock which can with certainty be said to have been built up under conditions similar to those under which the typical red and chocolate clays, the Pteropod and Globigerina oozes, the Radiolarian and Diatom oozes of the central oceanic regions are laid down at the present time. These pelagic deposits cover considerably more than one half of the surface of our planet. The typical pelagic deposits are principally made up of the shells and skeletons of calcareous and siliceous organisms now living in the surface waters and of inorganic material derived from submarine eruptions, or of pumice and volcanic dusts floated or wind-borne from volcanic areas. The calcareous organisms play a most important rôle in the pelagic deposits, and their greater or less abundance, or complete absence, is more or less puzzling to the oceanographer. If, for instance, we should find in the tropical or subtropical regions of the ocean a cup-shaped or horseshoe-shaped elevation rising from the deep floor of the ocean, having a diameter, say, of fifty miles across, and the summit or edges of the cup rising to within 6000 feet of the surface of the ocean, while in the interior and on the outside of the cup the bottom descended to 20,000 feet below the waves, then we should find on the elevated edges of the cup deposits made up of 90 per cent of calcium carbonate, consisting almost wholly of the remains of pelagic organisms. As we descend into the hollow of the cup, or into the depths outside the cup, these organic remains would slowly disappear, till in the deposit at the bottom in 20,000 feet hardly a trace of calcareous organisms would be found, and the deposits there would consist of a red or chocolate clay derived from volcanic ejecta, with manganese-iron nodules, earbones of whales, sharks' teeth, and some cosmic spherules derived from meteorites. This hypothetical case represents what is found again and again throughout the ocean basins. Where exactly similar surface conditions prevail at the surface of the ocean two wholly different marine deposits are being formed on the floor of the ocean, the only varying condition being depth. The calcareous organisms are all dissolved away in falling through an ocean 20,000 feet in depth, or soon after they reach the bottom, whereas they nearly all reach the bottom at a depth of 5000 or 6000 feet, and there accumulate so as to form an almost pure deposit of carbonate of lime. The clayey deposit at 20,000 feet evidently accumulates with extreme slowness, the calcareous deposit at 6000 feet much more rapidly. The recent observations of telegraph engineers appear to show that, at one place in the North Atlantic, Globigerina ooze forms at the rate of about one inch in ten years.[1] In the case of terrigenous, as well as of pelagic deposits, it has been shown that two very different deposits, both in organic and inorganic constituents, may be formed in the same area at the same time, but in different depths.

All these considerations go to show that the deposits formed in inclosed seas and along the borders and slopes of emerged continental land have again and again been shoved up on the continental areas to form dry land, by the action of those internal forces called into play through the solid crust accommodating itself to a shrinking nucleus. And, further, it follows that more than one half of the surface of the planet — the abysmal regions of the great ocean basins — may never have contributed to the formation of those stratified rocks of which continental land is so largely made up. The continents have been far from permanent and stable, but those areas on the surface of the planet now occupied by the continents and the adjacent marine terrigenous deposits appear, from the foregoing argument, to have been the areas on the surface of the planet on which continental land has been situated from the very earliest ages. The grand result of all the denuding and reconstructing agencies since the first precipitation of rain has been the building-up on these continental areas of a great mass of lighter highly siliceous materials. If this has been the course of the evolution of the present continental areas, then it appears amply to account for the deficiency of matter beneath the continents indicated by pendulum observations, and for the alleged fact that along continental shores the plumb-line tends towards the ocean basins, where the heavier materials have been accumulating on the earth's surface, ever since the first precipitation of water on the cooling crust.

Temperature may be defined as that state of matter on which depends its relative readiness to give or to receive heat. Variations of temperature are intimately associated with all changes in nature, and nowhere are the effects of these variations of temperature more pronounced than in the ocean. The relations of oceanography to many other sciences can best be exemplified by a consideration of the distribution of temperature in the waters of the ocean. Nearly all the sun's rays falling on water are at once diffused downwards to at least 600 feet. So great is the thermal capacity of water that a unit of heat only raises the temperature one degree, while the same amount will raise the temperature of rocks four or five degrees.

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[1] See Murray and Peake, *On Recent Contributions to Our Knowledge of the Floor of the North Atlantic Ocean*, *Roy. Geogr. Soc.*, Extra Publication, 1904, p. 21.

It is well known that our planet is surrounded by three atmospheres: one of oxygen, one of nitrogen, and one of water-vapor. In the case of oxygen and nitrogen a complete mixture takes place throughout the whole atmospheric envelope. A complete mixture never takes place in the case of water-vapor, because its equilibrium is continuously disturbed by changes of temperature, which may reduce the vapor to the liquid or solid state; evaporation and condensation, freezing and melting, are ceaseless at the surface of the earth. It has been shown by numerous observations that in the open ocean far from land the daily fluctuations of temperature in the surface waters do not exceed one degree F. Hence the atmosphere over the ocean may be regarded as resting on a surface the temperature of which is practically uniform at all hours of the day. This is in striking contrast to what takes place on the land surfaces, where solar and terrestrial radiation produce a very wide daily range of temperature. On the sand of the Sahara and the American deserts the temperature ranges about 100° from three A. M. to three P. M. The temperature of the air immediately over the ocean has a slightly greater daily range than that of the water, — being some three or four degrees F., — but this is in no way comparable to the enormous daily range of the air resting on the land surfaces. Here we come on one of the prime factors of meteorology, which must be considered in connection with some other facts. As the diurnal oscillations of the barometer occur alike over the sea and land, it follows that this diurnal oscillation is caused by the direct and immediate heating of the molecules of the air and its aqueous vapor by solar radiation. Air with a large quantity of water-vapor absorbs more of the sun's rays, becomes in consequence more heated, and is specifically lighter than dry air; hence air ascends in cyclonic and descends in anti-cyclonic areas. The diurnal variation in the elastic force of the vapor in the air is seen in its simplest form over the open ocean, and the diurnal variation in the force of the wind, and the diurnal variation in the amount of cloud are both much less over the open ocean than over the land. All these conclusions derived from observations at sea go a long way towards a rational interpretation of many atmospheric phenomena, such as the unequal distribution of the mass of the earth's atmosphere, the ascending currents in cyclonic areas, the descending currents in anti-cyclonic areas, the prevailing winds, and the greatly diversified climates in different parts of the world. The aqueo-aërial currents from sea to land, and the oceanic currents thus brought about by changes of temperature in the atmosphere, are the great equalizers of temperature in diverse regions; for instance, except for these currents the mean winter temperature of London would be 17° F. in place of 39° F., London thus being benefited 22° F., while the Shetland Islands to the north of Scotland are benefited 36° F. by the Gulf Stream and the aqueo-aërial currents due to the winds from the southwest.

At the surface of the earth, both on land and on sea, bands of equal temperature run more or less parallel to the equator. This is true, notwithstanding the fact that oceanic currents cause wide deflections, as, for instance, in the case of the Gulf Stream: on the sea-floor the bands of equal temperature run north and south along the continental shores.

The extreme range of temperature in the surface waters of the ocean is from 28° to 95° F., and 84 per cent of the surface waters have a temperature exceeding 40° F. There is a circum-tropical zone where there is a high temperature and small range not exceeding 10° F., which embraces most of the coral-reef regions of the world, and there are two circum-polar zones where there is a low temperature and small range not exceeding 10° F., where carbonate-of-lime secreting organisms are poorly developed. Between these two polar zones and the circum-tropical zone are two intermediate zones where there is a wide range of temperature. It is in these intermediate zones that warm currents occupy the surface at one season of the year and cold currents at another season, and here there is a consequent great destruction of marine life. This gives us some indication of the conditions under which phosphatic and glauconitic deposits were laid down in past ages.

Many areas at the surface of the ocean used formerly to be regarded as barren and devoid of life, but there are no such barren regions. The whole surface of the ocean — both in cold and warm waters, and down to a depth of 600 feet — must be regarded as a vast meadow, more extensive and more important than the vegetable covering on land-surfaces. Everywhere there are myriads of Diatoms, calcareous and other microscopic Algæ with a red-brown color, the chlorophyll in which is ever busy under the influence of the sun's rays converting inorganic into organic compounds. These minute organisms are the original source of food for the vast majority of marine animals both in the surface waters and on the floor of the ocean, even at the greatest depths. The reserve food of these minute organisms is little globules of oil, instead of granules of starch which prevail in terrestrial vegetation. This is doubtless the original source of the oil which appears in marine fishes, birds, and mammals in such abundance.

Many interesting physiological problems are suggested by the study of oceanography. In the ocean there are very few warm-blooded and air-breathing animals, and we have to deal chiefly with cold-blooded animals, the temperature of whose blood and bodies rises and falls with that of the water in which they live. In the tropics marine animals — for instance, a Copepod or Amphipod, — pass all their lives in water with a temperature of 80° to 90° F. In the polar seas a quite similar animal passes the whole of its life in water below the freezing-point. In these cases it is evident that the metabolism of the warm-water animal is much more rapid than that of the cold-water one; it reproduces its kind much more frequently, and its individual life is shorter than in the case of the cold-water animal. All chemical and all physiological changes take place much more rapidly in warm than in cold water. In cold sea-water there is much albuminoid ammonia, in warm water regions much saline ammonia, which fact points to more rapid change in the warm water of the ocean. By remembering these conditions we may account for the fact that genera and species are much more numerous in the warm water, while on the other hand the species are few, but the individuals of a species are enormously greater, in the cold water. The animals in a tow-net from the tropics are most probably not more than a few weeks old, whereas a similar tow-net in the polar waters captures animals, some a few weeks old, and others, it may be, years of age. It seems certain that the warm tropical waters are the most favorable for vigorous life and rapid change, and here the struggle for life is most severe, and the evolution of new species much more frequent, than in the cold waters of the poles or the deep sea. In this direction we must look for an explanation of the so-called bipolarity in the distribution of marine organisms.

A great characteristic of organisms in warm tropical waters is the very large quantity of carbonate of lime they secrete from the ocean. This is evident, not only in the massive coral reefs, but also in the abundance of calcareous organisms in the plankton of the tropics — like coccospheres, Globigerinæ, and mollusks. All these lime-secreting organisms become less abundant as we approach the poles, or descend into the deep sea. In the warm water the carbonate of lime is deposited in shells and skeletons as aragonite, but in the cold water it is deposited much more slowly, and in the form of calcite. This shows that when we find a limestone rock with abundance of fossil-shells we may assume that it was laid down in a warm sea where the temperature approached 70° or 80° F. It may be safely asserted that at the present time lime is being ac cumulated towards the tropics through the action of lime-secreting organisms which obtain the lime from the sulphate of lime in sea-water.

The great abundance of pelagic larvæ of benthonic organisms in warm tropical surface waters, their periodicity in the intermediate zones with a wide range of temperature, and their almost total absence in polar waters and in the deep sea, are facts in distribution of great interest to the biologist and evolutionist, and may be accounted for by the varied temperature conditions in the several areas.

The temperature conditions in the deep sea and on the floor of the ocean form a striking contrast to those prevailing in the surface waters. The lines of equal temperature, instead of running parallel to the equator, as at the surface, run on the whole north and south, following the general trend of the continents. The water which rests immediately on the ocean's floor in great depths has nearly everywhere a temperature under 40° F., and a very large part of it is below the freezing-point. Only a small band running north and south in shallow water along the continental shores has a temperature over 40° F. It follows, then, that much more than one half of the solid crust of our globe is kept at a low temperature at all times. The abysmal regions have not only a low temperature, but eternal darkness reigns there so far as the sun's rays are concerned; any motion which takes place in the water must be of extreme slowness. Transport and erosion do not take place in this deep region, which is an area of deposition. The materials composing the deposits, being saturated with sea-water during immense periods of time, become highly altered, and secondary products are formed in and on the surface of the deposits, such as manganese-iron nodules, palagonite, and zeolitic crystals.

The animals which have been able to accommodate themselves to life in the abysmal regions derive their food primarily from the dead organisms and excreta which have fallen from the surface waters: they are, indeed, mud-eaters. There is much reason to believe that the whole of the marine deposits are sooner or later eaten by organisms; it is, indeed, probable that all stratified rocks, whether marine or lacustrine, have in like manner passed through the intestines of animals. In many instances the excreta of the benthonic animals are converted into glauconitic and phosphatic grains. Phosphorescent light plays a large part in the economy of marine organisms, and it is a remarkable fact that this phenomenon of phosphorescence has never been observed in any fresh-water organisms. Some deep-sea animals are blind, some have very large eyes, some have highly developed tentacular organs. Some have complicated organs for the emission of light, some are many times larger than their shallow-water allies, while others are much smaller. All have a rather feeble development of calcareous shells and skeletons and a rather sombre color. All these modifications can be satisfactorily explained by reference to the pressure, the temperature, the food, the light, and other physical and biological conditions to which we have referred as prevailing in the deep water of the great ocean basins.

A point of some interest to paleontologists is that in deep marine deposits the remains of marine organisms which lived on the bottom in cold water with a temperature below zero are mingled with the remains of surface organisms which lived at a temperature of 80° F.

It has been shown by hundreds of analyses of ocean-water from all parts of the world that the chemical composition of sea-water, that is to say, the ratio of acids to bases in sea-salts, is very constant, with some insignificant exceptions. Sea-water has acted as a gigantic solvent; it almost certainly now contains every known chemical element. The salts now present in solution represent what water has been able to leach and filter out of the solid crust and sea-water has been able to retain in solution. The history of the composition of sea-water should be the complement of all the terrestrial changes that have taken place on the dry land of the continental areas. An endeavor may be now made to trace that history, in the same way that the geologist and paleontologist trace the evolution of the stratified rocks. We have now many indications that the composition of the sea-water salts — or rather, the proportion in them of the various elements — has continually changed from that of the primeval ocean.

It has been pointed out that Radiolaria, Diatoms, and other silica-secreting Protozoa and Protophyta, are more abundant where sea-water mixes with a large amount of fresh water in the present ocean, as, for instance, in the tropical West Pacific and in the Antarctic. When we remember the abundance of Radiolaria in Paleozoic schists, it seems to show that in the early seas there was much more detrital and colloid silicate of alumina in ocean waters than at the present time, the oceans being on the whole much shallower and less salt. Again, in the present seas lime-secreting organisms are much more abundant in the warmest and saltest waters than elsewhere. This indicates, when the small development of limestone in the earliest stratified formations is considered, that lime was less abundant in the pre-Cambrian oceans than in our seas. Indeed, water before the formation of soil on the land surfaces would carry to the ocean very different salts in solution from those carried at this time. Potassium, for instance, is absorbed at the present time by all soils, and the same element has from the earliest times been extracted from the sea-water to form glauconite. Potassium is, then, probably much less abundant now than in the primeval ocean. Lime has also been extracted in greater abundance in recent than in ancient seas. This occurs especially in the warmest and saltest seas.

Marine organisms have had to accommodate themselves to the slowly-changing conditions of the ocean which I have just indicated, and it seems evident that the animal and vegetable protoplasm must have established fixed relations with the elements in solution in sea-water. This relation would almost certainly be handed on by heredity, for there is no reason for supposing that morphological structure can be handed down in this way, and not chemical composition. When we have a fuller knowledge of the chemical composition of the soft tissues of the different groups of marine organisms, and of the composition of their circulatory fluids, we may possibly be able to read the history of the ocean as clearly as the paleontologist reads the history of the rocks.

THE CULTIVATION OF MARINE AND FRESH-WATER ANIMALS IN JAPAN

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WHILE the pasturage of cattle and the cultivation of plants marked very early steps in man's advancement toward civilization, the raising of aquatic animals and plants, on any extensive scale, at all events, seems to belong to much later stages of human development. In fact, the cultivation of some marine animals has been rendered possible only by utilizing the most recent discoveries and methods of science. I believe, however, the time is now fast approaching when the increase of population on the earth, and the question of food-supply which must arise as a necessary consequence, will compel us to pay most serious attention to the utilization for this purpose of what has been termed the "watery wastes."

For man to overfish, and then to wait for the bounty of nature to replenish, or, failing that, to seek new fishing grounds, is, it seems to me, an act to be put in the same category with the doings of nomadic peoples wandering from place to place in search of pastures. Hereafter, streams, rivers, lakes, and seas will have, so to speak, to be pushed to a more efficient degree of cultivation and made to yield their utmost for us. It is perhaps superfluous for me to state this before an audience in America, for I think all candid persons will admit that the United States, with her Bureau of Fisheries, is leading other nations in bold scientific attempts in this direction.

Nor is it simply from the utilitarian standpoint that more attention is likely to be paid in future to the cultivation of aquatic organisms. Far be it from me to depreciate in any way beautiful modern laboratory technique, but I think all will agree the time is now gone by when science considered that when the morphology of an animal has been made out in the laboratory all that is worth knowing about it has been exhausted. We have been apt to forget that animals are living entities, and not simply a collection of dead tissues. But we are now beginning to realize that in order to arrive at the proper understanding of biological phenomena we must, in addition to laboratory methods, observe living animals in their natural environment, or study them by subjecting them to accurate scientific experiments. To show the efficiency and intricate nature of the new methods, I need only refer to the important results obtained by Professor Ewart, of Edinburgh. And America has also already started a zoölogical experimental farm, under the able directorship of Professor Davenport.

From this standpoint the cultivation of various organisms becomes an important and necessary aid to scientific researches, and it is partly for this reason that I venture to call your attention to some of the more successful of culture methods practiced in Japan.

Japan, I need hardly remind you, consists of an immense number of islands, large and small. In proportion to its area, which is nearly 160,000 square miles, its coast-line is immense, being, roughly speaking, 20,000 miles. This is broken up into bays, estuaries, inlets, and straits of all sorts and shapes, with an unusually rich fauna of marine organisms everywhere. In addition, the country is dotted with lakes and smaller bodies of fresh water. Put these natural conditions together with the facts that the population, in some districts at least, has been extremely dense, and that until within comparatively recent times hardly any animal flesh was taken as food, and even at the present day the principal food of the general mass of people consists of vegetables and fish, — it would be strange indeed if the cultivation of some aquatic organisms had not developed under these circumstances. And such is actually the case. For instance, the oyster culture of Hiroshima and the algæ culture of Tokyo Bay are well-known industries which have been carried on for hundreds of years. Within recent times there has been a development of a number of such enterprises, some of which are interesting even from the purely scientific standpoint. It is my intention to call your attention to the more important of these culture-methods, giving preference to those which are peculiar to Japan, and which might be interesting not only from the economic aspect, but as a means of scientific investigation.

The Snapping-Turtle, or Soft-Shell Tortoise, "Suppon" (*Trionyx japonicus* Schlegel)

The place occupied among gastronomical delicacies by the diamond-back terrapin in America and by the green turtle in England is taken by the suppon, or the snapping-turtle, in Japan. The three are equally esteemed and equally high-priced, but the Japanese epicure has this advantage over his brothers of other lands, — he has no longer any fear of having the supply of the luscious reptile exhausted. This desirable condition is owing to the successful efforts of a Mr. Hattori, who has spared no pains to bring his turtle-farms to a high pitch of perfection, and is able to turn out tens of thousands of these reptiles every year. As his are, so far as I am aware, the only turtle-farms in the world which are highly successful, a description of his establishment and methods will, I think, prove interesting and serve as a guide to those who may have similar undertakings in view. In passing, I may remark that I have known Mr. Hattori these twenty years and have spent a number of summers on his original farm, collecting, with his kind consent, ample materials for my studies on the development of Chelonia. In return, Mr. Hattori is kind enough to say some of the facts and suggestions I have been able to give him, based on my embryological studies, have been of service in carrying out improvements.

The Hattori family has lived a long time in Fukagawa, a suburb of Tokyo, which lies on the "Surrey" side of the Sumida River, and which, having been originally reclaimed from the sea, is low and full of lumber-ponds [1] and until recently of paddy-fields. The occupation of the family was that of collecting and selling river-fishes, such as the carp, the eel, and the crucian carp, and of raising goldfishes, in addition to the ordinary farmer's work. As far back as in the forties of the last century, the high price commanded by the "suppon" seems to have suggested to the father and the uncle of the present Hattori the desirability of cultivating it, and this idea, once started, seems never to have been lost sight of, although lying in abeyance for a long time.

In 1866 the first large turtle was caught, and since then additions have been made by purchase from time to time, so that in 1868 there were fifteen, and by 1874 the number reached fifty, which were all very healthy, with a good admixture of males and females. In 1875 these were placed in a small pond of 36 tsubos,[2] with an island in the centre which was intended for the turtles to lay eggs on. They, however, seemed to prefer for this purpose the space between the water-edge and the outer inclosure; hence, to suit the tastes of the reptile, the pond was hastily modified into a form very much like the one in use at the present day. That year over one hundred young were hatched, but, unfortunately, they were allowed to enter the pond in which the adults lived, and all but twenty-three of them were devoured, making it evident that some means were necessary to protect them from their unnatural parents. Thus was gradually evolved the present system of cultivation.

In general appearance a turtle-farm is at a first glance nothing but a number of rectangular ponds, large and small, the large ones having a size of several thousand tsubos. The ponds are undergoing constant modification, being united or separated just as need arises, so that their number may vary considerably at different times. Figure 1 gives the plan of the Hattori turtle-farm at Fukagawa as at present laid out. There pass through the farm two small canals which communicate on the one hand with the river across the road, and on the other with the ponds, so that the water can be drawn into, o emptied from, each of them at will.

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[1] Ponds in which lumber is kept soaked in water.

[2] One tsubo, an area six feet square, is the unit in the measurement of sm land surfaces.

All the ponds, whether large or small, are constructed very much on the same plan. They are limited on their four sides by plank walls, the top of which may either be on the level of the ground (see the right side of the section, Fig. 2), or may be more than a foot above the ground when two ponds are contiguous (the left side, Fig. 2). In either case the plank wall has a cross-plank of some width at right angles to it on its top, and is also buried some inches in the ground. The former arrangement is, of course, to prevent the tortoises from climbing over the wall, and the latter to prevent them from digging holes in the ground and making their escape in that way, while at the same time it serves to exclude the moles. On the inner side of the plank wall there is more or less of a level space, and then a downward incline of three or four feet. At the foot of this incline and directly around the water's edge there is another level space which enables people to walk around the pond. From the edge of the water the bottom of the pond deepens rather rapidly for a space of some three feet, and there reaches the general level of the bottom, which is about two feet below the level of the water. The greatest depth of a pond is about three feet and is always toward the water-gate by which the pond communicates with the canals. The bottom is of soft, dark mud, several inches thick, into which the tortoises are able to retire to pass the winter.

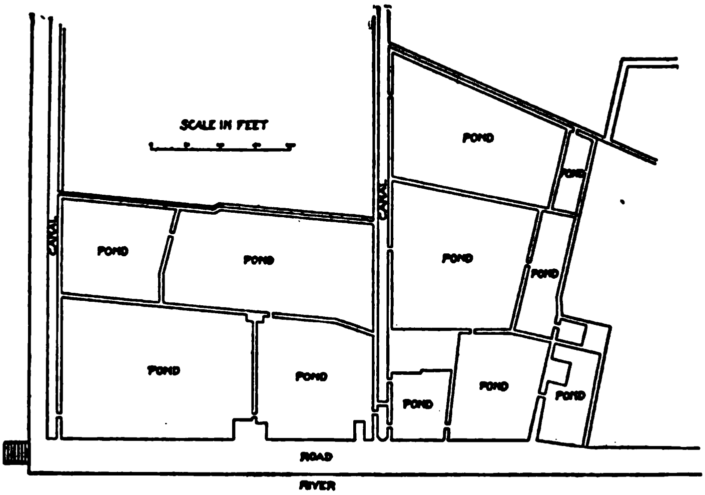

FIG. 1. – Plan of a turtle-farm

On a turtle-farm one or more of the ponds is always reserved for large breeding individuals, or "parents," as they are called. The just-hatched young or the first-year ones must have ponds of their own, as must also the second-year ones; those of the third, fourth, and fifth years may be more or less mixed.

In order to give a connected account of the raising of tortoises, we might begin with a description of the pond for large breeding individuals, or "parents," and with an account of egg-laying and hatching.

The "parents' pond" does not differ in any remarkable way from the general plan of a pond given above. Usually one of the largest ponds is chosen, and it can be distinguished from the others, because one or two of its slopes are usually kept up very carefully, while the other slopes, or those of other ponds, are apt to be worn by rain and wind and to become rugged. These well-kept slopes are invariably on the warmer sides, where the sun pours down its midsummer rays longest, and are carefully worked over in the spring so that the tortoises will find it easy to dig holes in them. In the breeding-season these sides are seen to be covered with wire baskets which mark the places where the eggs have been laid.

Copulation takes place on the surface of the water in the spring. Egg-deposition begins in the last part of May and continues up to the middle of August. Each female lays during that time two to four deposits, the number differing with individuals and with years.[1] The process of egg-deposition is very interesting. A female comes out of the water and wanders about a little while on the banks of the pond in search of a suitable locality in which to deposit eggs. Having finally chosen a spot, with her head directed up the bank, she firmly implants her outstretched forefeet on the earth, and during the whole operation never moves these. The process of egg-deposition, which takes altogether about twenty minutes, may be divided into three portions, occupying about the same length of time, namely: (1) digging a hole, (2) dropping eggs in it, and (3) closing the hole. The digging of the hole is done entirely with the hind legs. Each, with its nails outstretched, is moved firmly from side to side — that is, the right foot from right to left and the left from left to right, and the two are worked in a regular alternation, while the body is swayed a little from side to side, accompanying the motion of the legs. The force put in the lateral pressure of the feet is so strong that the earth that has been dug out is sometimes thrown off to a distance of 10 feet or more, although the largest part of it is heaped up around the hole. Digging seems to be continued as long as there is any earth within the reach of the legs to be brought up. The result is a squarish hole with the angles rounded off, and although its size differs with the size of the female, it is generally about three to four inches across at the entrance, with the depth and width inside about four inches or more. When digging is finished eggs are dropped from the cloaca into the hole, which naturally lies just below it. The eggs are heaped up without any order, but, there being no chalazæ, the yolk is able to rotate in any direction, and the blastoderm, having the least specific gravity, always occupies the highest spot of the yolk in whatever position the egg may happen to be dropped. The eggs are generally spherical in shape, although sometimes more or less oblate. Their diameter is in the neighborhood of twenty millimeters, the largest being as large as twenty-four millimeters, the others smaller according to the size of the females. The number of eggs in one deposit varies from seventeen or eighteen up to twenty-eight or more, the smaller individuals producing the smaller number.

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[1] See my notes: "How many times does the snapping-turtle lay eggs in one season?" *Zoölogical Magazine*, vol. VII, p. 143, 1895, Tokyo.

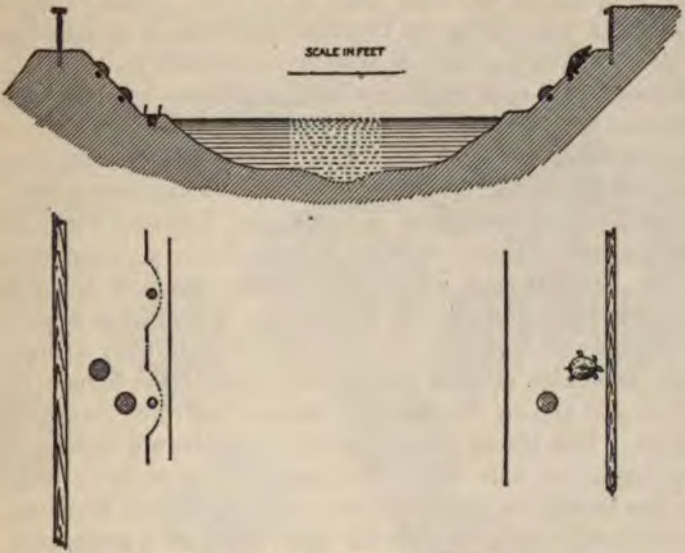

FIG. 2. – Section and plan of a turtle-pond

When the eggs have all been deposited, the turtle's legs are again put in requisition, this time to fill up the hole, which is done by alternate motions as before. The earth about the hole is used at first, but search is made for more loose earth for a little distance, as far around as the legs can reach with a slight motion of the body either to the right or left without moving the front legs. Toward the end of the process the loose earth is trampled down. When the hole is well filled up to the level of the ground, the turtle turns around and goes immediately down into the water, not casting even one backward glance.

I have noticed an interesting contrast between the behavior of *Trionyx* and of *Clemmys* during the egg-deposition. If one wants to watch a *Trionyx* depositing eggs, one has to crawl on all fours behind the plank wall of the pond and peep through a hole, being careful not to show one's self. The moment the snapping-turtle sees any one, it stops in whatever part of the egg-laying process it may be engaged and plunges straight into the water. Utterly different is the behavior of *Clemmys.* When once it begins the process of egg-laying it is never deterred from carrying it out, no matter how near or how boldly one may approach. Whenever I watched a *Clemmys* working away in the direct midsummer rays, with its carapace all dried up and with its eyes alone moist, I could not help comparing it to a slave of duty fulfilling his fate with tears in his eyes. What causes such a difference of behavior in the two species? What is its significance? What difference in the nervous system corresponds to it?

The traces of a spot where the snapping-turtle has laid eggs are (1) the two marks made by the forepaws holding on to the earth during the whole operation, and (2) a disturbed place some distance back of the line of the forepaws where the hole has been made. The three marks are at the angles of a triangle. I have noticed a very interesting fact in regard to these traces. When a young female is depositing her first eggs, she is very clumsy, the hole being badly made and the filling-in of it very imperfect, so that often a part of it remains open. Old females are extremely neat in their doings, and one can determine at once the age and size of the female by the skill displayed and by the distance between the three marks of egg-deposition. This shows that, although the elaborate actions necessary in egg-laying must be, in the main, due to instinct, each individual has to add its own experience to the inherited impulses, and is able thus only to accomplish the desired end with perfection.

In Hattori's farm a person goes around the "parents' pond" once a day or so and covers up with wire baskets all the new deposits made since the last visit. Each basket may be marked with the date if necessary. This covering serves a twofold purpose, — the obvious one of marking the place, and in addition that of keeping other females from digging in the same spot. When hundreds, or even thousands, of these baskets are seen along the bank of a "parents' pond," it is a sight to gladden the heart of an embryologist, to say nothing of that of the proprietor.

The hatching of the eggs takes, on an average, sixty days. The time may be considerably shortened, or lengthened, according to whether the summer is hot and the sun pours down its strong rays day after day, or whether there is much rain and the heat not great. It may become less than forty days or more than eighty days. By the time the last deposits of eggs are made in the middle of August, the early ones, which were laid in May or June, are ready to hatch; and inasmuch as, if small tortoises that have just emerged from the eggs are allowed to get into the "parents' pond," they are devoured by their unnatural fathers and mothers, a special arrangement has now to be put up to prevent this. The left side of the plan in Fig. 2 is intended to show this arrangement. Long planks about eight inches wide are put up lengthwise around the edge of

the pond, leaving perhaps one foot margin between them and the water. Two successive planks are not placed contiguous, but a space of about three feet is left between every two, and closed by a bamboo screen put up in the shape of an arc of a circle, with its convexity toward the pond. Thus the slope or the bank where the eggs have been deposited is completely cut off from the pond itself. In the centre of every pocket-like arched space made by a bamboo screen an earthenware jar is placed, with its top on the level of the ground, and some water is put into it. This elaborate arrangement is for the reception of the young tortoises, which, as soon as they break through the egg-shells, — those belonging to the same deposit generally coming out at the same time, — crawl up to the surface of the ground by a hole or holes made by themselves, and go straight down the incline toward the pond, as naturally as the duckling takes to the water. They are stopped, however, in their downward hydrotaxic course by the planks put up, as stated before, around the pond, and they crawl along the length of the planks, and sooner or later drop into the jars placed in the recesses between every two planks. A man going around once or twice a day can easily collect from these jars all the young hatched since the last visit.

The young just hatched are put into a pond or ponds by themselves and given finely chopped meat of a fish like the pilchard. This is continued through September. In October *Trionyx* ceases to take food, and finally burrows into the muddy bottom of the pond to hibernate, coming out only in April or May. The young are called the first-year ones until they come out of their winter sleep, when they are called the second-year young. At first the same kind of food is given these as that given to the first-year young, but gradually this may be replaced by that given to older individuals, namely, any fish-meat or crushed bivalves, etc. From the third to the fifth year, inclusive, the young need not be kept in ponds strictly according to age, but may be more or less mixed, if necessary. The young of these years are also the best and most delicate for eating, and are the ones most sold in the market. In the sixth year they reach maturity, and may begin to deposit eggs, although not fully vigorous till two or three years later. How old these snapping-turtles live to be is not known. Those one foot and more in length of carapace must be many years old. The following table gives the average size of the carapace and the weight of the young:

Age.	Length in centimeters.	Breadth in centimeters.	Weight in grams.
Just hatched	2.7	2.5	
First year	4.5	4.2	23
Second year	10.5	8.8	169
Third year	12.5	10.5	300
Fourth year	16.0	13.5	563
Fifth year	17.5	15.1	750

One of the most important questions in turtle-farming is that of food-supply. The profit depends largely on whether a constant supply of healthful food can be obtained cheaply and abundantly. In the Hattori farm chief dependence in this respect is laid on the "shiofuki" shell (*Mactra veneriformis*, Deshayes) which occurs in enormous quantities in the Bay of Tokyo. These shells are crushed under a heavy millstone rolled in a long groove in which they are placed. Other kinds of food given are any dried fish-scraps, silkworm pupæ, boiled wheat-grains, etc.

A curious part of the ecological relations of a turtle-pond is this: It would be supposed that putting other animals in the same pond with the snapping-turtles would be detrimental to the welfare of the latter, but experience has proved just the contrary. It is now found best to put such fishes as carp and eels in the same ponds with the turtles. The reason, I am told, is that these fishes stir up mud and keep the water of the pond always turbid, and this is essential to the well-being of the turtles, as is proved when the messmates are taken out of the pond. Dirt and mud then settling down, and the water becoming clear and transparent, the turtles, which are extremely timid, will not go about searching for food, and thus very undesirable results are brought about.

The business of turtle-raising has thrived well. When I first became acquainted with the turtle-farm, now over twenty years ago, it was a small affair with only a few small ponds, and the eggs hatched out in one year were, all told, not much over 1000. Now the enterprise embraces three establishments: (1) The original farm at Fukagawa, Tokyo, now enlarged to seven acres; (2) the large farm at Maisaka, near Hamamatsu, province of Totomi, over 25 acres, whither the main part of the business has been transferred; and (3) the second farm in Fukagawa, about two acres in extent. These three establishments together will yield this year (1904) about 4100 egg-deposits, which means 82,000 eggs, counting 20 eggs to a deposit on an average. Probably 70,000 young will be hatched from these, and, deducting 10 per cent loss before the third year, there will be about 60,000 "suppon" ready for the market in three years. The turtles sold in a year in Osaka, Tokyo, Nagoya, and a few other towns weigh about 2000 kwan (=16,500 pounds), and are worth about 6.50 to 7.50 yen (1 yen =$0.50) per kwan.

There are several minor turtle-farms besides those mentioned above, but as they are all modeled after those under Mr. Hattori's management, they need not be described further.

The Goldfish (*Carassius auratus*, Linnæus)

The goldfish is the characteristically Oriental domesticated fish. Its beautiful bright coloration and graceful form, with long, flowing fins, appeal most strongly to one's sense of the beautiful. It also is intensely interesting from the scientific standpoint, and proves a source of endless surprises to the biologist, for it is a plastic material with which skillful breeding can, within certain limits, do almost anything. Our goldfish-breeders seem to have understood the principle of "breeding to a point" to perfection, and I have often been interested in hearing some of them talk in a way which reminded me of passages in the *Origin of Species* or other Darwinian writings. This must be considered remarkable, for these breeders are, as a general thing, without much education, and have obtained all their knowledge from the practical handling of the fish.

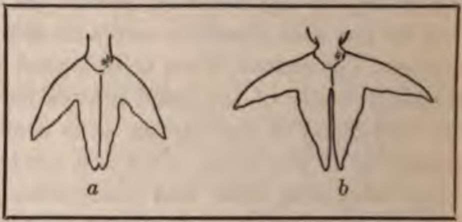

Fig. 3. — Diagram of the tail of a goldfish. *a*, Three-lobed; *b*, four-lobed.

The history of the goldfish is lost in obscurity. Like so many things in Japan, it seems to be an importation from China. There is a record that about four hundred years ago — that is, about the year 1500 — some goldfish were brought from China to Sakai, a town near Osaka. The breed then brought in is said to be that now known as the "wakin." There must also have been several later importations, and the Japanese must have improved vastly on the original forms, as in so many other cases of things introduced from foreign countries. Several varieties have thus resulted, but before proceeding to describe these I may say a few words about goldfish in general. A characteristic of the goldfish, no matter of what variety, is that the black pigment with which the body is uniformly colored when first hatched from the egg disappears in a year or so and gives place to bright colors, which are of various shades between carmine and vermilion red, and which may be either spread all over the body or variegated with white in various degrees. A fish that is entirely white fetches no price in the market, and is mercilessly eliminated in the first year. A fish with the white body variegated with red around the lips and on the opercula and all the fins is considered to have the best coloration. The dorsal fin is either single or absent. The tail may remain simple, as in ordinary fishes, but should best split open and spread out horizontally, when it is therefore three-lobed (Fig. 3, *a*), but quite as frequently it may be split in the median lines, when it is four-lobed (Fig. 3, *b*). The anal fin may also very often split open and become paired.[1]

There are five well-established varieties of the goldfish in Japan, and in addition one or two which have not become so common as yet. I will go over these varieties briefly:

(1) The "wakin" (literally Japanese goldfish). This has a shape nearest the normal form of a fish. The body is slender and long, closely resembling that of the common crucian carp. The tail may be single, vertical, and normal, but should, to be a good form, split open and become either three-lobed or four-lobed. This may, in short, be characterized as the bright-colored variety of the common *Carassius auratus*, with or without the modified tail.

(2) The "ryukin" (literally "Loochoo" goldfish), also called the "Nagasaki." The first name may possibly denote whence the variety came originally. The body is strikingly shortened, — this being one of the points to which the variety was bred, — and has a rounded, bulged-out abdomen. The tail and all the fins are long and flowing, the former being as long as or even longer than the body. This, in my opinion, is the most beautiful breed. A "ryukin" two or three years old, slowly swimming with its long, flowing, graceful fins and tail, full of quiet dignity, I can liken to nothing so much as to Japanese court ladies of olden times, dressed in long robes and walking with quiet grace and dignity.

(3) The "ranchu," also called "maruko" (literally, round fish), "shishigashira" (literally, lion-headed), and sometimes "Corean goldfish." This is distinguished by its rather broad head, its extremely short, almost globular body, the short tail, and the absence of the dorsal fin. Some individuals of this variety develop in the second year, or at the latest in the third year, a number of peculiar wart-like protuberances all over the head, making it look as if it had a low coxcomb or some skin disease. Such fish are called the "shishigashira," or "lion-headed." This variety is seen often swimming upside down, a fact with which the absence of the dorsal fin probably has something to do.

(4) The "oranda-shishigashira" (literally, Dutch lion-headed). The adjective Dutch is known to have nothing to do with the place of origin of the fish, but was attached to the name to denote something novel. This variety was produced in Osaka in the forties of the last century by crossing the "ryukin" with the "ranchu." There-

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[1] For further details see S. Watase: *On the Caudal and Anal Fins of Goldfishes, Journal Science College*, vol. I, p. 247, pl. XVIII–XX.

fore, it possesses a body more or less like that of the "ryukin" with the dorsal fin, but from the second year or thereabouts the head begins to develop the wart-like protuberances described under the "ranchu." When fully developed, this breed is, to my mind at least, anything but beautiful. It is cultivated near Kyoto or Osaka, while the "ryukin" is reared mostly in Tokyo.

The above four breeds are common, and can be seen in almost any goldfish-seller's. There are some other rarer or newer varieties:

(5) The "shukin." This is a breed only recently produced by my friend, Mr. Akiyama, a skillful goldfish-breeder of Tokyo, and also produced independently in Osaka. It was obtained by crossing the "oranda-shishigashira" with the "ranchu." It is "lion-headed," — that is, has warts on the head, — has the globular body of the "ranchu" without any dorsal fin, but it has a long, flowing tail. It may be characterized as a long-tailed variety of the "ranchu."

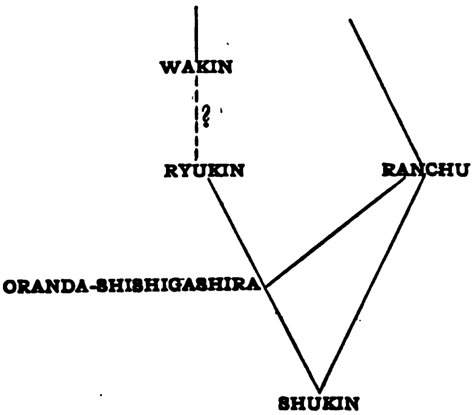
(6) The "demé" (literally "protruding eyes" or "telescope-fish"). Contrary to what is stated in many American and European books, the telescope-fish is only a recent introduction into Japan. In fact, it was brought to Japan at the end of the late Japan-China war (1894–95). As is well known, in this variety the large eyeballs have started out of the skull and protrude sideways from the head, which thus somewhat resembles (although only superficially) that of the hammer-headed shark. The body is short; the color is yellowish, or at least not usually bright red, and often has black spots or irregular black patches scattered over the body. It should be stated that the first-year young have the eyes in the normal position, the protrusion occurring gradually in the course of growth and not through any artificial devices. These fish, when fully grown, are apt to strike their eyes against the sides of the ponds, tubs, etc., in which they are kept, and to injure them so that they often become blind. In nature, therefore, such a protruding eye must be a distinct disadvantage, and would never have been produced except by artificial selection.

(7) The "demé-ranchu." This variety is not yet naturalized in Japan, having been imported from China only within the last two or three years. Of all the extraordinary and odd-looking fishes, it certainly is far in the lead in many respects, and is interesting as showing how far man can proceed in modifying nature. It is a telescope-fish with a short globular body resembling the "ranchu," and, like it, without the dorsal fin. The eyes have assumed a most extraordinary position. The ordinary telescope-fish is odd enough, with the eyes protruding, but in this variety dislocation has gone one step farther. The eyes have not only started out of the head, but have turned upward ninety degrees, and have their pupils looking straight skyward. For this reason I should be inclined to call this

the "astronomical telescope-fish." As a fish, it is so monstrous that it gives one almost uncomfortable feelings.

It is an interesting fact that in the forms without any dorsal fin, many young show more or less traces of that fin. Sometimes there may be only the first spine, at other times only a few spines, at still others a little bit of a fin, etc., showing that the fin must have been bred off comparatively recently.

There can be no doubt that of these varieties the "wakin" is the most primitive, as can be seen from its shape, as well as from the fact that it is much hardier than the others, and therefore easier to rear. The "ryukin" is next the "wakin" in its nearness to the original *Carassius*. It is still like an ordinary fish, although its shortened body and long, flowing fins show that changes have already gone very far. The "ranchu" seems farther removed from the original type, as its globular body and the absence of the dorsal fin well testify. The relations that these three varieties hold to one another are involved in obscurity. Some think that the "ryukin" is a cross between the "wakin" and the "ranchu," but I think that this can hardly be so. I am inclined to think that the "ryukin" must have been bred from ancestors somewhat like the "wakin" by careful selection, and that the "ranchu" is the offshoot of another branch which must have separated from the "wakin" stem very early. The cross between the "ryukin" and the "ranchu" is the "oranda-shishigashira," and this, crossed again with the "ranchu," is the "shukin." An interesting fact is that in the first cross both the dorsal and the tail fins are long, but in the second cross the dorsal fin is lost, while the tail is not only retained, but remains long. Expressed in a diagram, the supposed genealogy would be as follows:



The goldfish is very common in Japan, and more or less reared in all parts, but the main centres of cultivation are Tokyo, Osaka, and Koriyama (a small town near Nara, where almost every household engages in this business). Each of these places has its own peculiarities in the method of raising, but the differences are, on the whole, in minor details only. In Tokyo goldfish-breeders are all located in low-lying parts of the city, where ponds, a *sine qua non* of this business, can be easily made.

One establishment is very much like another, the principal differences being in the number and size of ponds. There is always a number of shallow ponds, sometimes to the number of ten or more. Shallow dishes, slung by three strings from bamboo poles stuck in the muddy bottom of the pond, are the dishes in which food is given to the goldfish. Besides these shallow ponds there is always a large number of shallow cement basins of various sizes, some as small as three feet by three, others as large as twelve feet by twelve, with intermediate sizes of all sorts. They are very shallow, being not more than a few inches deep, can be easily drained or filled, and can be shaded or exposed to the sun at will. A visit to such an establishment would delight the hearts of not only children, but grown-up persons who love bright colors and graceful forms, for the ponds are full of brilliantly colored fish of all ages and sizes. Here are huge fourth-year "wakin," there graceful second-year "ryukin," off there fine "ranchu." Ornamental little carps, little tortoises, and tiny fish called "medaka" (*Aplocheilus latipes*) are also generally found in the goldfish-breeders' establishments.

The process of rearing goldfish is in its main outline as follows: Large goldfish that are three or four years old, with good forms and healthy in every respect, are carefully selected for the purpose of breeding. This takes place any time between the last part of March and the middle of June, the usual time being in April and May. At this season the color of the fish becomes more brilliant than ever, and small, low warts that can barely be felt with one's finger are said to be produced on the opercula of the male. Both sexes crowd together, causing great commotion in ponds in which they are kept. Plenty of a water-weed ("kingyomo," or "matsumo," *Ceratophyllum demersum* Linnæus), or bundles of fine roots of the willow-tree, are placed in the pond, and on them the goldfish lay their eggs. It is an interesting fact that goldfish-breeders are able to control, within a certain limit, the time of deposition of eggs. If the fish are given plenty of food beforehand, and then the water of the pond in which they are kept is renewed, or if they are placed in another pond, they will deposit eggs in a day or two. On the contrary, if they are underfed and kept in the same stagnant water, they will desist from depositing eggs, sometimes altogether.

The eggs take eight to nine days to hatch. The young for the first few days are given the yolk of hen's eggs, boiled. Food is usually given them on shallow earthenware plates, slung by three strings from a bamboo pole for the youngest, these plates being kept at the depth of a little over one inch below the surface of the water. For the next two or three weeks the young are given various kinds of fresh-water Copepoda. These the goldfish-breeders prepare beforehand in a separate pond, for they have the knack of producing these water-fleas in any quantity they need at any time they like. After Copepoda succeeds the ordinary food of the goldfish, such as fresh-water earthworms, boiled cracked wheat, etc. It is essential for the growth and health of the fish that they be kept as warm as possible; hence, the shallow earthenware dishes from which they are fed are kept at first — that is, when the fish are first hatched, and, therefore, in the hot season — only a little over an inch below the surface of the water. With the growth of the young and the approach of the colder weather they are gradually put down lower and lower, until in the winter they are down nearly ten inches, such a depth being naturally warmer than nearer the very surface of the water.

Among the young fish all sorts and conditions of the body and the fins are found, — that is, all forms intermediate between those closely resembling the normal crucian carp with a long, slender body, the unsplit tail and anal fins, etc., and those which are extremely modified, as shown in the varietal types described above. If a lot of young contains a large percentage of those with the unsplit tail, it is considered, from the commercial standpoint, a failure, for these latter are only a fraction of the split-tailed in price. In some experiments I have tried, it was found that, in selecting for breeding, the adults which have the split anal fin give, on the whole, better results than those with a single anal. It is needless to say that all undesirable ones are early eliminated.

All the young just hatched are dark in color, the bright colors coming only later. A great deal of experience and skill is needed in making the goldfish change its color from black to red. If a person who is not an expert tries his hand at raising a lot of young goldfish, he will find to his sorrow that the fish remain black and do not assume bright colors, while those which may be from the very same lot of eggs, but have been under the care of a professional breeder, may have all donned the beautiful hues. The essential points to be attended to in bringing about this change seem to be (1) that the young fish should be given plenty of food, (2) that they should be exposed to the sun's rays and be kept as warm as possible, and (3) that the water of the pond in which the young are kept should be changed occasionally, although sudden transfer from warm to cold water in the middle of the day is to be avoided. The change of color begins in about sixty to eighty days from the time of hatching, and by the middle of August the fish should all have lost the dark pigment, and acquired bright colors. I am told a curious fact, — that the fish which change their color earliest are apt to be white, or variegated white and red, while those that change later are apt to be uniformly red. What can be the significance of such a fact? I am also told that by the middle of August of the second year, all the individuals, however obstinate, change their color. It is worth while determining whether, even if the young are left to themselves and not given the care which they receive at a breeder's, they will change color by the summer of the second year.

White is commercially worthless, and is ruthlessly weeded out. It is also said that, to improve the brightness of the color, the fish should be somewhat underfed, — that is, should be given about 80 per cent of the ordinary feed. In Koriyama they have the trick of bleaching out white spots in the red, by applying some mixture. The result, I think, is not worth much.

I have by no means exhausted the subject of the goldfish; in fact, I doubt whether any one can write all the minute details of the art of goldfish-raising. But I think I have said enough to show how full of interest goldfish-breeding is, not only from the commercial or æsthetic point of view, but from the purely scientific standpoint. A most casual glance shows it to be full of problems which have ever attracted the serious attention of biological investigators.

I have just now no available statistics in regard to the output of goldfish, but the number produced must be millions upon millions. It shows the power of children in the nation, for they are *par excellence* the customers of these establishments. It is said that in the old régime, even in years when a famine was stalking in the land and hundreds were dying from starvation, there was a tolerable trade in goldfish, proving the truth of an old proverb: "Crying children and landlords must not be disputed." Landlords are not now tyrannical as of yore, but children have not abated their power in the slightest degree, and that they do not get the moon seems simply to be due to the fact that it involves an impossible feat for their parents.

The Carp (*Cyprinus carpio* Linnæus)

Closely connected in some respects with the culture of the snapping-turtle and of the goldfish is that of the carp. As stated before, the carp is put in the same pond with *Trionyx*; and the raising of the ornamental varieties is generally undertaken by goldfish-breeders. There are several breeds, among which the red carp ("higoi"), the "hokin" (literally "gold-cheeked," with the operculum of the gold or silver color), and the "goshiki-goi" (literally "five-colored," or variegated) are the most common. Travelers in Japan must have noticed in ponds belonging to various temple-grounds these ornamental carps which often reach the enormous size of two feet or more, and which children delight in feeding.

The ordinary carp itself has been very extensively cultivated from olden times in Japan in ponds, reservoirs, and various other bodies of water, and the business has been considered profitable, as the fish commands a comparatively high price.

Around or near Tokyo, especially in the district called Fukagawa, there have sprung up within the last twenty years a number of carp-culture establishments. They carry out at the same time and in the same ponds the culture of the eel and of the gray mullet ("ina," or "bora," *Mugil oeur* Forskål), the three fishes going well together and being consumed to a great extent in the city of Tokyo. It is estimated that there are in this small district alone 225 acres devoted to carp-culture, producing annually 405,000 pounds of the meat of this fish, valued at 30,000 yen at the wholesale price, and furnishing a large part of the supply for Tokyo and its neighborhood. I ought to add that Mr. Hattori, who is the proprietor of the turtle-farm, was largely instrumental in developing the industry in this region.

Some of these establishments are very interesting. A very large establishment has an area of 75 acres, and a large number of ponds, the largest of which are about five acres in extent.

The carp is reared from the egg in these establishments. In May of every year large adult individuals are carefully selected for breeding, and, as in the case of goldfish, eggs are made to be deposited on the water-weed ("matsumo") or bundles of fine willow-roots, where they hatch in about a week. The young are some five millimeters in length, and undergo the same course of feeding as the young goldfish. The rate of growth depends very much upon the extent of the crowding in the ponds. It is found that for individuals 14 to 16 centimeters long the best rate of distribution is about two for every "tsubo" (six feet square). Skillful culturists can push the fish, if necessary, to the length of 30 centimeters in the first year, and to 50 centimeters in two years. They are put on the market any time after the second year.

Carp-culture is carried out extensively in parts of Japan other than Tokyo, especially in mountainous parts where sea-fishes can be transported only with difficulty, and the industry is spreading more and more every year into all parts. One interesting reason for this is found in the circumstance that wet paddy-fields in which rice is grown, and which occupy such a large portion of the cultivated area in Japan, are found in many low-lying districts to be excellent for the raising of the carp. The rice-plant not only does not receive any serious injury from it, but is benefited, because many insects are devoured by the carp. In the prefectures of Nagano (province Shinano) and of Gifu

(Province Mino), carp-culture has progressed very far in this way. In Nagano, which is also noted for silkworm-raising, abundant food for the carp is found in the pupæ of the silkworm, taken out of the cocoons when these are reeled. This gives a bad flavor to the meat of the carp, however, which has therefore to undergo the process of purifying culture before it suits the taste of the fastidious. In one village in Shinano (Sakurai Mura) the agricultural society, which represents the whole village, undertakes to utilize 250 acres of paddy-fields in the village in this way, and annually raises 25,000,000 young fish to be sold and raised in the eastern provinces. In Mino, in the prefecture of Gifu, these communistic enterprises have gone farther. There land is partitioned off into what are called "embankment areas," — that is, areas inclosed within a circle of embankments against the overflowing of large rivers. In one of these areas, called the Takasu embankment area, all the villages within it, with a total of 75,000 acres of paddy-fields, have combined in the business of carp-culture, and although the enterprise is still in its infancy, succeeded in realizing 48,000 yen in 1902. The example is being followed in other areas.

The Eel (*Anguilla japonica* Temminck and Schlegel)

As has already been mentioned, in the piscicultural establishments in Fukagawa, Tokyo, and in the neighborhood of Maisaka, province Totomi, the snapping-turtle, the carp, the eel, and the gray mullet ("ina"), especially the last three, are often cultivated together in the same ponds. That the eel finds itself one of this trio is due largely to the efforts of Mr. Hattori, the expert pisciculturist. He experimented long as to the best way to make eel-culture a paying business, and succeeded so well that this is now the most profitable of the three fishes named.

The process is as follows: In April little eels that are brought to the Tokyo market from all the districts around the capital (Tokyo, Ibaraki, Chiba, Kanagawa, etc.) are bought. They are probably in the second year of their growth and are about 15 to 25 centimeters in length and weigh 3 to 20 grams. They are put in the same ponds with the carp and the gray mullet in varying ratios, although the total weight of the fishes put in should not exceed 610 grams per 1 tsubo (6 feet square). They are fed abundantly with the same kinds of food as the carp — that is, crushed mollusks, earthworms, etc. It is a wonderful sight when they are fed. They come crowding from all parts of the pond to the spot where food is given them, and literally thousands are seen crowded in hopeless tangles. They climb in their eagerness some distance up almost vertical wooden walls, and, looking at them, one begins to understand how eels are able to make their way into ponds and lakes which appear inaccessible to any fish coming up from the sea.

By July they weigh on an average 40 grams and are ready to be sent to the market. When they were put in, in April, they were worth 0.80 yen per kwamme (3.75 kilograms). Three months' culture has raised their value to 1.50 to 2 yen per kwamme, giving thus a large margin of profit. They are all sold by April of the next year, when the largest reach the weight of about 110 grams. The ponds are then ready to receive the next lot.

Eel-culture, as I have said, has been mainly developed by the efforts of Mr. Hattori, and all the piscicultural establishments which are more or less directly connected with him are engaged in the business. These are in Fukagawa, Tokyo, and in Maisaka, Province Totomi, where the industry is being very widely taken up. I believe that there are also some who were engaged in the business before and without any relation to Mr. Hattori, but I am sorry I cannot gather any facts about these at present.

The Gray Mullet, "Ina" (*Mugil oeur* Forskål)

This is one of the commonest fishes in the estuaries, river-mouths, etc., of Japan. In large numbers it penetrates brackish ponds or any other brackish body of water, where it may grow to a large size and may be gathered in by the proprietor without his having spent any labor on it. Mr. Hattori tells me that from the culturist's point of view fear is not that there may be too few, but that there may be too many, of this fish that will get into culture-ponds. The young are caught in April with a net in the sea or river near the establishments. At that time they are no more than 4 to 5 centimeters long. They are divided into two lots, according as they are to be sold that year or the next. Those that are to be sold that year are given plenty of space, not more than one or two per tsubo being put in ponds, and are fed abundantly. By September they attain the length of about 25 centimeters and weigh 225 to 860 grams, and are sold for 0.50 to 0.80 yen per kwamme. They are all sold by the end of the year.

Those that are to be sold the next year are not allowed to grow larger than 20 to 25 centimeters before April. This is accomplished by giving them not too much food and by keeping them in ponds or streams where there is a good circulation and a current of water. It is found that those with plenty of fat will not live through the winter. They are all sold off by the end of the second year, for beyond this they do not keep well. They reach the length of 33 to 40 centimeters and 450 to 750 grams in weight, and fetch 0.70 to 1.10 yen per kwamme.

I should say that practically there is no limit to the demand in the Tokyo market for this fish or the eel. They can be sold in any quantity. The same is true more or less in other parts of Japan.

Salmon and Trout, "Sake," "Masu," "Benimasu." Oncorhynchus keta (Walbaum); *O. kisutch* (Walbaum); *O. nerka* (Walbaum).

The salmon that is most widely distributed and most abundant in Japan is the "sake," or dog salmon (*Oncorhynchus keta*). It ascends all the rivers of Hokkaido and the northern half of Honshu down to near the Bay of Tokyo, and is one of the most important wealth-producing fishes in Hokkaido. In olden times, when the annual catch was not so great as at the present day, there does not seem to have been any necessity for artificial culture. Still there were some attempts at the propagation of the fish. For instance, on the Sammen River, in the Province of Echigo, salmon-fishing was prohibited in a branch of the river, and the salmon which entered it were caught only after they had deposited eggs, and by the daimio to whom the district belonged, thus securing an income for him and some safety for the salmon-eggs. It was a very imperfect method, but still an attempt at propagation, and is even at the present day practiced at the same place.

The modern method of salmon-culture is taken bodily from the American method, so I can communicate nothing that is new in America. As early as 1876 a Mr. Sekizawa, then an officer of the Home Department, inspected and carefully examined salmon- and trout-culture in America, and on his return started experimenting on them, which was largely imitated in the hope that these delicious fishes might be easily increased and propagated. But these undertakings were mostly on too small a scale and no important results came of them, except that Chuzenji Lake at Nikko was stocked with some American trout about this time and has since become tolerably full of fish.

Meanwhile the salmon fishery in Hokkaido was going on upon a destructive scale, and matters came to such a pass in the eighties of the last century that a need of artificial propagation was strongly felt, and an expert of the Hokkaido Government, Mr. K. Ito, was sent over to America to examine into the system of salmon-culture there carried on. On his return Mr. Ito established, in 1888, a hatchery at Chitose, on one of the upper branches of the Ishikari River. It was modeled after the hatchery at Craig Brook, Maine. By the efforts of Mr. Ito and his successors and by the able superintendence of Mr. Fujimura, the hatchery, which has been enlarged several times, has now become the centre of salmon-culture. It comprises an area of over 30 acres, and hatches annually 8,000,000 to 14,000,000 "sake" eggs, besides a much smaller number of trout ("masu") eggs. All the hatched fry are liberated in the Ishikari River system.

Besides the central hatchery at Chitose, there are seventeen smaller hatcheries scattered all over Hokkaido, maintained by private fisheries associations with some Government aid. All of these hatch between 1,000,000 and 5,000,000 eggs, while the largest of them, at Nishibetsu, may go up as high as 8,000,000. We may therefore assume that something like 35,000,000 to 50,000,000 eggs — being 37,000,000 in 1903 — are annually liberated in Hokkaido.

Besides those in Hokkaido there are some five hatcheries on the main island — Honshu — supported by the five northern prefectures (Nigata, Akita, Miyagi, Awomori, and Ibareki). All of these establishments, however, are small, the largest (Niigata) hatching only a little over 2,000,000 eggs.

At Chitose and Nishibetsu, in Hokkaido, a small number of the "masu" (*O. kisutch*) are hatched, and on Lake Shikotsu, near the Chitose hatchery, there is a small branch hatchery. Here the eggs of the land-locked "beni-masu" (the Ainu "kabacheppo" — land-locked *O. nerka?*) are hatched. This fish was originally found in Lake Akanka, in the eastern part of the Hokkaido; from there transplanted to Lake Shikotsu, mentioned above; from there again to Lake Onuma near Hakodate, and still farther to Lake Towada, in the Akita Prefecture on the main island.

There is one interesting fact which is perhaps worth mentioning. Of the salmon-fry that were liberated in the spring of 1896, 30,000 were marked by cutting off the operculum. Of these some are said to have come back in the winter of 1901–02, and two grown to the size of 2.3 and 2.4 feet are specially mentioned. In the winter of 1902–03 some twenty (according to Mr. Fujimura) were heard from, and five specially recorded. In the winter of 1903–04 some forty (according to the same authority) were heard from, and several were no doubt specially examined, but the records are not just now available. Thus the salmon liberated in one single year are returning during several years in succession, the earliest recorded being five years and a half after being set free. In the years 1897–1901 a certain number of the young fry were marked by cutting the adipose fin, and these are already being reported. All the certain recorded cases have come back to the same Ishikari River system.

I need hardly say that salmon- and trout-culture is still in its infancy in Japan. The dog salmon is considered by the Americans as not delicate in flavor, and we should not confine ourselves to its cultivation, but should make efforts to introduce the finer salmon and trout of America. At the same time we should undertake the culture of other members of the Salmonidæ native in Japan, such as the "shirauwo" (*Salanx microdon*), the "ayu" (*Plecoglossus altivelis*) etc.

Pisciculture in Formosa

In Formosa, recently acquired by Japan, the native Chinese engage in the culture of various species of fishes, such as the carp, the gray mullet, the crucian carp, etc. Of these, two stand out prominent. One species belonging to the Clupeidæ and called in Chinese "sabahi" (*Chanos salmoneus* Bloch and Schneider) is abundantly cultivated in the southern parts. Although a sea-fish, it is able to accommodate itself easily to fresh water. The fish are at first put, when small fry, into ponds not more than four feet square, and are fed with hen's eggs. When grown to a larger size, in twenty to thirty days, they are put into larger ponds, given plenty of food, and when they reach the size of ten inches or more are put on the market. The other fish much cultivated is called "lenhi" (*Hypophthalmichthys molitrix* Cuvier and Valenciennes), belonging to the Cyprinidæ. These are brought from China in November and December, when nine to ten inches long. They are kept in ponds and abundantly fed, and may reach the size of three and one half feet, but are sold from the time they become one foot long. This fish is cultivated in all parts of Formosa.

The Oyster (*Ostrea cucullata* Born)

The oyster has probably been longer under cultivation by man than has any other mollusk, and it is also the most extensively cultivated. As to the former point, I need only refer to Roman pictures delineating oyster-rearing, and as to the latter, to the extensive enterprises carried on at the present day in Europe and America. In Japan, also, the luscious mollusk received an early attention, and its culture is becoming more and more extensive. The first place where this was done systematically appears to have been the neighborhood of Hiroshima, a town about in the middle of the length of the Inland Sea and on the north side of that waterway. There is a record preserved there showing that the art of oyster-raising was well understood certainly one hundred and eighty years ago, and the practice is, no doubt, much older. There were several reasons why it should prosper here, among which may be mentioned (1) that the sea about there is as quiet as a lake; (2) that the differences of level between the high- and low-water marks are comparatively great, being ten to fifteen feet, thus exposing a very wide area adapted for oyster cultivation; (3) the bottom of the sea is rather firm there, being composed of finely ground granite; (4) lots were early divided and leased to individuals, thus securing the utmost exertions of those lessees; (5) monopoly was acquired by the people of this region in selling oysters in Osaka, thus insuring a large market.

I made in 1894 a careful inspection of the oyster industry of Hiroshima at the request of the Department of Agriculture of the Japanese Government, and wrote a report on it (in Japanese). This has been, in its main outline, together with some valuable additions of his own, put into English by Professor Bashford Dean, of New York (*U. S. Fish Commission Bulletin for* 1902, pp. 17–37, pls. 3–7), and the reader may be referred to it for details. I shall, however, touch here, though briefly, on various systems carried out around Hiroshima, for they are, after all, the most complete of any known in Japan.

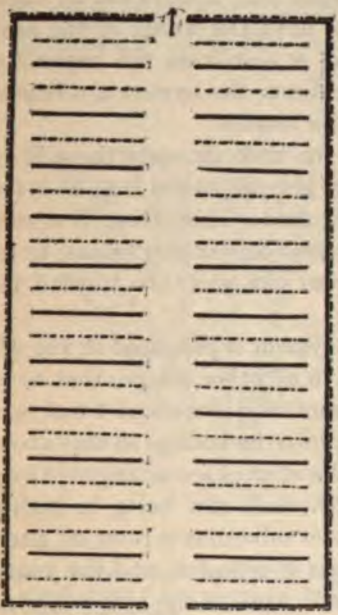


FIG. 5. — Typical oyster-farm, Kaida Bay.

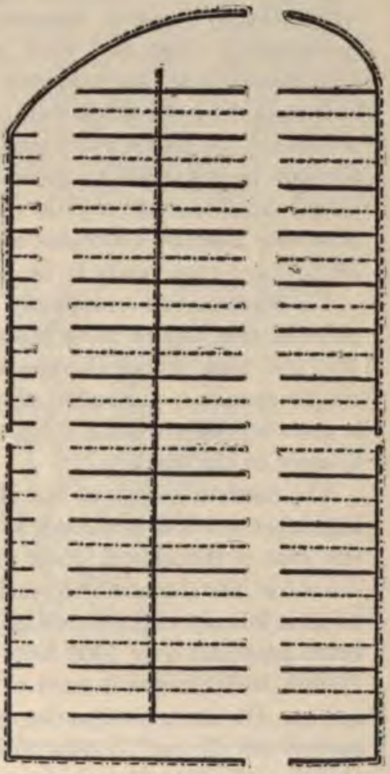


FIG. 6. — Diagram of well-developed oyster-farm.

The black lines in Figs. 5 and 6 represent newly arranged bamboo collectors, the dotted lines the collectors of the second year.

The simplest method among them is practiced in a village called Kaidaichi, a few miles east of the city of Hiroshima. When the tide is in, this bay is a quiet, placid piece of water; one sees nothing unusual unless he looks deep below the surface and notices long lines of bamboo fences. When the tide is out, the scene takes on an entirely different aspect. One sees that the entire area, only so recently covered by the water and over which one glided in a boat, seems to be cut up into lots looking very much like town lots, with streets intersecting. Two examples of these lots are given in Figs. 5 and 6. The lines in the figures indicate bamboo collectors on which the oyster-spat becomes attached and grows, the full lines representing those that were put up any one year, and the dotted lines those of the year previous. From a distance these bamboo collectors and oyster-fields reminded me of nothing so much as vine-trellises in the Rhine vineyards. The spat that is collected on these bamboo fences is left to grow on them until the winter of the next year — that is, only a little more than a year from the beginning. Then the bamboo collectors are taken down, the oysters are beaten off, and are then ready to be sent to the market.

The oysters are necessarily small, for unfortunately there is no place in this bay to allow their further growth, as the bottom is too soft and they would become buried in mud. This, then, is a very simple system, — to collect the oyster-spat on bamboo fences, to let it grow on them until a little over a year old, and then to send the oysters to the market.

The method known as the Kusatsu system is practiced in the village after which it is named, as well as in all other villages that lie to the west of Hiroshima. Four or five bamboo sticks about 4 feet long are made into clusters and stuck firmly into the bottom so that about 3 feet is left above ground (Fig. 7). These clumps are arranged in long rows, generally over 1000 feet in length, each row being in reality double, with clumps in each of these two subordinate rows set alternately. On these clumps the oyster-spat is collected, and the young oysters are allowed to grow on them until April of the next year. At that time the old collectors have to give place to the new set of collectors to be ready for the spat that will soon be shed. Young oysters are therefore struck off the collectors at that time and taken to the place called "ike-ba" (literally living-ground), where they are placed directly on the rather firm, gravelly sea-bottom, and allowed to grow there until the cold season of the third year. These "ike-ba" may be some distance from, or quite near, the spat-collecting ground, according to the circumstances of each collector and how and where he can get a good bottom for the purpose. Finally, toward the cold season of the third year, the oysters are removed to the "miire-ba," or maturing-ground, which is to receive all that are ready for the market. This ground must, of course, be quite near the culturist, and easily accessible.

At Nihojima, about 2 miles east of Hiroshima, the nature of the oyster-grounds has necessitated the development of a most elaborate system of oyster-culture. Here the main part of the grounds is in a sheltered inlet, or rather in an enlarged mouth of a river, which naturally brings down a great deal of fresh water. As I think, for this very reason the spat-collecting is done just outside the inlet. Here, in April, when the breeding-season begins, bamboo collectors, four or five in a bundle, are planted in close clusters along the channel to receive the spat. At the end of the breeding-season, —that is, in the latter part of August — the collectors are uprooted and conveyed inside the inlet, care being taken not to injure the spat upon them. There they are built into peculiar structures called "toya," which are round-pryamidal in shape, and measure about three to four feet high and five to six feet across at the bottom. A "toya" is constructed (Fig. 8) as follows: In the centre are small bamboo collectors of last year on which some young oysters are still adherent. Outside of these the new bamboo collectors, which have just been brought in from the spat-collecting ground with tiny oysters adherent on them, are placed in two circles, one outside the other, the bamboo branches being made to interlock. The "toyas" are left in this condition exactly one year, when they must give place to the next new set.

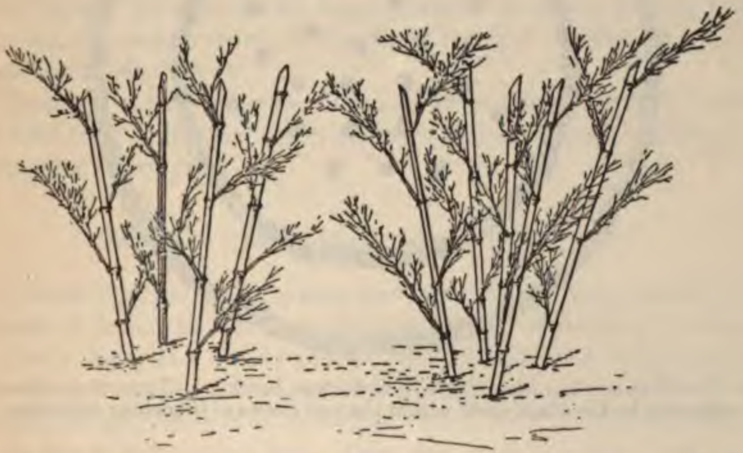


FIG. 7. — Bamboo collectors arranged after the fashion common in Kusatsu. They stand about 3 feet above the bottom and their tips diverge; the clumps are set 4 or 5 feet apart.

The oysters that are now in their second year and are of a fair size are struck off the bamboo collectors, which are rotten by this time, and are then placed in the living-ground, where they lie directly on the hard and gravelly bottom. They are left here until the next year, although they are given a thorough raking every fortnight or so. By autumn of the third year they are ready for the market. The sea-bottom in the inlet of Nihojima has been completely utilized for this purpose and has been cut up into lots and leased to different persons. Put this together with the fact that hills around here are cultivated to the very top, and it would be difficult to go beyond this in the utilization of land and water. Hiroshima has perhaps gone ahead of most places in Japan in this respect.

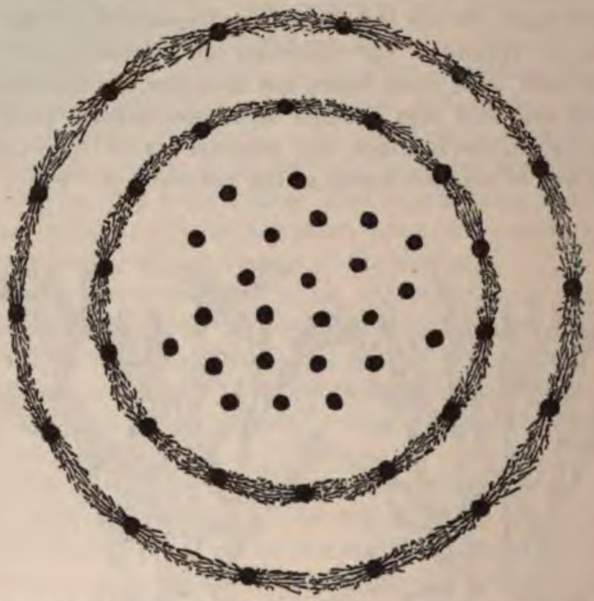


FIG. 8.—Ground-plan of a "toya." Collectors bearing well-grown oysters are indicated by the black spots within the two circles of branching collectors.

A rather interesting and simple system of oyster-culture has been developed within the last twenty years at the mouth of the Suminouye River, in Ariake Bay, in the prefecture of Saga, Kiushiu. It seems that people here were in the habit of collecting all the natural oysters they could and of preserving larger ones among them for a little while on the bottom of the Suminouye River to be sent later to Nagasaki for sale. For some reason, in 1884 those thus preserved were left through the winter and it was discovered that by next year they had grown to a large size. This fact was not lost on the sagacious people thereabouts, of whom Mr. Murata, an enthusiastic culturist, seems to have been the head and soul. From this beginning the industry was developed so that 18,330 bushels of oysters, valued at 21,181 yen, were produced in 1897, and the output has no doubt increased since. The method is as follows: Young oysters about an inch or more in length are collected constantly from July till March of the next year from stone walls, old shells, etc. All these are placed on oyster-beds in the river-mouth, and, as these small ones may be choked by being covered up with the silt, they are heaped close together in masses, and are, moreover, washed and cleaned two or three times in a month, at low tide. In April these oysters are stuck into the mud almost vertically, with the hinge-end below and with the ventral margin above. As the mud is firm, they seem not only to keep this position, but also to grow finely. They are often cleaned, and as they grow they are often thinned out and given more space. In August and September they grow most rapidly. By October they are six by five inches in size and ready for the market. I think the rapid growth, the round shape, and the large size must distinguish this from the ordinary Japanese species of oyster. This system seems very profitable, as one tsubo (six feet square) is said to give a return of three yen. In Formosa there is also a system of oyster-culture practiced by the Chinese. In oyster-farms near Tamsui, on that island, large blocks of stone are arranged one foot apart in regular rows, and on these the spat is collected and the oysters are left to grow.

There are various other methods and variations of methods carried on with more or less success in different parts of Japan, and they are increasing every year.

The Pearl Oyster (*Avicula martensii* Dunker)

Various kinds of pearl oysters are found in southern semitropical islands of Japan, but the only one which is at all common in Japan proper is the species named above. This pearl oyster is found more or less along the whole of the coast of Japan, but there are some localities famous for producing it in quantities. Such are Shima, Omura (Province Hizen in Kiushiu), Noto, Tosa, etc., and some fine pearls have been obtained from these places. As in so many other matters in Japan, there was a time after the restoration of 1868 when the fishery for these precious shells was thrown into a chaotic state, and, as is usual in such a case, carried to an excess, so that the yield of pearls dwindled to almost nothing.

In 1890 I suggested to a Mr. Mikimoto, a native of Shima, who had grown up and lived in the midst of the pearl-producing district, the desirability of cultivating the pearl oyster. He took up the subject eagerly, and began making experiments on it. Soon after I pointed out to him also the possibility of making the pearl oyster produce pearls by giving artificial stimuli. He at once proceeded to experiment on it. The results have been beyond expectations, and to-day the Mikimoto pearl-oyster farm, put on a commercial basis, has millions of pearl oysters living on its culture-grounds, and is able to place annually a large crop of pearls on the market.

The Mikimoto pearl-oyster farm is in the Bay of Ago, on the Pacific side of central Japan, a few miles south of the famous Temple of Ise.

The bay, like all in which the pearl oyster grows in abundance, is a very quiet piece of water with a most irregular, highly broken-up coast-line full of deep-running inlets, coves, etc., with a depth of three to seven fathoms, and affording most favorable shelter. Somewhat out of the centre of the bay to the north there is a little island called Tadoko, where the land part of the enterprise, necessary buildings, etc., are placed, and where altogether about 100 persons connected in some way with pearl-oyster culture are now living. Around and in the neighborhood of this island a large area of sea-bottom, which with several large recent additions now amounts to 1000 acres, has been leased by Mr. Mikimoto.

The farm is divided into two portions: (1) Those parts where the spat is collected and the young are kept to their third year, and (2) the parts where the shells older than three years are kept. The breeding-season of the pearl oyster is July to August, and before this comes round — namely, in May to June — stones six to eight pounds in weight are placed over the bottom of the spat-collecting grounds, which are generally in shallower parts, penetrating deep into land. By August tiny shells not more than three to four millimeters long are first discovered, attached to these stones by their byssus, and the number increases steadily with the season. An immense number of shells is collected every year. They are allowed to lie as they are until November, and then those that are too near the shore are removed, with the stones on which they are anchored, into depths greater than five or six feet. This is necessary to protect them from cold, from the effects of which they are apt to die in the course of winter if left in the original places. The young shells are then left quietly and allowed to grow for three years, or, better, some may be removed to deeper waters, and where they are given more space, and get more food, and grow better. At the end of three years, when they are about five to six centimeters. across, they are taken out of the water and the operations necessary for inducing them to produce pearls — that is, of putting in nuclei for pearls — are performed on them. At present the number thus operated on in a year is only 250,000 to 300,000. They are then put back in the sea and spread out at the rate of about thirty to every tsubo (six feet square), and are left alone for four years more. At the end of that time, or seven years and a half from the beginning, they are taken out of the water and opened. Natural pearls, as well as "culture pearls," as I have named those produced from the introduced nuclei, are thus harvested and put on the market.

As in all culture enterprises, there are many enemies of the pearl oyster, as well as unexpected difficulties in the way of its culture. *Octopus*, *Codium*, *Clione* (sponges), all sometimes play sad havoc among the mollusks, but the most dreaded enemy of all is the "red current" or "red tide." This is an immense accumulation of a Dinoflagellata, *Gonyaulax*, causing discoloration of the sea-water, and, in some way not well accounted for, causing in its wake an immense destruction of marine organisms, large and small.

The "culture pearls" are, I regret to say, either half pearls or only a little more than half pearls, but as regards luster, shape, and size ,they are beautiful beyond expectations, and meet the requirements completely in cases where only half pearls are needed.

Pearl-oyster culture is still in its infancy, but its promises are bright. If, in addition to half pearls, full or "free" pearls can be produced at will, as there are some hopes, it will be a great triumph for applied zoölogy.

The Ark-Shell, "Haigai" (*Arca granosa* Lischke)

One of the most interesting cultural enterprises in Japan is that with the ark-shell (*Arca granosa*), or "haigai," as we call it. This was originally, and is at the present day, most extensively carried on at Kojima Bay, near Okayama. This bay opens into the Inland Sea by a narrow mouth, hardly a mile across, and is about eight miles in length by six miles of breadth. The differences between high and low tide-marks are comparatively great here, as in all parts of the Inland Sea, being five to seven feet, and at low tide the whole of the bottom of the bay is exposed, leaving only four river channels which run through the bay to its mouth. This flat is the area utilized for the cultivation of *Arca granosa.* It seems that this idea was present in the minds of some of the people as far back as the sixties in the last century, and was actually put in practice by 1869. At the beginning different individuals undertook the cultivation by themselves, and the conflict of interests soon became the source of endless disputes. People soon getting tired of this, it was agreed in 1886 to form an association in which all the conflicting interests were amalgamated, and, as this worked very smoothly, it was organized in 1890 into a stock company. At present a little over 830 acres of the bottom is utilized, the cultivated areas being scattered mostly along the southern and western sides of the bay. The annual sale amounts to 75,000 to 100,000 bushels, valued at more than 30,000 yen, and yielding a return of 40 to 60 per cent on the capital invested.

The method of culture is as follows: By September or October of every year, the larvæ of the mollusk, quitting their swimming stage, have become tiny shells not more than two or three millimeters long, buried directly below the surface of the bottom mud. These are collected from various parts of the bay by an ingenious instrument, which may be described as a huge comb more than six feet long, being a series of short pieces of wire with their points slightly bent, and planted with the other end on a piece of board. This, being applied on another piece of plank, is forcibly pushed along the mud bottom with the tooth part down, and all the tiny shells in the mud are caught between the teeth of the comb and accumulated on the bent ends of the wire. These are collected once in a while and put into a tub, after which another raking is gone through. The distance between the wires regulates the size of the shells to be caught. If the interval is large, the shells caught are naturally large, and *vice versa*.

These tiny shells collected from various parts of the bay are placed in the culture-grounds. It has been found that the best size for starting culture is quite small — that is, one which will go into the number of 30,000 to 70,000 per "sho" (1.58 quarts or 1.8 liters). In order to distribute them over the ground allotted to them, the little shells which have been collected are heaped up in a boat. One man rows the boat along slowly, and two others measure out the shells and throw them overboard with wooden scoops. The quantity of shells that can be most profitably put into a unit-area differs, of course, with the size and age of the shells, and has been very carefully studied out.

The tiny shells that in September are only two to three millimeters across, and run 30,000 to 70,000 to a "sho," grow by the autumn of the next — that is, the second — year to nearly twenty millimeters in length, and run only 1000 to a "sho." In the autumn of the third year their average length is already thirty-two millimeters, and they run only 200 to a "sho," and by the autumn of the fourth year they become forty-two millimeters long, or only 120 to a "sho."

As the shells grow, their number per unit-area must be diminished to the proper number determined by previous experience, and all the superfluous ones must be removed to near lots. These culture-grounds show, therefore, a large number of partitioned or marked areas, each of which contains a special lot as regards size and age, and give one an idea of the most methodical procedure.

It has been found that the crop of tiny shells which can be collected each season differs greatly in amount with different years. For instance, in 1893 the crop was very large, amounting to 14,145 bushels, but in the following year there were only fifteen bushels, and in the two years after that matters were still worse, there being practically none at all. In order, therefore, to have the market supply constant, and not fluctuating as these "seed"-shells, it has been found possible to retard the growth of the shells. That is, after they reach a size of 2000 to a "sho," they are removed to a somewhat deeper place, where the current is slow and where they are, no doubt, also kept more crowded than usual. This has been found enough to make their growth slower, and the seed-shells collected in one year can thus be depended on to supply the market for five years.

The three-year-old shells are exported in the fresh condition to China, where they are very much valued, while the four-year-old and the older are consumed in Japan.

Another species of *Arca* (*A. subcrenata* Lischke) is cultivated more or less in the same Kojima Bay, but this shell flourishes best in deeper waters which are not exposed at low tide and where seaweeds are growing. Such a condition is found in Nakano-Umi near Matsui, Province Isumo, on the Japan Sea side, where the ark-shell has now been cultivated for over a hundred years. The system of culture is that of rotary crops, giving fine results. The area under cultivation is at the present day about 2631 acres.

The Razor Clam, "Agemaki" (*Solecurtus constricta* Lamarck)

Reference has been made to a peculiar system of oyster-culture begun lately in the mouth of the Suminouye River in Ariake Bay. The shores of the same bay have extensive mud-flats exposed more or less at low tide, and here the cultivation of two other animals has gradually been developed, "agemaki" (*Solecurtus constricta*), a shell somewhat resembling razor shells, and barnacles (*Balanus* sp.).

The first of these is dried and exported to China. The trade began in 1875, and increased so rapidly that by 1882–83 the supply was not equal to the demand, and, owing to the consequent overfishing, the shells caught were becoming smaller and smaller. To remedy this state of things, the Department of Agriculture and Commerce established there an experiment station for the cultivation of the shell, and one Mr. Negishi, belonging to the district, one year put in, for trial, about 135 bushels of the shell in the tide-flats, and found that these had increased by the following year to 820 bushels, thus thoroughly demonstrating the practicability of the culture. From this beginning the industry increased so rapidly that by 1896 in this part of the bay alone over 700 acres[1] were under cultivation, and about 50,000 bushels of seed-shells were collected, and 112,845 bushels sold, fetching 79,329 yen. The cultivation has since extended to other parts of Ariake Bay, and promises to become more and more important.

The method of culture is very simple. The young are collected all over Ariake Bay in July and August of each year. They are then between four and five centimeters in length, and are dug out by spades and hands and then transplanted to culture-grounds, care being taken to protect them from the sun's rays during the passage.

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[1] The calculation of areas on the sea-bottom in Japan is very rough, and only approximate. As a general thing, it falls far short of the actualities.

Arrived at the culture grounds, they are scattered about, and soon find their way into the mud of the bottom, which must, therefore, be well adapted for the life of this mollusk.

These shells are left for about three years. According to the specimens given me by Mr. Fujita for examination, at the end of the first year after transplanting they are 5.6 centimeters long; at the end of the second year, 6.6 centimeters; at the end of the third year, 9 centimetres; and at the end of the fouth year, 10 centimeters. In some parts growth is no doubt more rapid.

Barnacles, "Jimegi" (*Balanus* sp.)

Further out in the same tide-flats, where the agemaki is cultivated as described in the previous section, there are planted bunches of bamboo collectors that look like the collectors for oyster-spat. Here, however, they are to collect a species that is generally considered injurious to cultural enterprises, — namely, the barnacle. The collectors are put up twice in a year, — that is, in the spring and in late August. The spring collectors begin to be taken down after sixty days, and it is thirty days more before they are all disposed of. The autumn collectors are left standing one hundred days, after which they are gradually taken away before the next March. The barnacles that are attached to the collectors are beaten off and used as manure. The annual yield is 400,000 bushels, fetching 30,000 yen. This cultivation has been going on ever since 1830 or thereabouts.

Miscellaneous

"Tairagai" (*Pinna japonica* Reeve): The cultivation of *Pinna* is confined to a small village on the Inland Sea, but it is interesting as a specimen of what can be done in the way of mollusk-cultivation. A little west of Onomichi, a large town on the north side of the Inland Sea, there is a small village called Hosojima. It has only twenty-five households, but each of these twenty-five possesses a small *Pinna* culture-ground of its own, not more than fifty by thirty feet.

Every October young *Pinna*, between seven and eight centimeters long, are collected at a shoal near the village and put rather thickly into the culture-grounds. The triangular shell, upright, with the acute apex below, is buried in the mud to the edge of the shell and placed in such a way that the hinge-line is toward the land and the open gaping side toward the sea, thus preventing the muddy water that runs down from entering the mantle-cavity of the mollusk. By October of the next year the shells have increased about two and one half times in size, although they are said to decrease in number forty per cent, and will not grow much more, even if left longer. They are then taken out, and new, young shells are put in their place.

Egg-cases of Gastropoda: The peculiar leathery egg-cases of various gastropods have a commercial value in Japan. You see them sold in the streets, dyed red, each costing about half a cent. They are bought by young girls. The cases are turned about in the mouth, and, when filled with air and then squeezed between the tongue and the roof of the mouth, emit a peculiar sound. The same use is made of the fruit of a plant (hozuki), and the mollusk egg-cases serving the purpose are called "umi-hozuki" (sea-hozuki). These toy things are in such demand that the supply cannot be left simply to the accidental finding of them, and so various methods of cultivating them have been devised in different parts of Japan. In Chiba boxes are constructed, six by three feet, and two feet high, with wooden sides, and covered with bamboo basket-work on the top and the bottom; in these large whelks (*Rapana bezoar*) are placed, and the whole left floating in the sea. The mollusks soon deposit their egg-cases on the wooden sides. In Noto pine sticks two to three feet long are anchored by a line and a weight, and are left floating in the sea for the mollusks (*Fusus inconstans*) to come and deposit their egg-cases on them. In Okayama inverted bamboo baskets are kept anchored in the same way, and serve as the repository of the eggs. There are, no doubt, other methods in other places. These egg-cases, although mere toys, must altogether be worth several tens of thousands of yen. Chiba alone produces them to the value of 30,000 yen, and Noto 10,000 yen.

"Bakagai" (*Mactra sulcatoria* Deshayes); "asari" (*Tapes philippinarum* Adam and Reeve); "shijimi" (*Corbicula atrata* Prime), and other species: These mollusks, especially the last two, are very common, and are consumed in enormous quantities, which facts have naturally led to a greater or less amount of cultivation in some places. They may be collected when young and allowed to grow in culture-grounds, or they may be allowed to grow by systems of rotary crops. Methods would seem to differ in different places.

The trepang, "namako" (*Stichopus japonicus* Selenka): In a recent paper of mine (*Notes on the Habits and Life-History of Stichopus japonicus* Selenka, *Annotations, Zoölogical Japonicæ*, vol. v., pt. 1), I offered suggestions on the method of propagation of this holothurian, after a study of its life-history. My ideas have not yet been given a fair trial, but in Mikawa Bay, where a part of them have been enforced, the complaint of the decrease of the supply, at least, seems to have ceased. I may perhaps be allowed to quote the last paragraph of the paper. "After I had thought out these measures of protection for *Stichopus japonicus* from its habits and life-history, my friend, Doctor Kishinouye, was traveling in the somewhat out-of-the-way island of Oki, and found that people there had been a hundred years or more in the habit of putting up loose stone piles in the shallow sea in order to obtain a supply of the holothurians. A village headman had thought it out from practical experiences. Verily, there is nothing new under the sun."

"*Amanori*" (*Porphyra tenera* Kjellman); "Funori" (*Gloiopeltis furcata* Post and Ruprecht): Although the present discussion is on the cultivation of animals, I cannot help alluding at the end to the cultivation of some seaweeds, as one of them, at least, is very important indeed. The "amanori," or "asakusanori," is most extensively cultivated in various parts of Japan. Of all places, however, the system has reached greatest perfection at Shinagawa and Omori, at the mouth of the Sumida River, which passes through Tokyo. In the late autumn or in the winter can be seen here miles upon miles of culture-areas in which tree-branches are set up as collectors. During the cold season the alga keeps growing on them, and any fair day one can see hundreds of little skiffs, mostly with women and young girls, going out to collect it. Being brought home, the plant is thoroughly cleansed and then made and dried in the shape of thin rectangular sheets about twenty-five by eighteen centimeters, looking very much like sheets of dark paper. In this state it can be kept for a long time, and is sold in shops. When slightly roasted, the sheets have a peculiar taste and are used much to give flavor to various articles of diet. The production about Tokyo alone is over 1,000,000 yen, and for the whole country it must, of course, be much more.

"Funori" (*Gloiopeltis*) is used as the starch-yielding source in the manufacture of various kinds of silk and cotton goods and in washing, and is one of the most important articles produced by the sea. Its cultivation is not so extensive as that of the amanori, but, according to Mr. Endo, it is undertaken to some extent in the village of Shimofuro, in the district of Shimokita, prefecture of Aomori, on the south side of the strait between Hokkaido and Honshu. At that place there is a large ledge of rock that is exposed at low tide. Here people place 700 to 800 large blocks of stone, and the alga, which grows between tide-marks, soon becomes attached to these. After five or six years, when the blocks become too old and the alga no longer grows on them, they are pushed into deeper parts, and new blocks are placed in their stead.

I think I have now given — how imperfectly, I am but too well aware — a brief survey of the marine and fresh-water animals cultivated in Japan. The subject has always been an attractive one to me, as it might in many respects be called applied embryology. Aside from its immediate economical results, there are many things in it which might be utilized to solve problems in heredity, growth, ecology, etc.

In conclusion, I wish to express my thanks to all who helped me in the preparation of this paper. Especially I would mention Doctor Kishinouye, Messrs. Fujita, Mikimoto, Nishikawa, Wada, Fujimura, and Hattori. To Mr. Uchiyama, my assistant, I am indebted for much painstaking photographic work.

SECTION H — COSMICAL PHYSICS

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(*Hall* 10, *September* 22, 10 *a. m.*)

CHAIRMAN: PROFESSOR FRANCIS E. NIPHER, Washington University.
SPEAKERS: PROFESSOR SVANTE ARRHENIUS, University of Stockholm, Stockholm.
DR. ABBOTT L. ROTCH, Blue Hill Observatory.
DR. L. A. BAUER, Washington, D. C.

THE RELATION OF METEOROLOGY TO OTHER SCIENCES

BY SVANTE AUGUST ARRHENIUS

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METEOROLOGY is concerned with the scientific investigation of the properties of the earth's atmosphere, and, consequently, is to be regarded as an application of mechanical, physical, and chemical sciences to the study of this atmosphere. An exhaustive review of the relation of meteorology to these sciences would be practically a review of the science of meteorology itself. It is obvious that the field is far too large to permit, in the short time at my disposal, of so extensive a discussion. I must, therefore, content myself in presenting to you a short review in connection with some of the most important points of contact of meteorology with the above-mentioned sciences, which are just now being industriously investigated.

The motions of the air which we designate atmospheric currents have long attracted the chief attention of meteorologists. The theoretical investigations of these motions fall naturally within the domains of mechanics, and more particularly within that of hydrodynamics. It is well recognized that this is one of the most difficult branches of mechanics. When we pass to the consideration of the atmosphere, the difficulties are notably increased; for we may by no means regard it as an approximately incompressible fluid, as we may a liquid. It is, therefore, not surprising that, in spite of the great ingenuity expended upon this problem by Ferrel, Guldberg, Mohn, and others, it has not advanced beyond the first stages of solution. Although the problem in its complete generality may never be solved, an adequate treatment of it is tolerably well assured if the practically important factors are taken into consideration, while, on the other hand, the less important aspects of the problem are neglected. In other words, the problem is reduced to the working-out of an ideal case, which, although never present in nature, will approximate to the actual case as nearly as possible.

In this connection, the working-out by Bjerknes of the so-called circulation theory for the atmospheric case has attracted much attention. He considers the density and the pressure at each point along a closed curve in a given mass of air, and derives the conditions of motion in a simple manner. Sandström, a student of Bjerknes, has computed practical cases. The successful application of the method depends very much upon choosing the closed curve in such a way that the calculation may be carried out with facility and clearness. Bjerknes and Sandström, with this end in view, choose, in preference, two perpendicular lines whose end points are connected by two isobaric lines (along which the atmospheric pressure is constant). The necessary integrations may be then easily carried out and lead to easily interpretable results. The circulation theory indicates that the influence of the earth's rotation, which in general complicates very much the theoretical treatment, may be treated in an extremely clear, simple, and elegant manner. In this connection, perhaps the greatest difficulty involved is in the consideration of the effect of friction. In consequence of its magnitude, the friction of the air with reference to the surface of the earth plays an important rôle. It is not a question here of the generally small internal friction of the air, but of the restriction of the motion through the formation of vortices, a phenomenon which, up to the present time, has been but little investigated; and probably extended empirical work will be necessary before it can be satisfactorily treated.

Bjerknes, like his predecessors in the study of the motions of the air, disregards the time-intervals in which the accelerations pertaining to these motions occur. He therefore investigates only the so-called stationary state, whereby important simplifications are introduced without disregarding the practically important cases.

The application of the circulation theory to the treatment of the motion of air in cyclones and anticyclones, as well as to atmospheric circulation in general, has already led to very interesting conclusions. It is, therefore, of the greatest interest to apply this theoretical method of treatment to the great mass of empirical data which has been furnished from observations made with the aid of balloons and kites. Such work is in prospect for the near future.

Formerly, the investigation of the atmosphere in the immediate neighborhood of the earth's surface was thought to be sufficient, but now the opinion is becoming more and more prevalent that, for a deeper insight into the nature of the properties of the atmosphere, an accurate knowledge of the higher layers of the atmosphere is also necessary. For example, in the practical carrying-out of calculations, the circulation theory presupposes a knowledge of the pressure and density of the atmosphere at various altitudes.

In the last few years, quantitative observations have led to the conclusion that the temperature of the air in the lower layers decreases, in the mean, by about 4° C. per kilometer increase in altitude. Further up, the rate of decrease becomes still greater, so that at altitudes between 5 and 10 kilometers the rate of decrease is somewhere about 8° C. per kilometer.

This is explained as due to the adiabatic expansion of the masses of air with their vertical displacement. A mass of dry air is, through expansion, cooled in rising by about 9.8° C. per kilometer. The presence of moisture in consequence of precipitation — cloud-formation — causes a decrease in this cooling effect, and the somewhat lower figures derived from observation are thus explained. The influence of this precipitation of water is particularly strong in the lower regions of the atmosphere — up to about three kilometers, where the air contains much water-vapor, which gives rise to the formation of huge clouds. This application of physics leads to the conclusion that probably in the higher, more nearly water-free air layers, the temperature sinks still more rapidly with increasing altitude. This conclusion is, however, not borne out. For, with a decrease of 8° C. per kilometer, the temperature of the air at an altitude of about 35 kilometers would sink below absolute zero. In other words, higher up no air could exist. But observations on the heights of meteorites, made with the aid of their glow, as well as upon the heights of auroras, indicate that there is an atmosphere of considerable density at a height of 100 kilometers. The decrease of temperature with altitude must, therefore, be very much smaller than previously assumed. This conclusion, founded on astronomical and physical observations, has been recently confirmed through direct temperature measurements at high altitudes by Teisserenc de Bort, and Assmann. They found that at great altitudes — somewhere about thirteen kilometers — the decrease in temperature with the height is extremely small, practically vanishing.

This cannot be otherwise explained than by the assumption that at these altitudes the vertical circulation of the air is, in comparison with other factors, too insignificant to be considered. The factors are radiation, — heat conduction is too small a factor to come here into consideration, — and the addition of heat through convection currents from warmer surroundings; the horizontal currents, so far as our observations go, become more significant the greater the altitude.

Here, again, is a field for the application of physics. This shows that the gases which are the chief constituents of the atmosphere, oxygen, nitrogen, and argon, absorb practically no heat, — oxygen shows some weak, so-called telluric lines, in the sun's spectrum. On the other hand, carbonic acid gas, and, in a still higher degree, water-vapor, which enter to some extent into the constitution of the atmosphere, possess a remarkable capacity for absorption of the non-luminous heat radiation. They thus effect a moderating influence upon the climate. This is a well-known fact, which is quite evident when the daily variation in temperature at a dry place, *e. g.*, in the desert, is compared with that at a damp place, *e. g.*, on an oceanic island. About a hundred years ago, Fourier and Pouillet showed that the air acts in a similar way to the glass of a hothouse bed. This is true for water-vapor, and carbonic acid gas, and also for a few other gases which enter to a less degree into the constitution of the atmosphere, namely, ammoniac, and the hydrocarbons. If the quantity of these gases in the air increases, the hothouse action also increases, and the temperature of the earth's surface is increased. Furthermore, the warming of the earth's surface through direct radiation from the sun is diminished, while that of the air is increased. The vertical circulation in the lowest layers of the atmosphere would, by virtue of the absorption, be decreased; on the other hand, the horizontal circulation in the higher layers would be increased, whereby the differences between the temperatures of the air at various places of the earth would decrease.

As a matter of fact, geology teaches us that, in earlier times, for the last time in the Tertiary period, the temperature of the air was not only much higher than now (in the Tertiary period about 10° C.), but also that it was much more uniformly distributed. In order to explain this, there has been previously found no more plausible ground than the assumption that there has been a change in the content of the atmosphere with respect to the heat-absorbing gases, and, in this connection, one thinks first of carbonic acid gas. Through the increase of heat, the content of the air with respect to water-vapor is greater, and the effect is increased. In a similar way, the lower temperature of the ice age, through the decrease in the heat-absorbing constituents of the atmosphere, may be explained.

Before accurate calculations can be made, there is needed an accurate spectrum analysis investigation, particularly in the ultra-red spectrum, of the gases which are important in this connection.

At any rate, the previously made calculations seem to indicate that the order of magnitude of the possible variations is about as large as the corresponding observations of the geologists.

Here, then, the sciences of meteorology, physics, likewise geology or its companion sciences, botany and zoölogy, work together. Meteorology has, of course, not only to consider the present condition of the atmosphere, but also the past, and, so far as possible, its future condition.

I have recently carried out a calculation with respect to the sun's corona which shows that the temperature of the corona, which may be regarded as locating the highest atmosphere of the sun, may be considered as due solely to radiation from the sun. Although the radiation there is incomparably greater than in the highest layers of the earth's atmosphere, it is yet probable that here the temperature of the floating dust-particles — especially those which through their negative charge serve in the explanation of the polar lights — is chiefly determined by radiation, from the sun, and from the earth. The temperature of these dust-particles on the side of the earth facing the sun lies between 40° C. and 60° C., and upon the dark side between — 30° C. and — 40° C., in temperate zones. This temperature may be regarded as approximately that of the highest layers of the atmosphere. It is, in any case, much higher than formerly supposed.

We have now penetrated to a certain extent into the domain where meteorology and the modern theory of electrons come into contact. C. T. R. Wilson showed that the negative electrons of the air serve to a greater degree than the positive electrons as condensation nuclei in the precipitation of water-vapor. A consequence of this is that generally the precipitation is negatively electrically charged, a fact recognized by Franklin, and later confirmed by Elster and Geitel. Furthermore, since the ionization of the air increases with the altitude, it is reasonable to expect that the clouds will be more strongly charged the greater the height at which they are formed. This conclusion is confirmed by experience. Clouds which are formed at low altitudes are, for the most part, only weakly electrified; and the peculiar thunder-clouds, which are more strongly charged the higher the rising air-currents upon whose upper side they are formed extend, originate at great altitudes. Such powerful air-currents occur to the best advantage over the land at the hottest time of the year, and upon this fact depends the distribution of thunder-storms with reference to the time of year. With respect to the warm air-currents over the sea, the conditions are just the reverse. Since the excess of temperature of the sea over its surroundings is greatest in winter, the fact of the maximum occurrence of oceanic thunder-storms in winter is explained.

Through rainfall, negative electricity is communicated to the earth, while, in the higher layers of the atmosphere, where the clouds originate, an excess of positive electricity remains behind. In this simple way, according to J. J. Thomson, atmospheric electricity is explained. Part of the negative charge of the earth's surface goes back into the air by conduction. This phenomenon is much more marked in summer than in winter, and hence, the reason for the smaller negative charge of the earth's surface in summer is established.

Observations on polar lights indicate that in the highest layers of the atmosphere there is again a negative charge present. To explain this, it is assumed that small globules, which are formed in the neighborhood of the sun through condensation on negative electrons, are driven away from the sun by radiation pressure and later entangled, to a certain extent, by the highest layers of the atmospheric envelope of the planets. Thus the relation, discovered by Busch, between the dust content of the highest atmospheric layers and the eleven-year sun-spot period is quite intelligible. These globules carry their negative charge with them, and consequently there originate the electrical discharges which give rise to the polar lights. In this way is explained the coincidence, found by Schwabe, of the sun-spot periods with the polar lights. Furthermore, the ions of the air, produced by the discharges, give rise to the condensation of water-vapor; and in this way the remarkable frequency, noticed by Tycho Brahe, and prominently mentioned by Ad. Paulsen, of the occurrence of higher clouds in polar light years, is explained.

We have now reached a very interesting part of our discussion, in which the facts observed by astronomers and meteorological observations stand in very close connection. Sir Norman Lockyer has treated this subject very comprehensively in a report to the International Solar Committee in Southport in 1903, and I can therefore refer you to this report.

Among the most puzzling phenomena in connection with meteorological data and known facts concerning the sun, the half-yearly variation of barometric pressure is to be mentioned. This variation shows a decided parallelism with the polar lights; so there is no doubt of the existence of a common cause for both.

The small charged particles in the highest layers of the atmosphere are carried along by air-currents, and so give rise to magnetic phenomena. Thus, the periodic daily variation of the earth's magnetic field, and the cause for this variation being much greater (about double) in years when sun-spots are prevalent, are explained. An exhaustive study of this variation would perhaps furnish us with a knowledge of the currents in the very highest layers of the atmosphere. Since this knowledge is of the greatest importance in the interpretation of meteorological phenomena (up to now we have no other means whereby to arrive at such knowledge), it is evident that meteorology may secure most important explanations from a study of the phenomena of the earth's magnetism.

The chemical properties of the constituents of the atmosphere have hitherto received at the hands of meteorologists relatively little consideration. And yet, this phase of the subject is of the very greatest importance to us. The disintegration of the earth's crust and the production of plant-life upon it stand therewith in intimate connection. Here again, carbonic acid gas and water-vapor play the principal rôle. An increase in the carbonic acid gas content of the air would promote in the highest degree the luxuriousness of plant-life. And consequently, more oxygen would be produced. Probably, as first suggested by Koene, all the oxygen of the atmosphere is a product of plant-life, which has reduced the carbonic acid gas coming from volcanoes to oxygen. The amount of coal present in the earth's crust corresponds tolerably well with the amount of oxygen in the atmosphere. In addition to the carbonic acid gas referred to above, that which is stored in the carbonates — particularly in limestone — must have been gradually removed from the earth's interior through volcanic action.

From the foregoing, we perceive how extraordinarily powerful have been the chemical processes at the boundary surface between the atmosphere and the solid crust of the earth. Moreover, the appearance of oxygen in the atmosphere, which is so vastly important in animal as well as in human life, is explained. One might have expected that this constituent of the atmosphere, so chemically active, would long ago, through disintegration processes, have been consumed. In this domain, meteorology and plant physiology work together.

The other constituents of the atmosphere, nitrogen, argon, and the numerous rare gases recently discovered by Ramsay, are remarkable by virtue of their chemical inertness. It is, therefore, not astonishing that they have remained in the atmosphere. It is much more surprising that one of these gases, namely, helium, is not more met with in the atmosphere, since it has been pointed out that many sources furnish helium to the atmosphere from the interior of the earth.

In order to explain this difficulty, Johnstone Stoney assumes that the lightest gases, hydrogen and helium, have such active molecular motions that the earth's gravitational force is not sufficiently strong to hold them to our planet. Against this view, the objection has been made that the helium would escape from the higher layers of the atmosphere, and that there, on account of the existing low temperature, the molecular motions of the gas are extremely much reduced. Without wishing to dispute that the Johnstone Stoney view has to combat the difficulty that it demands somewhat more active molecular motions for helium than is called for by the mechanical theory of gases, we must yet admit that the objection based upon the low temperature of the highest atmospheric layers is weak.

Another peculiarity concerning the distribution of gases in the atmosphere is brought out in the observations on polar lights. The principal line in the spectrum of the polar lights is that corresponding to the rare gas, crypton, which is only present in extremely small quantities in the lower regions of our atmosphere. Sir William Ramsay is therefore of the opinion that crypton must be much more generously distributed in the higher layers of the atmosphere than in the lower. Furthermore, tests of air taken at different altitudes, as well as in rising or falling air-currents, indicate that the stirring-up of the air through vertical circulation is sufficient to obliterate the difference in the constitution of the air at different altitudes which would obtain in quiet air owing to the different density of its constituents. Since crypton is heavier than air in the mean, one would expect a tendency whereby this gas would be rarer in the higher layers of the atmosphere than below. The clearing-up of this interesting question is left for thorough spectrum analysis investigations.

From this short sketch we perceive that meteorology not only stands in the closest connection with other branches of physical science as well as with hydrodynamics, but that it is also connected intimately with questions of chemical, geological, and biological character. The study of the so-called phanologistic phenomena, *i. e.*, the periodically recurring life-processes of the animal and plant world, is also of striking importance in climatological investigations. Furthermore, physical geography and meteorology have much in common.

THE PRESENT PROBLEMS OF METEOROLOGY

BY ABBOTT LAWRENCE ROTCH

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NEVER in the history of the science have so many problems presented themselves for solution as at the present time. Numerous *a priori* theories require demonstration, and, in fact, the whole structure of meteorology, which has been erected on hypotheses, needs to be strengthened or rebuilt by experimental evidence. Until recently the observations have been carried on at the very bottom of the atmosphere, and our predecessors have been compared justly to shellfish groping about the abysses of the ocean-floor to which they are confined.

Probably meteorology had its origin in a crude system of weather predictions, based on signs in the heavens, and it did not become a science until the invention of the principal meteorological instruments in the seventeenth century made possible the study of climatology by the collection of exact and comparable observations at many places on the globe. These data, owing to extensive operations of the meteorological services in the different countries, are now tolerably complete, there being comparatively small portions of the land-surface, at least, for which the climatic elements are not fairly well known, the gaps that remain to be filled lying chiefly on the Antarctic continent and in the interior of Africa.

Although it is about fifty years ago since the first observations, made synchronously over a considerable territory, were telegraphed to a central office for the purpose of forecasting the weather, it must be confessed that practically no progress has been realized in this art, for, while much has been done to complete and extend the area under observation by the creation of a finer and wider network of stations, and while the transmission of the observations and the dissemination of the forecasts based on them have been accelerated, the methods employed in formulating the forecasts are essentially those empirical rules which were adopted at the inception of the work. A recent extension of the field of observation over the ocean, by wireless telegraphy, may here be mentioned as offering advantages to certain countries; for example, the reports now being received in England from steamers in mid-Atlantic give information about the approaching weather conditions, — subject, of course, to any subsequent changes, — long before they reach the western shores of the British Isles.[1] Nevertheless, the data obtained still relate mainly to the lowest strata of the atmosphere, and we are ignorant of the conditions that prevail at the height of a mile or two, both during storms and in fine weather. Until these are known, and their sequence in the upper and lower atmosphere has been established by careful investigation, our weather forecasts, based on synoptic observations, will continue to be largely empirical. However, it should be remembered that, since weather predictions constitute the aspect of meteorology which most appeals to mankind, the incentive to improve them is the most likely to stimulate the investigations needed. Therefore it is the problems of dynamic meteorology that now press for solution, and to achieve this purpose we must not only look upward, but also elevate ourselves, or our instruments, into the higher regions.

This mode of study belongs entirely to the last half-century, for only within that period has a systematic attempt been made to ascertain the conditions prevailing in the upper air. To the credit of the United States it should be remembered that the first post of observation upon a mountain peak was one established in 1871 upon Mount Washington in New Hampshire, and this was soon followed by the highest observatory in the world, maintained during fifteen years upon the summit of Pike's Peak in Colorado.[2] The observatory upon the Puy de Dôme in France, opened in 1876, was the first mountain station in Europe to be equipped with self-recording instruments.[3] A large amount of data has been collected at these stations which illustrate chiefly the climatology of the mountainous regions, for what we obtain in this way still pertains to the earth, and, as is now admitted, does not represent the conditions prevailing at an equal height in the free air. During the present century, the organized efforts which have been made to explore the ocean of air above us have already resulted in a great increase of knowledge respecting the atmosphere as a whole. This task of ascertaining the conditions of the free air was resumed in 1888 with balloon ascents in Germany, in which special precautions were taken to obtain accurate temperatures,[4] previous observations in balloons leaving much to be desired in this respect. Four years later the French demonstrated that by means of balloons carrying only self-recording instruments, meteorological information might be acquired at heights far greater than those to which a human being can hope to ascend and live.[1] The use of the so-called *ballons-sondes*, liberated and abandoned to their fate with the expectation that when they fall to the ground the records will be recovered, was soon adopted in Germany, and has since spread all over Europe. It has been introduced into the United States by the writer, who has just dispatched the first of these registration-balloons from St. Louis, hoping in this way to obtain the temperatures at heights never before reached above the American continent.[2]

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[1] *Nature*, vol. LXX, pp. 396–397.
[2] *American Meteorological Journal*, vol. VIII, pp. 396–405.
[3] *Ibid.* vol. II, pp. 538–543.
[4] *Ibid.* vol. IX, pp. 245–251.

In 1894, at the Blue Hill Observatory, near Boston, kites were first used to lift self-recording instruments and so obtain graphical records of the various meteorological elements in the free air,[3] and this method of observation, which presents the great advantage of securing the data in the different atmospheric strata almost simultaneously and nearly vertically above the station on the ground, has been extensively employed both in this country and abroad. Heights exceeding three miles have been attained, and it is possible to ascend a mile or two on almost any day when there is wind. To render the method independent of this factor, the plan of flying kites from a steamship was introduced by the writer three years ago,[4] and this scheme, too, is now being successfully employed in Europe. The exploration of the free air by balloons and kites, it may be remarked, has given rise to the construction of special types of light and simple self-recording instruments, which are capable of recording automatically the values of temperature, moisture, and wind with a precision comparable to the eye-readings of standard instruments by a good observer.

Having examined some of the newer methods of meteorological investigation, let us now consider how they may help to solve certain problems in dynamic meteorology. It should be premised that, since the atmosphere is relatively a thin layer with respect to the globe which it covers, no portion of it can be regarded as independent of another, and, consequently, a weather-map of the whole globe, day by day, is of prime importance. Were this provided, the atmospheric changes occurring simultaneously in both hemispheres could be watched and their relation to what have been called "the great centres of action" investigated.[5] Thanks to the increasing area covered by reports from the various weather services, the unmapped surface of the globe is being diminished, so that a complete picture of the state of the atmosphere each day over the land is gradually coming into view.

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[1] *Nature*, vol. XLVIII, pp. 160–161.

[2] *Science*, N. S., vol. XXI, pp. 76–77.

[3] *Quarterly Journal of Royal Meteorological Society*, vol. XXIV, pp. 250–259.

[4] *Ibid* vol. XXVIII, pp. 1–16.

[5] *Report of International Meteorological Committee*, St. Petersburg, 1899, Appendix XI.

The mathematical application of the theory of a rotating sphere surrounded by a heated atmosphere to explain the circulation of the atmosphere as we find it, has not been satisfactory, owing to our lack of knowledge of the conditions of the upper air, as well as our ignorance concerning the physical properties of the atmosphere itself. To acquire the latter knowledge, research laboratories must be established at selected points, at both high and low levels, and as subjects of study there may be mentioned the determination of the amount of heat received from the sun and its secular variation, if any, the radiating and absorbing power of the air, the relation of pressure, density, and temperature, the chemical composition of the air, its ionization and radioactivity, and other investigations which have been proposed by Professors Abbe and McAdie[1] in their pleas for the creation of such aërophysical laboratories. The observatory now under construction by the United States Weather Bureau on a mountain in Virginia will, it is hoped, enable some of these problems to receive the attention which they deserve.[2]

The average circulation of the lower atmosphere is now well known, by reason of the monumental work of Lieutenant Maury on the winds over the oceans, and from the mass of data since collected over oceans and continents through the meteorological organizations of the various countries. While, naturally, much less is known regarding the circulation of the upper air, a great deal has been ascertained from the observations of clouds that were instituted a few years ago in various parts of the world by an international commission. In order to insure that the same cloud should everywhere be called by the same name, it was necessary to instruct the observers by publishing a cloud-atlas,[3] containing pictures and descriptions of the typical forms of clouds which experience has shown to be identical all over the globe. Then, during one year which had been agreed upon, measurements of the direction of drift and the apparent velocity of the several cloud-types were made at many stations, and measurements, by trigonometrical or other methods, of the height of these clouds above a few selected stations enabled the true velocity of the air-currents to be determined up to the altitude at which the cirrus clouds float.[4] Thus an actual survey of the direction and speed of the atmospheric circulation at different levels was effected, and a recent discussion of the results by Professor Hildebrandsson shows that the theories which have been held heretofore are untenable. Professor Hildebrandsson's conclusions in brief are that there is no exchange of air between poles and equator, the circulation over the oceans, at least, resolving itself into four

[1] *Smithsonian Miscellaneous Collections*, vol. XXXIX, no. 1077.
[2] *National Geographic Magazine*, vol. XV, pp. 442–445.
[3] *Atlas International des Nuages*, Paris, 1896.
[4] *Quarterly Journal of Royal Meteorological Society*, vol. XXX, pp. 317–322.

great whirls, the air which rises above the tropics flowing over the trades and descending probably in the extra-tropical regions, while around each pole is an independent cyclonic circulation.[1] Although this general circulation of the atmosphere appears to be indicated, many details require to be investigated. In particular, the movements of the masses of air overlying the trade-winds and doldrums, which is a region nearly barren of upper clouds, are still unknown, and the determination of these movements, as well as the temperature and humidity of the different strata, by means of kites flown from steamships, was suggested by the writer, since it would be possible in this way to penetrate even the masses of quiescent air which probably separate the trade-winds from the superposed anti-trades.[2] This suggestion has already been put in practice on the yacht of the Prince of Monaco in the neighborhood of the Azores,[3] but a more extensive campaign is necessary, which the writer himself hopes to undertake, if the funds necessary to charter and equip a steamer can be procured.

Here it will be encouraging to state some results of the efforts to ascertain the vertical thermal and hygrometric gradients in the atmospheric ocean, and to show what may be accomplished in the future. Observations on mountains, as we have seen, cannot be expected to give the conditions which exist at the corresponding heights in the free air, and hence the necessity of sending observers or self-recording instruments into this medium through the agency of balloons and kites. By the aid of an international commission, formed eight years ago under the direction of Professor Hergesell at Strassburg, much has been accomplished in Europe in this way, and something in this country through kite-flights. At the present time such atmospheric soundings are made once a month in most European countries, and at Blue Hill in the United States, with the result that a knowledge is being acquired of the vertical gradients of the meteorological elements which entirely contradicts previous conceptions. For example, it was formerly supposed that the temperature diminished with increasing altitude more and more slowly, and that at a height of about ten miles it remained invariable during winter and summer and above pole and equator. But the recent investigations of my colleagues in France and Germany show that the temperature decreases faster and faster as one rises in the air, and that not only is there a large seasonal variation at the greatest heights attained, but that non-periodic changes occur from day to day, as they do at the earth's surface.[4] Still more remarkable is the indication of a warm current at a height of about seven miles, while the stratification of the atmosphere as regards temperature, moisture, and wind has been shown by the kite-flights at Blue Hill to be a normal condition, and not merely confined to the high atmosphere, as was formerly supposed. Daily soundings of the atmosphere to the height of a mile or two are now being made with kites or captive balloons at the meteorological institutes of Berlin, Hamburg, and St. Petersburg, and are furnishing valuable data concerning the changes in the meteorological elements which occur simultaneously or successively in the overlying strata.[1]

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[1] *Quarterly Journal of Royal Meteorological Society*, vol. XXX, pp. 322–343.

[2] *Monthly Weather Review* of United States Weather Bureau, vol. XXX, pp. 181–183.

[3] *Nature*, vol. LXXI, p. 467.

[4] *Monthly Weather Review*, vol. XXX, pp. 357–359.

Of the various unsolved questions relating to this subject, perhaps the most important is whether the core of the cyclone possesses the excess of temperature over the surrounding body of air which the convectional theory of its origin requires. We need to know also the height to which the cyclone extends, the circulation around it at various levels, and further to generalize the theory of an accompanying cold-centre cyclone in the upper air, deduced by Mr. Clayton from the Blue Hill observations.[2] Other important questions which can be elucidated by future researches are the conditions favorable for precipitation and the action of dust-nuclei in producing it, the source of our American cold-waves, the exact relations of thunderstorms and tornadoes to centres of pressure and temperature, and, finally, the causes which, in the upper air, influence the trajectories and velocities of the cyclones and anti-cyclones that give us our broader weather features. When these correlations are determined from the investigations of the free air now in progress, and we possess a sufficient number of aërial stations to make it possible to chart a daily map of the upper air, then we may expect an improvement in the weather forecasts. The prediction of fog over the ocean on and adjacent to our coasts is of great practical importance to shipping, especially off the banks of Newfoundland, and the writer believes that meteorological kites flown from a steamer in these regions would reveal the unknown conditions of temperature, humidity, and wind in and above the fog-bank which might lead to the prediction of the situations favorable to its formation.

We now pass to another branch of meteorological research, namely, the cosmical relations. It is incontestable that the sun, the source of all terrestrial energy, has great influence upon the magnetic conditions of the earth, but a consideration of the relation of terrestrial magnetism and meteorology will be left to my colleague, Dr. Bauer. The cause of atmospheric electricity has always been an enigma to meteorologists, but the discovery of "ions," or "electrons," as carriers of electricity has thrown some light on this question.[1] It is of importance in geophysics to know how the capacity of the air for positive and negative electrons varies with altitude, to learn the periodic and non-periodic variation of the potential at the earth's surface and the law of dissipation of electricity.

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[1] *Report of International Meteorological Committee*, Southport, 1903. Appendix II.

[2] Blue Hill Meteorological Observatory, *Bulletin* no. I, 1900.

Attempts to regard all atmospheric phenomena as periodic and due to the influence of the sun or moon have long occupied the attention of eminent investigators, but it must be admitted that the effects of neither the periods of solar nor of lunar rotation upon the earth's meteorology can be claimed to have been proved, although a correspondence has been found by the distinguished speaker who preceded me in regard to the frequencies of auroras and thunder-storms and the position of the moon in declination.[2] To Professor Arrhenius is also due the remarkable generalization that the pressure of light emanating from the sun causes alike the streaming-away from it of comets' tails, the zodiacal light, and the aurora borealis. The relation of sun-spot frequency, which has a periodicity of about eleven years, to atmospheric changes on the earth, especially as manifested by barometric pressure, rainfall, and temperature in India, has been investigated, and the coincidences, even if nothing more, which have been shown to exist by Sir Norman Lockyer and his son are suggestive.[3] It may be pointed out that the same action of the sun might cause simultaneously increased rainfall in India and a deficiency of rainfall in England, because rising currents in one region are necessarily accompanied by descending currents elsewhere, and, therefore, no objection can be offered to a theory of cosmical influence which produces different weather conditions in different parts of the globe.

Since the sun is the source of our energy, the discovery of any variation in the heat emitted is of the deepest interest, and the important investigations of Professor Langley[4] are now to be supplemented by the broader work of a committee appointed by the National Academy of Sciences[5] and also by an international commission,[6] with the general object of combining and discussing meteorological observations from the point of view of their relation to solar phenomena. It does not seem improbable, therefore, that eventually we may have seasonal predictions of weather possessing at least the success of those now made daily, and that possibly forecasts of the weather will be hazarded several years in advance. The value of such forecasts, as affecting the crops alone, would be of inestimable benefit to mankind, and predictions already made in India for the ensuing season, while not entirely successful, have still proved advantageous. A number of short cycles in the weather have been detected, including a seven-day period in the temperature, which Mr. Clayton found could be used for forecasting were it not for an unexplained reversal in the phase of the temperature oscillation.[1]

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[1] *Terrestrial Magnetism and Atmospheric Electricity*, vol. VI, pp. 9–10.

[2] Arrhenius, *Lehrbuch der Kosmischen Physik*, pp. 791, 893.

[3] *Nature*, vol. LXIX, pp. 351–357.

[4] *Report of Secretary of Smithsonian Institution*, 1903, pp. 23, 78–84.

[5] *Science*, N. S., vol. XX, pp. 316, 930–932.

[6] *Quarterly Journal of Royal Meteorological Society*, vol. XXXI, p. 28.

The interesting question of the value of meteorological observations may appropriately conclude this address. Professor Schuster, the English physicist, has recently denounced the practice of accumulating these observations with no specific purpose.[2] To an extent this criticism is valid in all the sciences, since those observations are most useful when made by or for the person who is to utilize them, but although modern meteorology demands special series of observations to solve such problems as the temperature in cyclones and anti-cyclones, it is sometimes true that long series of observations made with one object in view may subsequently become valuable for quite another purpose. For the study of climate and its possible change long-continued observations in each country are a necessity, though these might properly be confined to selected stations from whose normals the values for other stations may be computed. Professor Schuster's wish to limit the number of observations implies that the existing series have been inadequately discussed, for the reason that it is easier to find observers than competent investigators. For this unfortunate condition the weather services of most countries are chiefly to blame, because, being burdened with the routine work of collecting climatological and synoptic data and formulating and promulgating weather forecasts, which is the public estimate of their entire duty, most Government meteorological organizations concentrate their energies and expenditures on these functions, and partially or completely neglect the researches by which alone our knowledge of the mechanics of the atmosphere can be increased. In this criticism must be included the United States Weather Bureau (exception being made in favor of Professor Bigelow's discussions), and the similar bureaus of such equally enlightened countries as France and England. However, in the latter country an attempt is now being made to create an Imperial meteorological institute which could undertake the discussion of the great mass of data accumulated in Great Britain and her colonies, especially the relations of solar phenomena to meteorology and magnetism, and it is argued that this would contribute towards the formation of a body of scientific investigators adequate to the needs of the British Empire, and be of the highest educational and scientific worth.[1] In the United States, meteorological research has always been fostered by individuals, of whom the names of Franklin, Redfield, Espy, Coffin, Maury, Loomis, and Ferrel are brilliant examples. To-day my colleague, M. Teisserenc de Bort in France, and we ourselves at Blue Hill, are endeavoring, unassisted, to solve problems in dynamic meteorology, which ought to be undertaken by the national services of our respective countries. It behooves, then, those who are desirous of advancing the status of meteorology to strive to convince the public that the function of a Government Bureau is not merely to collect meteorological data and to make inductive weather predictions based on remembrance of the sequence in similar conditions, but that the science of meteorology requires laborious researches by competent men and the generous expenditure of money before practical benefit can result from improved weather forecasts. If some of my hearers are converted to such an opinion, this address will have served a useful purpose.

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[1] *Proceedings of American Academy of Arts and Sciences*, vol. XXXIV, p. 613 *et seq.*

[2] Address at British Association, Belfast, 1902. *Nature*, vol. LXVI, pp. 617–618.

[1] Sir J. Eliot at British Association, Cambridge, 1904. *Nature*, vol. LXX, p. 406.

THE PRESENT PROBLEMS OF TERRESTRIAL MAGNETISM

BY LOUIS AGRICOLA BAUER

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In view of the expansion of the Section from "meteorology," as originally planned, to that of "cosmical physics," I was requested to give a thirty-minute address on the problems of the earth's magnetism, the two principal speakers dealing chiefly with the investigations and problems of meteorology. The time allotted will not permit, however, a presentation of the problems concerning the earth's magnetic and electric phenomena with that completeness and thoroughness the subject deserves. Suffice it, therefore, if we select such concrete examples as shall be typical of the relationship between these problems and those of the related sciences of the earth, and as shall exhibit the rôle their solutions are destined to play in the unraveling of many of the vexed questions pertaining to the physics of the earth and of the universe.

While eminent investigators, not directly engaged in magnetic work, have evinced, in one way or another, a conception of the prominence of this rôle, my humble opinion is that the full importance is not adequately realized by those concerned with the problems of the physics of the earth and of the universe. The chief reason for this is to be sought in the fact that it is just beginning to be recognized that in order to secure a steady advance in our knowledge of the magnetic and allied phenomena of the earth, the subject of the earth's magnetism must be raised to that plane of independent investigation occupied by its sister sciences, astrophysics and meteorology. It must be recognized that this subject is to be studied *per se*, and not merely as an adjunct to meteorological or geodetic work. The fact must be appreciated that to be an expert in terrestrial magnetism requires a lifetime of exclusive devotion and singleness of purpose, such as is requisite for success in any of the older, well-recognized sciences.

The magnetician must struggle to have accorded him equal privileges and recognition with the astronomer, the astrophysicist, the geologist, or the meteorologist. I am confident that the day is not far off when even he who devotes his entire time and energies to terrestrial magnetism will be obliged to specialize in this field also to secure the best results, just as the physicist, for example, nowadays must restrict himself to one definite branch of his entire subject. To illustrate, the study of the secular variation of the earth's magnetism is one sufficiently broad and extensive to occupy one's *sole* attention. Those of our eminent investigators who are only indirectly interested in this branch of terrestrial magnetism are found to deliver opinions regarding this phenomenon representing no advance upon the ideas prevailing a half-century or a century ago. And thus it happens that papers on the secular variation are even to-day being presented to learned academies involving theories previously advanced and exploded by both past and recent experience.

The first problem, therefore, is to secure that proper recognition of the study of the earth's magnetism as a subject of scientific inquiry universally conceded as essential to the best success in other sciences. A great advance in this direction must be recorded, viz., that the Carnegie Institution of Washington, in full appreciation of this first and great need of magnetic research, has recently established a department of Research in Terrestrial Magnetism on an entirely independent footing from its other established departments, its operations embracing the entire globe. Here the great problems of magnetic research, in coöperation with the leading magneticians, can be studied not as subsidiary to some other great branch of scientific inquiry, but by themselves, wholly apart from any considerations of immediate economic value.

The next great problem is to secure the necessary recognition of the fact, among those advancing theories on any of the earth's magnetic or electric phenomena, that in nearly every instance sufficient data are not at hand for crucial and decisive tests of theory. The cause of this is twofold: First, the observational data in general have not the requisite extent and proper distribution either in time or space or both; and second, the mathematical discussions or analyses to deduce the facts from such data as may be at hand are in most instances not complete or are entirely lacking, primarily because of the inadequacy of the means necessary for such discussions as these which involve much time and labor. Thus one of the great questions of the day, one of liveliest interest to the astrophysicist and to the meteorologist, as well as to the magnetician, — the subject of magnetic storms or perturbations and the connection with solar phenomena, — is one in which the investigation, both observationally and theoretically, is merely in its pioneer stage. Although good work has already been done in this direction, a number of carefully and comprehensively conducted experimental and theoretical investigations will be needed before this subject is thoroughly understood, and before any theory, however ingeniously it may be worked out, will be entitled to full credence and final adoption.

Another cogent illustration of the lack of the requisite data pertains to the distribution of the magnetic forces over the earth. Considering the earth as a whole, very little advance in our knowledge of the distribution of the earth's permanent magnetism was made during the second half of the past century. Chiefly on this account, it is found that the accuracy of the determination of the magnetic potential of the earth has in no wise been increased by the most refined and elaborate of the modern calculations. We appear to know the numerical coefficients entering into the Gaussian potential expression about as accurately for Sabine's magnetic charts (1840–45) as for Neumayer's (1885). Such an important question as whether the earth, like any other magnet, is gradually losing its magnetism or not cannot be definitely answered, because of the lack of sufficient and accurate data. Recent calculations based on all the observations at hand would apparently yield the result that the earth is losing at present annually one twenty-four hundredth part of its total magnetic moment, — a loss which if continued would reduce the intensity of the magnetization of the earth to one half its present amount in sixteen hundred and sixty years. However, the data, as stated, are not sufficient to make safe this assertion.

A case in which a large amount of valuable observational data have been collected, but of which the analysis and discussion have not as yet been made with that completeness and thoroughness the subject demands, is that of the diurnal variation. And so we might go on; suffice it to say *that it appears to be the specific task of this generation to bring together the great facts concerning the earth's magnetism and to formulate them as far as possible in such language that clear, concise, and decisive deductions of theory may be made, if not by us, then by our successors.*

After these introductory, general remarks, let us briefly turn our attention to a concrete occurrence of a magnetic phenomenon destined to play an important rôle in the physics of the earth. This is a particularly fortunate example, as it is of decided interest to several of the departments into which Physical Science has been grouped by this Congress.

On May 8, 1902, as you will recall, a great catastrophe overwhelmed and annihilated the town of St. Pierre on the Island of Martinique. The destructive agency was the products from an eruption of the neighboring volcano, Mt. Pelé. All reports agree that this eruption occurred shortly before eight A. M., St. Pierre time, and you may remember that the hands of the clock on the town hospital were found stopped, according to Heilprin, at 7 h. 52 m. A. M. No distant earthquake effects or barometric fluctuations were observed in connection with this eruption, such as were recorded resulting from the mighty eruption of Krakatoa in 1883. The Mt. Pelé eruption left, therefore, no record behind on any seismograph or barograph.

However, coincident with this eruption a *magnetic* disturbance set in simultaneously around the entire globe. On the diagram exhibited, the disturbance, as recorded by the horizontal intensity magnetographs, is shown for twenty stations encircling the entire earth, some of them situated in the southern hemisphere, the majority being in the northern hemisphere. It is noticed that shortly before eight A. M., St. Pierre time, a sudden rise in all the curves occurs, resulting in an increased intensity, on the average, of about one fifteen-hundredth part of the usual value. For about one and one half to two hours after the first impulse, the curves progress fairly smoothly, when all at once they are broken up into a system of most interesting and characteristic waves, whose corresponding features can be traced from station to station.

If now we determine the absolute time of beginning of the magnetic disturbance at each station, we shall find that the times differ from each other by quantities on the order of the error of the time determination, and that, hence, the magnetic disturbance traveled over the whole earth with such great velocity as to make the times of beginning practically the same over the whole globe. Thus, by comparing the times of beginning of the magnetic observatories closest to Mt. Pelé with the times obtained at the magnetic observatories halfway round the globe, we shall find that they agree within one minute. The mean of all the times, considering the disturbance in the three magnetic elements — declination, horizontal and vertical intensity — was 7:54.1 A.M. St. Pierre time, or practically the same as given by the town clock. Since we have no means of knowing how accurately the town clock kept local mean time, it is possible that the most accurate determination of the time of the eruption of Mt. Pelé on May 8, 1902, was afforded us by this *unique* magnetic disturbance.

I have called this disturbance *unique* for several reasons. First, it is the only case at present known in which the occurrence was so sharp and decisive as to lead several magneticians to suggest, independently of each other, a causal connection with the volcanic eruption. While it is quite possible that upon research it may be found there are other cases of similar connection, it is not likely that there will be found an instance in which the data are as complete and as accurate as in the present case. In the mightier eruption of Krakatoa, no magnetic disturbance affecting the entire earth simultaneously was noted. A discontinuous disturbance occurred at the near-by Batavia magnetic observatory, which lasted merely during the rain of volcanic ashes upon Batavia, and the observer attributed the magnetic effect to the magnetic character of the ashes. If there was any general magnetic effect referable to the eruption, it was of a totally different character from that of Mt. Pelé, for Whipple deduced a velocity for the rate of propagation of a magnetic disturbance which occurred on the day of the Krakatoa eruption of about 1000 miles an hour. At this rate, it would have taken the Mt. Pelé magnetic disturbance several hours to travel around the whole earth.

The coincidence of the magnetic disturbance with the Mt. Pelé eruption was such a striking one as to suggest, as already stated, some physical connection. And the first thought might naturally be that the displacement of masses in the earth's interior produced a redistribution of the electric currents inside the earth, which in turn gave rise to the magnetic disturbance observed on the earth's surface. We have had, namely, repeated instances in which seismic disturbances, known to have occurred, were recorded not on seismographs, but on *magnetographs*. This might occur if, for example, the mechanical displacement of masses below the surface resulted in either the formation, destruction, or redistribution of the electric currents, which in turn produced the magnetic effect. This magnetic effect would then propagate itself more rapidly to the surface of the earth than the mechanical vibration, and hence might be recorded first or even give a record when the mechanical vibrations by the time they reached the earth's surface would be too feeble to leave their trace on seismographs.

However, in the case of the magnetic disturbance before us no such simple explanation is possible. While the mathematical analysis has not yet been completed, it has progressed sufficiently far to show that the cause of the magnetic disturbance cannot be referred to any distribution of electric currents *below* the earth's surface, but that, on the other hand, the observed phenomena are better satisfied by assuming a distribution of electric currents in the regions *above* us. As is known, it is with the aid of the changes in the vertical component of the earth's magnetism that we can decide whether the forces producing the observed disturbance have their seat in the earth's interior or in the regions outside. The question now is, was the coincidence between the magnetic disturbance and the Mt. Pelé eruption a mere chance connection? If not, then the further analysis of the magnetic disturbance is going to be of the greatest interest.

The production of static electric charges by the rapid ejection of particles of steam or vapor is well known. It may thus be possible that the violent and tremendous ejection of vaporous particles from within the volcanic cone produced such a high electrification of the regions above the volcano as to have sufficiently altered the potential of the semi-permanent electrification of the upper regions to have immediately produced an inflow or outflow from outside space of electric charges so as to make the resultant effect comparable to that associated with a magnetic storm coming from without.

It will be recalled that the products ejected by the eruption were described to be principally of a vaporous or gaseous character and finely powdered ash. All reports dwell especially upon the electric flashes over the mountain during the eruptions.

If it is possible, therefore, to disturb the entire earth's magnetism by an explosion on the earth, our conceptions as to the manner of the connection of magnetic disturbances and solar eruptions have had some light shed upon them.

It is, furthermore, of interest to add that the solution of the actual cause of the rapid and complete destruction of all life in the ill-fated town of St. Pierre may find some assistance in the study of the magnetic disturbance. Thus it will be recalled that in many instances it was found that the death-dealing, scorching blast passed through the clothing without injuring it, burning the flesh beneath, however, to a crisp. This might be explained, if, for example, there were in the mountain crystals of copper sulphate. The rapid heating of this, accompanied by violent ejection, would be accompanied by enormous electric charges and the production of vaporous sulphur trioxide. The latter, violently ejected, would pass through the clothing, doing comparatively little injury to it, but as soon as the vapor entered the pores of the body, it would combine with the finely divided particles of water in the skin and form sulphuric acid, which in turn burned the flesh and quickly brought death to the afflicted. Certain other substances would have a similar action.

Had we time, we might bring forth a most interesting case of the relationship between physiographic features of a land area and irregularities in the magnetic distribution. Such an instance is shown by the recently completed magnetic survey of Louisiana.

Permit me to call your attention to the great and promising field of inquiry relating to the rôle played by the terrestrial magnetic lines of force in deflecting or dissipating such solar radiations as affect the magnetic needle and which are prevented, possibly with benevolent purposes for our welfare, from reaching the lower depths of the atmosphere. With respect to such radiations, these magnetic antennæ of the earth may perform the same function as does our atmosphere with respect to swarming meteors.

Calculations appear to show that in the regions above the earth there exists a magnetic field the exact counterpart of that of the earth itself. The precise manner in which this has been brought about is one of the great problems. The composition of this field is revealed to us by the variations in the magnetic elements during the earth's rotation and by the analysis of the earth's permanent magnetic field.

Had we time, we might speak of the association between certain magnetic and meteorological phenomena; however, this field is covered in Professor Arrhenius's address.

In conclusion, let me say then that if it be conceded that the study of the physics of the universe is primarily concerned with the unraveling of the bonds of union between the constituent bodies of the universe, and with the interchange of minute electrified particles between them, and inasmuch as it appears that magnetic and electric variations constitute the surest and most sensitive indications of these existing bonds and mutual interchanges, it behooves all those interested in the steady development of the sciences of the universe to accord to the subjects of terrestrial magnetism and terrestrial electricity the fullest possible recognition, and thus give the patient workers in these fields the stimulus and encouragement necessary for best work in any field of human inquiry.

SHORT PAPER

Professor H. H. Clayton, of Blue Hill Observatory, presented a paper to this Section on "The Circulation of the Atmosphere."

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(*Prepared by courtesy of Herbert P. Whitlock of the staff of the New York State Museum*)

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